Reaction to Other Commentaries

Edward E. Roskam & Jules Ellis
University of Nijmegen

The main issue should be whether or not Guttman's arguments concerning the relevance or irrelevance of factor analysis for the study of group differences are valid. Schönemann adds a further argument to Guttman's, and ends his commentary by simply declaring Jensen's conclusion another expression of the *black inferiority myth*. Gustafsson presents his view against Guttman's, and adds an example to demonstrate the usefulness of factor analysis. Loehlin analyses Guttman's arguments and finds them fascinating, but argues that they are only partly relevant to the issue. Loehlin's position appears to be similar to our own. Jensen, in his commentary, reiterates his own arguments, claiming (correctly as we believe), that Guttman has misunderstood or misinterpreted his claims, but he fails to analyze the relevance of Guttman's arguments for the issue of studying group differences.

We should sharpen our own conclusion concerning the "first law" and the "missing theorem": All items from a universe of content have non-negative covariances in every (sub)population if and only if one latent trait g is involved. In addition, the item-trait regressions are all linear (i.e., the items are congeneric) if and only if the correlations among every (sub)set of items satisfy the tetrad condition in every (sub)population, and if and only if normalized group differences are proportional to the g-loadings of the items (for all groups). Moreover, if Spearman's (1927) two-factor theory is true, that is, each item (or test, resp.) depends on g and on a test-specific factor, and if there are group differences not limited to one factor (either g or a specific factor), it is not likely that the correlation matrix of any (sub)population will satisfy the tetrad condition, and so the missing theorem does not apply. The best that can be expected is that a major principal factor is present which closely resembles g.

Loehlin proposes to rephrase Jensen's (1985) task as one of determining whether in fact the identical factor structure holds in the combined population as in the subpopulation. We have argued that this need not be the case even if the two factor theory (g and specific factors) is true. The factor loadings should be invariant across groups, but the factor (co-)variances will in general be different. The assumption that g and specific factors are always uncorrelated can be violated by group differences. One might think of *the* population where

g and specific factors are uncorrelated, but there appears no simple way of defining *the* population.

Both Guttman's "missing theorem" and Schönemann's theorem state that if certain conditions are met, group differences on tests are perfectly correlated with the one and only common factor loading, or with the first principal component of the within-groups covariances matrix, respectively. Empirically no such perfect correlations have been reported. There can be but one conclusion, namely that the assumptions of these theorems do not hold empirically. For Guttman's theorem, it is obviously the assumption that no specific factors are involved, which is at fault. For Schönemann's theorem, either the assumption of a multivariate normal distribution, or the assumption of equal within-group covariance matrices may be at fault. But neither refutes *g*, nor group differences strongly related to *g*.

One should, of course, in delicate and sensitive matters like the black-white differences on common intelligence tests, be particularly alert for possible artifacts in the data analysis, but neither Guttman nor Schönemann have demonstrated that Jensen's (1985) results are artifacts, or are based on faulty reasoning. Moreover, Jensen was careful enough not to overinterpret his findings. Alternative explanations of black-white differences on common intelligence tests are conceivable and have been proposed, but as long as those have not been substantiated by equally thorough analyses as are demanded from those who support Jensen's view, there is no compelling *scientific* reason to abandon the hypothesis that black-white differences largely reflect *g*, that is, the major principal factor, even if it is only approximately ascertained. Some find it reasonable to assume that this *g* reflects *mental speed,* and there exist data which make this assumption at least plausible. Here, too, as long as no equally reasonable alternative is proposed and tested, it seems unscientific to reject that hypothesis apriori. Of course, to the best of our knowledge, little is known about what might cause differences in *g* or mental speed, and even less is known about its relation to *race* (the concept of *race* is itself elusive and has little scientific merit), as pointed out by Gustafsson. Even if it would be true that the dominant principal factor of intelligence in one group is not exactly the same as in the other group, we are left with the conclusion that those groups differ on each or both of these factors.

Jensen, in his commentary, is mistaken in claiming that group differences are partialled out from the correlation matrices because the means are taken out. Taking out means implies that the factor score space is centered differently in different groups, and this is reflected in the correlations and in the orthogonal factor loadings, as has long been known. Although nothing concerning means can be inferred from a single correlation matrix, the differences between correlation matrices in different groups do reflect group mean differences. We

believe that these effects can only be properly taken into account by simultaneously estimating a single factor loading matrix and estimating factor score means and (co)variances in each group. Gustafsson, referring to Sörbom (1974) appears to take a similar position. LISREL (Jöreskog & Sörbom, 1988) might be capable of doing this, but the present authors have only superficially looked into the issue of the identifiability of this structure, which may be more intricate than it appears. If it would turn out not to be identifiable, then there is no solution to the question how group differences are related to g, at least not within the context of the general linear model.

Notably, there is no simple solution to the question of which (sub)population(s) should be used to identify the g loadings of tests. Assuming, for the sake of argument, Spearman's (1927) two-factor theory, and acknowledging that it is unlikely that any group correlation matrix will satisfy the tetrad condition if there exists (sub)group differences on more than one factor (g or s), we have a problem with too many unknowns. Only if we happen to find a group which satisfies the tetrad condition, we might take that group to define g loadings by the only common factor in that group. The problem gets more complicated if we allow *minor* common factors besides g. In that case, no single group can present itself as the one where these factors are uncorrelated, unless we happen to find a group where a small number of orthogonal factors fits the correlation matrix substantially better than in any other group. To pull this a bit to the extreme: the loadings found for a group of predominantly white people should not be taken as the true factor loadings of the tests. Yet, this seems to be what is done in practice, and it may bias any result based on such analysis.

Some points mentioned in Jensen's and in Schönemann's commentary made us doubt how useful Equation 3 in our own commentary is. This equation is, in fact, the regression equation corresponding to the so-called Spearman correlation (Jensen, 1985), and that correlation is affected by the range and average size of the loadings. In particular, α and β in $d_j = \alpha_j d_g + \beta_j d_{sj}$ are not independent if the test reliabilities are not too different. If indeed a large proportion of test score variances in the combined population is explained by the first principal factor, it is apriori to be expected that group differences will also be largely determined by that first principal factor, even if d_g is small and d_{sj} is not too large. This does not justify Guttman's implication that the Spearman correlation is a tautology, but it casts doubt on its usefulness to express the relation between group differences and g. Remarks made by Loehlin (and indirectly by Schönemann) make us wonder whether analysis of the between-groups covariance matrix might be a more direct method to find the most discriminant dimension, and to see if group differences reflect the same dominant dimension as obtained from the within-group covariances.

Both Jensen and Gustafsson have emphasized the usefulness of factor analysis for studying group differences, and presented demonstrations of that. However, a method can never be justified only by demonstrating that it yields stable, and plausible or apparently *good* results. Notably, even if we assume that there exist only *g* and test-specific factors psychologically independent of *g*, but there are (sub)group differences with respect to these factors, these factors will emerge as common factors with loadings and (co)variance which are hard to identify uniquely. Factor analysis, in our opinion, is a poor method since the factor covariance matrix must be arbitrarily fixed in one group, and moreover, its basic assumption of linear regression is likely to be too crude to be of more than heuristic value. More detailed and possibly experimental rather than correlational analyses of cognitive functioning are needed to unravel group differences.

In summary, we find that tests as performed by Jensen are not tautological or redundant, and that Guttman's "missing theorem" does not apply because its ignores specific factors, and because the tetrad condition in every (sub)population is not a necessary implication of two-factor theory. Such tests are even more relevant if the concept of *g* is given a broader interpretation and denotes the dominant principal factor in any subdomain of intelligence items.

References

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