Final comments to the committee as of June 6th. 2006:
By Helmuth Nyborg

Re 1.1.1/1: I only recently realised that a typo might constitute a possible explanation.

Re 1.1.1/2: The drop-out problem. In my 2005 publication the reader is, in fact, informed that subjects are drawn from a total study – part of them recruited from cross-sectional and the other part coming from a longitudinal streak of repeatedly tested subjects. This neither raised questions from the reviewers nor from any professional readers, even if they obviously all are well aware of the common drop-out problem in longitudinal analyses. Nobody, but the committee, find my mentioning the longitudinal recruitment of sufficient relevance to raise questions about it in the present context. Moreover, as said before the average drop-out was 50,7% - a level accepted for most longitudinal studies.

Re 1.1.2: The committee reference to point 5 in *Vejledende Retningslinjer for Forskningsetik i Samfundsvidenskaberne* from the Danish Social Science Research Council ought really to be re-considered in light of the fact that Rector Niels Chr. Sidenius, in connection with the present case, actually stated that a researcher must be given sufficient time to finish the final report. Moreover, Rector Sidenius also decided that no further action was to be taken unless a final report could not be published in a professional peer-reviewed journal.

Re 1.1.2/1: The journal's allocated maximum space of 5.000 words, including tables and figures, obviously does not permit a presentation in accord what the committee apparently sees as a minimum report on methodology (see also 1.1.3/3 below) and other aspects of the present research. Given the maximum, each researcher is forced to choose among what he sees as the most pertinent points, obviously risking that he in the peer-review process or later may be called to answer questions about why he chose as he did (see next point).

Re 1.1.3/3: Way back in 1994 Jensen and Weng raised the question of how invariant *g* is across various methods of factor analysis. They used six different factor analytic methods on four simulated data matrices where the factors were exactly known. They also used nine different factor analytic methods on a real correlation matrix with twenty-four tests taken by 145 grade 7 and 8 students. The average congruence coefficients between the true *g* factor and the *g* factors derived from the various methods amounted to +.998 (range +.997 - +.999). This applied even if some of the artificial matrices were deliberately designed to "trick" deviating estimates. For the real data, the forty-five congruence coefficients between the ten *g* vectors ranged from +.991 to 1,000 (average +.995). They concluded that all the different methods of factor analysis estimated the true *g* so closely that **there was hardly any basis for choosing between them** (my emphasis).

Moreover, Ree and Earles (1991) factor analyzed data for 9,173 recruits taking the Armed Services Vocational Aptitude Battery (ASVAB) with 14 different methods and derived 14 different *g* factor scores for each recruit. The many different methods resulted in very little variation among the obtained *g* factors (average correlation +.984).

Jensen (1998, p. 83) was able to conclude that "… whatever variation exists among the myriad estimates of *g* that have been reported since the beginning of factor analysis, **exceedingly little of it can be attributed to differences in the methods of factor analysis employed**." (my emphasis).

It was on this empirical basis I decided that providing intimate details about the specific factor analytic approach I used would waste valuable space that was better devoted to more pertinent questions. The reviewers agreed with my decision. I accordingly find that the committee's insistence on a fuller report of all factor analytic details fails to take into account the empirical evidence in the field.

1.1.3/4: It is true that one cannot tell for sure whether an observed difference is due to the secondary or to the primary factors. However, it has been demonstrated empirically that the hierarchical approach used minimizes the contamination of the *g* factor by the primary factors, and **that was all I claimed!** Moreover, many specialists in the field accept this view.

1.2.1/1: It is true that data collection for the children has stopped – but it has not stopped for adult subjects. Moreover, there are still raw data for children that are not registered electronically and analysed, and this update procedure will continue despite the committee's claim to the opposite.

Let me add some general comments to the committee work.

The identifiability problem is a controversial and highly technical question of relevance for a broad professional audience. It is not a question that a single psychologist with no degree in statistics or mathematics can be expected to deal with in a qualified manner. I therefore find it more appropriate that the committee published their analysis of the problem in the "Personality and Individual Differences" journal, where it would receive the qualified treatment it deserves. I find it entirely unacceptable that the committee uses its highly specialised insights in complex mathematics and statistics against a psychologist with no formal qualification in these areas in a closed university hearing where his position as professor in psychology is explicitly put at stake!

The point previously argued by the committee than even a social scientist should know intimate details of highly technical statistical matters raises another questions. It is common practice that psychologists' pay people with statistical expertise to help them with problems they are not formally qualified to deal with. Does the committee find that a psychologist should only publish such studies after the psychologists himself has obtained sufficient knowledge to act formally as an able statistician? I would say that this would be to raise standards that most psychologists would not be able to honour in practice being specialised in other areas. Moreover, this extreme claim would most certainly eliminate much cross-disciplinary work that is based on common trust in other peoples' specialisation. Or does the committee find that hired statistical expertise should in general be mistrusted to the point of no consulting (and no publishing) because the experts might perhaps overlook traps or misinform the social scientist who is paying for their services in the attempt to avoid such traps? It is always easy to raise exceptionally high statistical or mathematical standards for the behavioural science (also standards higher than professional reviewers might set). However, in my opinion they should never be set so high as to exclude most social and behavioural science or to work against a factor analytic model that has been recommended by leading authorities and – more importantly - has been demonstrated to work well in practice through predictive validity.

References

Jensen, A.R. (1998) *The g factor: The Science of Mental Ability*. Westport, CT.: Praeger.

Jensen, A.R., & Weng, L-J. (1994) What is a good *g*? *Intelligence, 18*, 231-258.

Ree, M.J., & Earles, J.A. (1991) The stability of convergent estimates of *g*. *Intelligence, 15*, 271-278.