

## Questions to the Committee

The case has two aspects – a professional side and the “other” side, and I should like to ask questions to both.

### The professional side

In 2001 and 2002 I discussed various methodological aspects of the observation that sex loads .272 on general intelligence  $g$  in adults at the annual meetings of the International Society for Intelligence Research.

In 2003 I published the result in chapter 10, Sex differences in  $g$ , in H. Nyborg (ed.) *The Scientific Study of General Intelligence: Tribute to Arthur R. Jensen*, Oxford: Pergamon, in addition to discussing methodological problems and the results of others.

In 2005 I finally published a slightly modified version of the finding in H. Nyborg, *Sex-related differences in general intelligence  $g$ , brain size, and social status* in the respected peer-reviewed journal *Personality and Individual Differences*, 39, 497-509.

The number of adult subjects with full data went up from 52 to 62 over the period in the ongoing project, and after correction for a previous minor error in calculation, sex loaded in the final paper .274 on  $g$  - a hardly noticeable change from the original ISIR2001 finding of .272. The nature and consequences of the error in calculation was spelled out at the public home page of the 2003 book (Elsevier).

The hierarchical factor-analytic method was used throughout the studies and the test for sex differences followed the recommendations by Jensen (1998, p. 538), i.e. to examine whether sex loads significantly on  $g$ . The result did not deviate from what other researchers have found, i.e. a 3-5 IQ point sex difference, but it was noted that some researcher do not find this difference. The paper discussed the methodology and the likely reasons for discrepancies.

**Question 1:** *Do you find that this approach deviates in any way from common practice in psychometrics?*

### The “other” side

Admittedly, the study of a sex difference in  $g$  is controversial, and this has had a devastating impact on my research. I have thus been accused 1) of premature publication (Jens Mammen; JM), 2) of withholding data and flatly refusing to provide empirical documentation and to cooperate (Pia Ankersen; PA), 3) of, in a publication, to refer to a congress paper that was not published or accessible on demand (Praksisudvalget), 4) of deliberately “fabricating” a sex difference by using a questionable method (Pia Ankersen, Jørgen Poulsen and Siggie Kristoffersen; APK), and finally 5) of treating data in questionable ways (Jens Mammen; JM).

#### **Re 1. Premature publication**

The “case” began when I turned down an interview (November 2001) by journalist Mona Samir Sørensen from “Politiken”. The reason for this was that before going public, I wanted to discuss my observation of a moderate sex difference in  $g$  at the forthcoming meeting of the International Society for Intelligence Research in December 2001 in USA. The journalist phoned me again in early January 2002, and insisted upon having the interview. It left me with the impression that she did not understand the complex methodology and interpretation of results. I therefore demanded to see a draft of the interview before publication, to which she agreed. Neither the first faulty draft nor the second accepted version stated anywhere a 27% sex difference. I also sent her tables and figures with details of the study. Neither did they mention a 27% sex difference. A few days later her newspaper “Politiken” nevertheless presented this sensational figure on the front page. The journalist later “excused” her error in an email to me.

A veritable media storm ensued. During the following weeks and months the University and my home was literally besieged by inquiries, demands for my resignation, attacks of varying quality from colleagues, organizations, and angry lay people, not to speak of personal phone and email threats spanning from the ridiculous to the vicious to the really scary.

In this heated situation the director of my Institute of Psychology at Aarhus University, professor Jens Mammen (JM) wrote me a letter (18<sup>th</sup> January 2002) stating that I had presented striking views ("markante synspunkter") on differences in male and female intelligence in the media. He further said that my "claims" deviated from what he had seen documented scientifically ("...dine påstande afveg fra, hvad jeg ellers har set dokumenteret videnskabeligt, ..."). It further "surprised" him that I went public with an "alleged" finding that was ascribed far-reaching consequences, without first having published it in a way that made possible the critical scrutiny of its basis by colleagues. His overall conclusion was that I was responsible for providing the public with striking and far-reaching claims that were not based on publicly accessible and testable material and which had not been subjected to the potential critique by colleagues and by the public ("Min foreløbige konklusion på det ovenstående er, at du fremstår som ansvarlig for til offentligheden at have formidlet markante og konsekvensrige påstande på et grundlag, som for nærværende ikke bygger på offentlig tilgængeligt og kontrollerbart materiale, og som ikke har kunnet underkastes kollegers og offentlighedens mulige kritik"). The letter ends by demanding that I released the data material or a press release.

JM then informed the public (through the newspaper ("Politiken") that he looked upon the case with gravity ("ser på sagen med alvor"). He further told the public that he found it unusual that a researcher goes public with investigations before there at least is a so-called "Paper" so that other researchers can examine the results ("Ifølge Jens Mammen er det usædvanligt, at forskere offentligt fortæller om undersøgelser, før der i det mindste foreligger et såkaldt "paper", hvor andre forskere kan se resultaterne efter i sømmene."). In the very same article the journalist (Nils Thorsen) indirectly admitted that it was his own newspaper that had made up the 27% male advantage, and I stressed (here as in countless situations to come) that what I found really was a moderate - actually a modest - average sex difference that was just statistically significant and that this average difference had no bearing whatsoever on individual women and men.

JM further underlined the gravity of the case by sending to me (January 18<sup>th</sup>, 2002) a letter accompanied by a printout of an account provided by High Court judge Hans Henrik Brydenholdt, Chairman of The Danish Committees for Scientific Dishonesty (DCSD). This account outlines the legal aspects of premature publication - also in non-scientific news media. JM stressed that this was relevant for the case at hand ("Problemstillingen"). He finally informed me that he had provided journalist Nils Thorsen from "Politiken" with a link to the account by the High Court judge ([www.forsk.dk/uvvu/publ/uvvu2000/kap\\_1.htm](http://www.forsk.dk/uvvu/publ/uvvu2000/kap_1.htm)).

In an email of 19<sup>th</sup> January 2002 Mammen further called my attention to guidelines for ethics in research and recommend that I consulted: [www.forsk.dk/ssf/publ/informat/ssf-etik.html](http://www.forsk.dk/ssf/publ/informat/ssf-etik.html). In particular, he referred me to section 5, which states, among other things, that the researcher ought to give an account of method, material and analyses, so that critics can evaluate the work.

JM finally found - based on reading my emails - that I had not conformed to these guidelines with respect to my appearances in the media, and that this created problems for myself and for the Institute ("Ud fra, hvad du selv har fortalt mig i dine mails, tyder alt på, at du ved din optræden i medierne ikke har overholdt ovenstående retningslinier, hvilket for mig at se stiller både dig og instituttet i en vanskelig situation").

**Question 2:** *Do you find that my observation of a moderate male advantage in intelligence deviates from what has previously been documented scientifically?*

**Question 3:** *Do you find that I had provided the public with striking and far-reaching claims that were not based on testable material and which had not been subjected to the potential critique by colleagues?*

**Question 4:** *Do you find it understandable that a researcher refuses to hand over conference notes on a moderate (but still highly controversial) sex difference without being accompanied by a detailed discussion of the finding – in particular after the notes were deliberately blown totally out of proportions?*

**Question 5:** *Do you find it unusual that a researcher wishes to acknowledge in a publication that the results have been discussed previously at a professional meeting.*

#### **Re 4. The deliberate fabrication of a sex difference**

Pia Ankersen, Jørgen Poulsen and Siggie Kristoffersen (APK; all from Political Science at the Faculty of Social Sciences, Aarhus University) accused me on October 9<sup>th</sup>, 2003 in a highly critical newspaper for deliberately having fabricated a sex difference that was not there. My fraud was said to consist of inserting points-biserial correlations in the factor matrix and then performing a hierarchical factor analysis to see how much sex loaded on g. The authors did not inform me about this damaging article before publication, but it certainly renewed the media storm.

This article is, in fact, one of the two pillars upon which the University of Aarhus is building the case against me (the other being PA's complaint over my lack of cooperation and unwillingness to publish data sheets). Thus, in a response to Science Magazine writer Constance Holden (30. December 2005) the Dean of Social Sciences, Tom Latrup-Pedersen, confirms that the University of Aarhus uses this newspaper article as a primary platform against me. This being the case, I have had the article translated into English (enclosure 3) and would like to address the main point of the critique of the method in the following way.

In a personal communication dated 08-10-2003 to Peter Hartman (then my Ph.d. student), Professor Arthur R. Jensen from Berkeley University, California, USA, writes:

*“ ... the point the critics are making is very niggling. If you put the sex point biserial r into a factor analysis, it will inevitably affect the factor(s) on which it loads, but if the number of tests is reasonable for a factor analysis (e.g., 10 or more), the effect of any one variable on the loadings of all the others becomes negligibly small. One can check this by looking at the factor loadings when the variable in question (sex) is omitted and when it is included in the analysis. (See p. 542, Note 9 in my book "The g Factor." I've used the Extension Analysis for a single variable added to the factor analysis and it makes so little difference as to be not worth the bother, and can't possibly change any conclusion that would be drawn from the analysis. The extension analysis, however, is required if several extraneous variables are added to the factor analysis.*

*Good luck to Helmuth. If one sticks one's neck out on a controversial subject, one has to expect the consequences, one of which is a microscopic examination of your research and often niggling and nit-picking criticism. I've been through this for almost 40 years. Shockley used to say that one shouldn't stick one's neck out on eugenics unless you have a Nobel Prize and at least a million dollars, which gives one a good deal of academic and financial security.*

*Best wishes, Art Jensen”*

Let me add that 1) I have obtained permission from Hartmann to quote this personal communication, 2) Shockley refers to William Shockley, who got the Nobel price for co-inventing the transistor, and 3) I operate with 19 or 20 rather than only with 10 tests.

Finally, let me quote the following empirical argument from Jensen (1998, page 541, Note 9):

*“Including the sex  $r_{pbs}$  for each of the subtests in the correlation matrix to be factor analyzed had no effect on the factor structure and only a negligible effect on the subtests' g loadings (congru-*

ence coefficients for all batteries are .999) when the factor analyses that include  $r_{pbs}$  in the correlation matrix were compared with the analyses that excluded  $r_{pbs}$  from the matrix. Therefore, it was not necessary to perform a Dwyer (1937) extension analysis (a mathematical maneuver that would be used in this case to isolate the sex variable itself from influencing the psychometric factors while showing its loadings on the psychometric factors).”

#### **Ad 5. Careless treatment of data**

It is important to note that the data I use for analysis and conclusion in the final published 2005 journal article mentioned above was drawn exclusively from the late phases 7 and 8 in the school project. The point of mentioning this to the committee is that most other aspects of the huge School project have little relevance for understanding how the adult sex difference was established. This means that the remaining 1.000+ variables, hundreds of children, and facts about how old they were when examined, have little relevance in the present context. Some of these data have admittedly been used in preliminary explorative analyses, as much of my work concentrated on evaluating various methodological approaches to the study of sex differences in *g*. However, they had no relevance whatsoever for the conclusions made in the final report, as they rely exclusively on phase 7 and 8 data. It was therefore under protest that I was forced by JM to publish these preliminary and exploratory calculations on my private home page.

With respect to the accusation of “changing” numbers, it is important to understand that the huge school project consists of several analytic layers: Raw data protocol sheets, a large number of SPSS programs for transforming the raw data into test dimensions, and finally Statistica spreadsheets for the final factor analyses. New raw data are collected continually in this ongoing project, the SPSS programs are run, and the Statistica spreadsheets are updated on a continuous basis. This implies that the total number of subjects as well as the number of members within the various age categories will necessarily change with time. Moreover, subjects who miss only a few data points are called in for further testing on a continuous basis. This explains the “changing” numbers JM worries about.

Finally, I am the first to regret that the updating of the files with the few missing data points from 52 to 62 subjects led to the problem that the 52 original dataset cannot not be re-established, because the Statistica program automatically updates the date for the original file creation when it is saved again. However, sex loaded on *g* in the original adult study of 52 subjects and in the final sample of 62 adults with a difference that showed up only at the third decimal level, and in both cases the objective selection criteria were full data sets and at least one WAIS sub-scores that differed from zero (see enclosure 8: The data disk sent to the committee).

**Question 6:** *Do you find that phase 0-6 data are relevant for understanding the moderate sex difference seen in the phase 7 and 8 data?*

**Question 7:** *Do you find that “changing numbers” is a problem when the adult sex load on *g* was first reported in 2001 to be .272 and in 2005 to be .274?*

#### **A general question:**

**Question 8:** *Do you agree that critical discussions of methods, data, and conclusions are best dealt with in open academic communities by independent international experts.*

#### **References**



DANSK MAGISTERFORENING

Ankersen, P., Poulsen, J.J., & Kristoffersen, S.B. (2003). Münchhausen i Århus. Morgenavisen Jyllandsposten, Kronik 9. oktober; English translation.

Ankersen, P., Poulsen, J.J., & Kristoffersen, S.B. (2003). Münchhausen i Århus. Morgenavisen Jyllandsposten, Teknisk Appendiks, 9. oktober; English translation.

Gottfredson, L. (1994), Egalitarian fiction and collective fraud. *Society*, 31 (3) Whole No. 209.

Nyborg, H. (2003), The Sociology of Psychometric and Bio-behavioral Sciences: A Case Study of Destructive Social Reductionism and Collective Fraud in 20th Century Academia. Chapter 20 in H. Nyborg (ed), *The Scientific Study of General Intelligence: Tribute to Arthur R. Jensen*. Amsterdam: Pergamon/Elsevier Science, pp 441-502).

Nyborg, H. (2005) Sex-related differences in general intelligence *g*, brain size, and social status. *Personality and Individual Differences*, 39, 497-509.