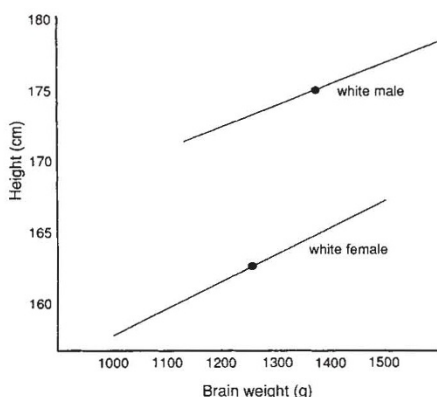


Brain size differences

SIR — Recent claims by Rushton and Ankney of racial and sex differences in brain size¹⁻³ are accounted for by a simple artefact of the statistical method employed. I illustrate this effect below.

I focus on the claim that men have larger brains than women, because this difference was the most dramatic of all comparisons made¹. I use the autopsy measurement of brain weight and body dimensions of Cleveland adults presented by Ho *et al.*^{4,5}. These same data were recently used by Ankney⁶ to bolster the conclusion that men have larger brains than women, once body size differences between men and women are accounted for. This conclusion is currently receiving a great deal of attention in the Canadian press⁷. Ho *et al.*^{4,5} did not present the raw data, but all necessary quantities can be computed from their tabular summaries. Using linear regression⁸ of brain weight on body size, men and women of the same body size are compared. The men have larger brains on average. For example, mean brain weight of white men 170 cm tall is about 100 g greater than of white women of the same height^{1,6}. A similar result holds when body weight is used instead⁶.

Do men have larger brains? The flaw in such a claim is forcefully illustrated by a further analysis of these same data. Rather than compare brain sizes between men and women of similar height, I compared mean heights of men and women having the same brain size. If men truly have larger brains for their body size than women, then men should be shorter than women of equal brain weight. Yet the opposite is true: white men are more than 10 cm taller on average than white women with the same brain weight (see figure). Indeed,



Linear regressions of body height on brain weight in white men and women. Regression statistics were computed from data in Ho *et al.*^{4,5} For men, $y = 0.015Y + 154.4$ ($n = 414$). for women, $y = 0.19Y + 138.7$ ($n = 388$). Lines extend two standard deviations to either side of mean brain weight, indicated by the large points.

to find a male brain-size category in which mean height is equal to that of women with 1,500 g brains, it is necessary to extrapolate downward to a male brain of about 850 g. Clearly, by this criterion women have much larger brains than men. The same is true when body weight rather than height is used. Furthermore, racial differences disappear when the data are analysed in the same way.

The source of the conflicting conclusions is the well-known 'regression effect'⁸. This paradox occurs because brain size of individuals varies partly independently of body size, and vice versa. The effect is enhanced in the present case because the causes of size variation between individuals of the same sex (which may be chiefly environmental) are not the same as the causes of differences between the sexes (which may be largely genetic). Hence it makes little sense to use natural variation in brain and body size within sexes to correct for differences between men and women.

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SIR — In your comment on Rushton's recent data showing different mean cranial capacities among Asian, Caucasian and Afro-American US Army personnel¹, you suggest that Caucasian military personnel may be of relatively higher quality, compared to the Caucasian population as a whole, than Afro-American, thus producing a biased estimate of the difference in brain size.

This is surely improbable. The US Army administers aptitude tests to potential recruits and accepts only those who score above a minimum threshold. Afro-Americans score lower on aptitude tests than Caucasians, so a greater proportion of Afro-Americans in the lower ability range are screened out by the tests. The admission tests appear to screen out 3.4 per cent of Caucasians and 30 per cent of Afro-Americans⁹. This procedure raises the mean ability levels of both Caucasians and Afro-Americans in the US Army, as compared with the general population, but it raises the mean ability of Afro-American military personnel considerably more than that of Caucasians. The effect is to raise the mean IQ of Caucasians in the US Army by 1 IQ point (100 to 101) and of Afro-Americans by 6 IQ points (85 to 91).

The effect of military selection tests in reducing the black-white difference in the general population goes some way

towards explaining why the black-white differences for cranial capacity reported by Rushton are much lower than those obtained by Beals, Smith and Dodd¹⁰. Their study of worldwide data on approximately 20,000 crania found that Caucasians have a mean cranial capacity of 1,362 cm³, indistinguishable from Rushton's figure of 1,361 cm³ for American Caucasian military personnel. But the Beals *et al.* figure for the cranial capacity of Africans is 1,276 cm³, considerably lower than Rushton's figure of 1,346 cm³ for Afro-Americans in the US Army. The most probable explanation of these differences is that the selection procedures for the US Army exert only minimum bias on the intake of whites but screen out large numbers of low-ability blacks. Thus you are correct in suggesting that there is a bias in Rushton's military sample, but in the opposite direction from that suggested in your comment.

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SIR — Science does not exist in a vacuum and is therefore subject to all the political, social or economic influences that exist in this world. Political correctness has its place, whether scientists like it or not. Everything relating to science, that is, the method, the data, the interpretations and results, are all as subjective as any other human endeavour (simply because it is a human endeavour). Thus, science and researchers are not immune to attacks from a nonscientific viewpoint such as the politically correct.

The conclusions drawn by Rushton² and Ankney³ in the brain size/IQ debate are dubious at best. Mary Warnock, commenting on John Stuart Mill's essay "The Subjection of Women", writes of Mills: "He was not prepared to accept any argument purporting to show that women were *naturally* inferior in intellect or originality to men. For, he said, there had never been a chance to test such a hypothesis. Only after generations of equal education could any proposition about the powers of women compared with those of men be considered."¹¹ In other words, neither Rushton nor Ankney considered the political, social and economic differences between the sexes. If men and women were educated in an environment where both sexes were equally encouraged to achieve, given equal opportunity, given equal rights and there existed no discrimination of any kind, both sexes would no doubt excel equally in all attributes to mental abilities. If the brain size data are true, this means that women's brains

work more efficiently than men's. Also, isn't it possible that the difference in brain size (if any) is the result of generations and generations of oppression of women by men? Has either Rushton or Ankney considered other reasons (or all the other possible reasons for that matter) for brain size difference that may be just as statistically significant as that from gender alone? What every experiment needs is a control. For the brain size/IQ debate, this appears impossible and thus Rushton's and Ankney's conclusions are without merit.

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SIR — Professor J. Phillippe Rushton offers an analysis of the cranial capacities of the several ethnic groups recruited by the US Army, with an additional comparison between those of men and women. John Maddox¹ has been rightly sarcastic about the biases implicit in Rushton's study: if ethnic differences exist, are they based upon neurological factors that have never been examined? How tightly are neurons packed within brains? Are differences of myelination involved? In the context of intelligence (whatever that may mean), how do the relevant neural networks function, anyway? For serious neuroscientists, much is uncertain. On a sociological front, how are men of diverse ethnic origin recruited by armies in the first place? What personal needs drive them?

Rushton's enormous study seems to have been based on anthropometric data gathered to help contractors to the US Army to provide a range of helmets. Should we take it seriously? Surely not.

Were it possible to accept Rushton's efforts as methodologically sound, we could leave him alone to get on with it. Alas, his record is not impeccable.

In his paper¹² "Genetic similarity in male friendships", he used data derived from application of a test of 'conservatism' invented in England in the late 1960s by G. D. Wilson and J. R. Patterson¹³. This farrago devised for the

British *milieu* of the period was transferred to Canada. To what were Rushton's harmless subjects asked to respond? A small sample will suffice: Self-denial, evolution theory, school uniform, hippies, sabbath observance, patriotism, modern art, colonial immigration, Bible truth, pajama parties, inborn conscience.

Surely, enough is enough.

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Anal sex

SIR — John Maddox (*Nature* 358, 13; 1992) criticized *The Sunday Times* for reporting that the Birmingham haemophiliac who infected four women with human immunodeficiency virus (HIV) had had anal intercourse with at least three of them. Neville Hodgkinson in his reply (358, 447; 1992) asked "Why is a journal dedicated to science so afraid of facts?"

I would like to put the same question not only to *Nature*, but also to those involved in AIDS research, and especially to those responsible for the information to the public about risk factors in heterosexual transmission of HIV. The reason is as follows.

Maddox pointed out that the extra hazard of anal intercourse is not novel. That is true. It has been known for at least seven years within the AIDS scientific community. But is it also well-known outside this community? I doubt it. At least in Sweden I have never seen government sponsored campaign indicating that anal sex is risky sexual behaviour.

Maddox refers to a report published by the European Study Group on Heterosexual Transmission of HIV (*British Medical Journal* 304, 811; 1992). In that report, the relative risk of HIV transmission from men to women is increased by a factor of 5.1 in anal intercourse, compared to vaginal intercourse.

What does this relative risk factor mean in practice? Raw data show that 46 per cent of women who had had anal intercourse with HIV-infected men became HIV-positive. This is most alarming to me (and probably to most other people) but not for the European Study Group. They conclude that this high-risk sexual practice (anal sex) was not essential for transmission as 40 out of the 82 infected women never practised anal sex (the other 42 did). Evidently they prefer to talk about relative risks obtained by complex statistical methods and miss or hide the most important message from facts.

A major conclusion from the report in

question should be that a dominant risk for transmitting HIV from an infected man to a woman is to practise anal sex. This is also supported by common sense (which sometimes is of value also in science). Male homosexuals frequently practise anal sex. Everybody knows about the high frequency of HIV transmission within that group.

That anal sex is practised by the heterosexual and bisexual population has to be recognized by society. In the two study groups in the report, the frequency was 23.7–31.8 per cent which is in accordance with results from the Stockholm area. Thus scientists dealing with AIDS research and people, organizations and media responsible for giving information and advice on AIDS protection have a duty to ensure that the following facts are clearly stated to the public: (1) anal sex is practised by many heterosexuals and bisexuals; and (2) anal sex is relatively more dangerous.

This information is especially important for women, because they are the main victims.

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Sex and gender

SIR — According to Fowler's *Modern English Usage* (Oxford University Press, 2nd edn 1965, revised 1983), "GENDER n. is a grammatical term only. To talk of persons or creatures of the masculine or feminine g., meaning of the male of female sex, is either a jocularity (permissible or not according to context) or a blunder."

For reasons of political correctness rather than biological delicacy, however, the nonjocular use of gender as a euphemism for sex seems now to be getting established. Whether this really serves any useful purpose for human sex is perhaps arguable, but its application to nonhuman animals should surely be resisted.

An example appears in *Nature* 358, 704 (1992), on the recovery of an enormous fossil of the ornithischian dinosaur *Stegosaurus*, found recently in Colorado. This has much larger back plates than in another specimen near by, "which may suggest that plate size is an indication of gender, and that the specimen is male".

But according to the rules of zoological nomenclature, and of Latin grammar, the *gender* of the name *Stegosaurus* must be masculine. It is the sex of this particular specimen, whether male or female, which is in question.

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- Maddox, J. *Nature* 358, 187 (1992).
- Rushton, J. P. *Nature* 358, 532 (1992).
- Ankney, C. D. *Nature* 358, 532 (1992).
- Ho, K., Roessmann, U., Straumfjord, J. V. & Monroe, G. *Arch. Pathol. Lab. Med.* 104, 635 (1980).
- Ho, K., Roessmann, U., Straumfjord, J. V. & Monroe, G. *Arch. Pathol. Lab. Med.* 104, 640 (1980).
- Ankney, C. D. *Intelligence* 16, 329 (1992).
- Strauss, S. *Globe and Mail*, Toronto (15 August 1992).
- Freedman, D., Pisani, R., & Purves, R. *Statistics* (Norton, New York, 1978).
- Flynn, J. R. *Race, IQ and Jensen* (Routledge and Kegan Paul, London, 1980).
- Beals, K. L., Smith, C. L. & Dodd, S. M. *Curr. Anthropol.* 25, 301–330 (1984).
- Warnock, M. in Wollstonecraft, M. *A Vindication of the Rights of Women & Mill, J. S. The Subjection of Women* (Dent, London, 1985).
- Ethol. Sociobiol.* 10, 361–374 (1989).
- Br. J. clin. Psych.* 7, 264–269 (1968).