

Sociobiology flops again

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Article:

It is difficult to take seriously something that states flatly that "physical and psychological characteristics can hardly be modified voluntarily," especially during the Olympic Games, which began shortly after Perusse's preprint was circulated for commentary. As people vie for status, we witness menarche delayed by starvation and tortuous exercise or muscle mass grossly exaggerated by pumping iron and popping steroids. Environment is not simply inspiration provided by a parent's dying words, national pride, or greed; it includes life-long habits and education that can sculpt the body and change the wiring of the brain. The idea that genes specify structure while environment merely modulates the mental is very far behind the state of the art in neuroscience.

Sociobiology purports to study the evolutionary origins of contemporary human social behavior patterns. Evolution is the domain of population genetics, social behavior is the domain of social science, yet sociobiology, rather than synthesize the best of these disciplines, constructs a world unto itself with its own journals and like-minded discussants. This is evident in the present target article in several ways.

Evolution without genetics? Perusse argues that "striving to elevate oneself in the social hierarchy" is an adaptation," by which he apparently means it is a behavior with an "underlying" genetic "design" and a product of Darwinian natural selection. In many human societies there is no association between social status and number of offspring, which to him is a conundrum threatening the credibility of sociobiology. What would well-established principles of genetic selection lead us to expect? If in some ancient world there had been a gene with two alleles, one of which conferred higher striving for status than the other, then, provided that higher status in those times did indeed lead to more offspring, the frequency of the allele with higher relative fitness value would have gradually increased. Barring the existence of interactions or negative feedback processes more complex than anything contemplated in Perusse's article, the fitter allele would eventually predominate and the remaining phenotypic variation in status striving would be nongenetic. The raw material of evolution is genetic variation and natural selection tends to exhaust this variation. Those rare mutations, which somehow enhance reproductive fitness, spread through a population and the older alleles disappear: Hence, most current variation in reproductive success should *not* arise from genetic variation. Most of the genetic loci closely related to reproductive success should be fixed, not highly polymorphic, and the functions of the relevant genes would need to be examined with the methods of molecular biology, not Pearson partial correlations.

If I understand Perusse correctly, he hopes to bolster the position of sociobiology in the human sciences by demonstrating a *high* correlation between social status and going through the motions of making babies without actually conceiving anything. To the extent that he succeeds in finding a correlation, which can exist only in the presence of a substantial range of each variable, he marshalls evidence *against* the recent evolutionary advantage of high mating frequency, although he does not seem to be aware of the irony. This is not the first sociobiological treatise to confuse high heritability of a trait with its importance for reproduction, and this commentary is not the first to point out the problem. A careful study of modern population genetics really ought to be a prerequisite for anyone who attempts to theorize about the evolutionary history of our species.

Of course, others might find vindication in the above simplistic scenario and argue that the lack of correlation between status and fertility proves that striving for status *is* genetically favored, and therefore universal, because of past selection. How can one refute a doctrine that sees the hand of Darwin in every possible outcome of history?

If one wants to do a proper study of the genetic evolution of reproductive behavior, it is essential to show that the pertinent behaviors are at least moderately influenced by heredity and that the frequencies of different manifestations of the behavior really do change over generations because of differential reproduction. To argue for the importance of genetic variation, one must at the very minimum show that certain behaviors seen in the parent(s) tend to be more likely in the offspring. The present study examines only one generation of men and oilers not one shred of evidence that their frequency of copulation resembles their fathers'. The study tells us nothing about heredity or evolution one way or the other. It is an exercise in demography inspired by "pop" sociobiology (Kitcher 1985; see also multiple book review, *BBS* 10(1) 1987).

A falling grade in demography. One would hope to find strength in this realm despite the weakness in evolutionary biology. Unfortunately, this aspect of the target article also disappoints. Expressed below is my own opinion, but it is informed by colleagues at the University of Alberta who are recognized experts and whose thoughtful comments are greatly appreciated (S. A. McDaniel, past president of the Canadian Population Society, and F. Trovato, former editor of *Canadian Studies in Population*).

Perusse uses a "snowball" survey technique which may be legitimate when alternative sampling methods are simply not effective, for example, when a representative sample would yield too few cases in rare categories of greatest interest (Veevers 1979). No competent demographer, however, would claim that a snowball survey yields a representative sample. A representative sample is clearly important for testing the author's hypothesis. and with a little more care and effort it could have been obtained. A rather small, unrepresentative minority of Quebecers attends university, and several biases are likely (e.g., nonmarried respondents might be friends of the students whereas married ones are relatives). From the data provided in Perusse's article, no adequate scientific judgment can be made, and a demography journal would certainly require a more thorough description of the methods and results. By publishing the primary empirical report in *BBS*, scrutiny by experts in survey methodology is circumvented.

Perusse does not appear to have a deep knowledge of the relevant literature. For example, he improvises his own scale of socioeconomic status when the Pineo and Porter scale has been recently updated (Blishen et al. 1987). His estimate, of the number of potential conceptions ignores the well-known fact that probability of pregnancy depends strongly on age at marriage and levels of infertility, voluntary and otherwise, which are high and increasing (Menken 1985). It is unacceptable to claim that in traditional societies reproductive "mechanisms are undisturbed," when all known societies work to intervene in biological reproduction in a variety of ways (see Blake 1972).

There is no justification for supposing that self-report yields truthful answers. Self-reports of many behaviors are known to be quite unreliable, especially for sexual behavior, where they are among the *most* unreliable data (Cersted et al. 1970; Greenberg et al. 1970). This is one very good reason to compare the author's data with the Canadian Fertility Survey and his own (unanalyzed) sample of women. The Canadian Fertility Survey provides a representative sample of Canada, including Quebec, and over 25 articles based on this have appeared in print since the data entered the public domain in 1988. It is difficult to understand why this highly relevant source was not even mentioned, let alone utilized, in the target article.

Partial correlations cannot be evaluated properly by the reader when the product-moment correlations are nowhere described. The final sample, especially maritally uninvolved men ($N = 88$), is far too small to warrant analysis of subgroups such as a paltry six men from 40 to 49 years of age. It is bizarre, indeed, to see a partial correlation for all ages (Perusse's Figure 8) *increase* substantially when the range of ages is drastically *reduced*. Statistically, this report leaves much to be desired.

Followers of sociobiology may wish to amuse themselves with profound discussion about the theoretical implications of this form of demographic investigation, but I would prefer to see it scrutinized closely by those best qualified to judge its merits as empirical research before trying to intuit what it all means for the evolution of the Francophone population of Quebec.