

Museum of Comparative Zoology: Expanded "Skeleton Staff"

In a news Briefing "Funding unsexy science" (25 Jan., p. 377), I was correctly quoted as referring to the "skeleton staff" of Harvard's Museum of Comparative Zoology. Unfortunately, this occurred near a mention of the recent drastic staff reductions at the British Museum of Natural History and the San Diego Museum of Natural History, giving the impression that the staff of the Museum of Comparative Zoology has been similarly trimmed. The opposite is the case. The Museum of Comparative Zoology has managed some expansion in staff despite the great difficulty in raising funds, and it is committed to further growth as opportunity allows. But like natural history museums everywhere, it is still sorely understaffed, given the now obvious needs and opportunities of systematic biology.

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IQ and Heredity

T. J. Bouchard *et al.* (Articles, 12 Oct., p. 223) use the IQ score correlation of one-egg (monozygotic) twins reared apart (MZA twins), which was about 0.7 in their recent study and in three previous studies by others, as an estimate of IQ heritability in the population at large. Bouchard *et al.* assumed "no environmental similarity" for co-twins. But the previous studies, each of book length, warned *against* such an extrapolation (1, 2), as did the two most thorough reviews of previous MZA data (3). In the main previous MZA study in the United States (1, pp. 337-343), Stanford-Binet score differences within twin pairs correlated 0.79 with rated differences in educational environment, most of which were small; some pairs had even gone to the same school. Bouchard *et al.* cite neither review and do not report on educational environments.

Correlations for twins in related environments may result from gene-environment interaction (4) as well as from purely genetic (or purely environmental) effects. Above-average mean IQs (around 110) and smaller than average variances for IQ in adoption studies (5) indicate that adoptive homes

make similarly positive contributions to IQ scores (6), not that homes in general make small contributions.

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5. M. Skodak and H. M. Skeels, *J. Genetic Psychol.* **75**, 85 (1949); S. Scarr and R. A. Weinberg, *Am. Psychol.* **31**, 726 (1976); S. Scarr and R. A. Weinberg, *Am. Social. Rev.* **43**, 674 (1978); *Race, Social Class, and Individual Differences in IQ*, S. Scarr, Ed. (Erlbaum, Hillsdale, NJ, 1981); M. Schiff *et al.*, *Science* **200**, 1503 (1978); J. M. Horn, J. C. Loehlin, L. Willerman, *Behav. Genet.* **9**, 177 (1979).
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Bouchard *et al.* purport to show that several psychological traits are highly heritable and, therefore, genetic in origin. They go beyond past hereditarian claims in this field and contend that there is a large genetic basis not only for IQ but for myriad traits such as religiosity, temperament, and vocational and leisure-time interests. Some of their data, however, have not been published in a format that permits independent scrutiny. Investigators in this field should indicate the precise nature of being "reared apart," including, for example, whether it involved only being raised in separate households within the same community. They should also consider the nature of particular adoptions, for example, whether they were made by relatives of the biological parents or friends. In the case of adoptions mediated by welfare organizations, investigators should indicate the criteria by which potential adoptive parents were chosen. Moreover, they should consider the reliability of answers given by identical twins who may be under social pressure to appear to be similar. The fact that data were collected only from the relatively few twins who came forward might also bias the data.

Given these potential problems in assessing the impact of the environment on twins, it is imperative that case studies be fully published. In 12 years, the Minnesota group have not provided these case studies, and the two apparently relevant articles cited by

Bouchard *et al.* (1) contain no case studies.

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Response: Dudley questions how far one can generalize from studies of IQ heritability in separated monozygotic (MZA) twins to the general population. He cites the authors of the three original studies of twins reared apart, each of whom had reservations about generalizability, as well as reviews by L. J. Kamin and S. L. Farber.

We believe we carefully qualified our findings.

Since only a few of these MZA twins were reared in real poverty or by illiterate parents and none were retarded, this heritability estimate should not be extrapolated to the extremes of environmental disadvantage still encountered in society...these findings do not imply that traits like IQ cannot be enhanced....[the] present findings, therefore, do not define or limit what might be conceivably achieved in an optimal environment.

Critical comments about the reviews cited by Dudley have been published elsewhere by a member of our research team (1).

We are aware of the reported correlation (0.79) between Stanford-Binet IQ score differences and rated differences in the educational environment for MZA twins in the study by H. H. Newman *et al.* (2). This correlation is, however, only indirectly related to the heritability of IQ. It is the correlation between a measured environmental feature and the variance in IQ *not accounted for by genetic factors* (differences in IQ between MZA twins must be environmental in origin). The higher scoring twin, on average, received more education. This does not mean that education is responsible for the difference in IQ. The direction of causation may be the reverse. The heritability of IQ in a sample such as ours could be high, and the correlation between co-twin difference in IQ

and a feature of the environment could also be high. We agree with Dudley on the matter of gene-environment interaction, as indicated in equation 2 of our paper. There is little doubt that IQ is malleable. The above-average IQ in adoption studies (including our own) must, however, be interpreted with caution. C. Locurto (3) has reviewed this problem in detail.

The above-average IQ in our sample could be attributed in large part to old norms associated with the WAIS intelligence test. Had we administered the WAIS-R test rather than the WAIS test, the average IQ of our sample would likely have been very close to 100 (4). The attenuated IQ variance observed in our sample can be attributed to our relative inability to sample those rare cases that fall at either extreme of the IQ distribution. More than 90% of the general population have IQs within the range we observed.

Beckwith *et al.* request additional information about the precise nature of being "reared apart," implying that the similarities between the MZA twins might be explained by unreported environmental similarities. We believe this to be highly unlikely. There is a substantial body of evidence relating to this issue, some quite dramatic. T. W. Teasdale and P. R. Owens (5) report a correlation of 0.02 for IQs of unrelated individuals reared together (age 18 to 26 years), and S. Scarr and R. A. Weinberg (6) report a correlation of -0.03 for IQ in 104 of adoptive, nonbiologically related sibling pairs (mean age about 18 years). Loehlin, Horn, and Willerman (7) report correlations of -0.09 and 0.05 for two samples of unrelated individuals reared together (age 13 to 24 years).

The data we reported in our article showed that the circumstances of rearing MZA twins could not have been as similar as that experienced by two individuals raised in the same family. The hypothesis that the twins are under "social pressure to appear similar" does not explain why the twins perform so similarly on IQ tests, special mental ability tests, and psychophysiological tests, as well as on self-reported measures such as personality tests and vocational interest inventories. The question concerning the adequacy of our sample size is answered by our statistical analyses, which take sample size into account.

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Altruism: Docility or Group Identification?

The "docility" model of H. A. Simon (Article, 21 Dec., p. 1665) resembles the "sociality" model presented by L. R. Caporael *et al.* (1). In both models, the general advantages of social life compensate for the costs of foregoing progeny in particular instances. Both models accommodate the gene as a basic unit of selection, but shift to a social psychological mechanism as the important unit of analysis. We applaud this shift for evolutionary analyses of altruism, but we suggest a different mechanism?

Simon does not discuss the significance of group living in both human evolution and in modern life; in fact, he specifically excludes groups (structured demes) as an essential component in his analysis. The crucial mechanism he proposes for understanding altruism is the facility for learning and believing the instruction "society" provides. Yet, as the field for human activity, what is "society" but the patching together of various small groups in various alliances? Humans are *adapted* to live in face-to-face groups; they are unable to survive and reproduce outside the context of group living. Both human history and experimental analysis (2) indicate that the critical mechanism is not just accepting what one is told to do. Rather it is identification with the group, which facilitates accepting group goals as individual goals even when people are perfectly aware of their rational self-interest. People do not vote because they are docile—if they did we could expect a much higher turnout. People *fail* to vote because there is no relevant community inducing identification and commitment. Indeed, they fail to vote because of "rational self-interest."

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1. L. R. Caporael, R. M. Dawes, J. M. Orbell, A. J. C. van de Kragt, *Behav. Brain Sci.* 12, 683 (1989).

Response: I have no disagreement with the Caporael, Dawes, Orbell, and van de Kragt "sociality" model. The thrust of my analysis was to show why docility (of which sociality is a special form) promotes the individual fitness of altruists, a demonstration that I do not believe Caporael *et al.* carried out.

Docility, combined with bounded rationality, enables adherence to group goals (where the group may be as small as a family or as large as a business organization or a nation). Judging actions by their value for the group simplifies decision-making by restricting both the values and the facts one must take into account. In other writings, and in a forthcoming paper (1), I show how group identification, as a product of altruism, is essential to the workability of modern organizations.

In excluding "structured demes" from my discussion, I did not exclude groups, but only a very special class of groups that was not relevant to my argument.

I know of no evidence that "People do not vote because they are docile—if they did we could expect a much higher turnout." Docility need not require full acceptance of all social urging and advice. Group identification is an important component of docility, but not the only one. Apart from this point, the "docility" model is fully compatible with the "sociality" model. In fact, the former provides the latter with a sound neo-Darwinian foundation.

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1. H. A. Simon, *J. Econ. Perspect.*, in press.

Erratum: The last sentence of the fourth paragraph of the letter from Albert B. Sabin that appeared in the 8 March issue (p. 1161) was incorrectly printed. It should have read, "It is well known that polio and other enteroviruses can multiply in the intestinal tract in the presence of antibodies in the blood."