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 identication and commitment. Indeed, they fail to vote because of "rational self-interest."

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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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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."