

## The causes and cures of prejudice: a reply

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In his critique of the Grossarth-Maticsek, Eysenck and Vetter (1989) study, Ray seems to doubt the methodology adopted by us was adequate. We started with the general hypothesis that the Frustration–Aggression theory could be applied to different forms of prejudice, and also formulated the more specific hypothesis that certain forms of frustration, i.e. socio-economic decline and interpersonal stress, would be most likely to mediate this effect. Arguing that this effect would be particularly noticeable at extremes, we constructed a questionnaire concerning eight possible objects of prejudice, which was administered to a group of 6796 males by interviewer. We found that all these items correlated positively together, suggesting a general factor of 'prejudice'. All the wording being rather extreme, it was decided to put a person in the 'prejudiced' group if he agreed with any one of the eight items. Ray argues that the items have no known 'reliability or validity', but this is not so. The correlation between the items is quite high, rising to a value of 0.79 for instance, thus demonstrating considerable reliability as a measure of generalised prejudice. Furthermore, the items are similar to those used in our studies of genetic factors in the causation of social attitudes (Eaves, Eysenck & Martin, 1989), and we have there demonstrated quite high reliabilities for individual items. The validity of the items is as high and as low as that of other items which have been used in the many studies on social attitudes in the literature; it was not the point of our study to demonstrate validity, but rather to concentrate on change.

Ray would have preferred less extreme items, perhaps in the form of scales. This would certainly have been a methodology which could have been adopted, but it would have required far more time than was available, and would have resulted in scores which would have made it very difficult to have selected Ss for the intervention study. By using extreme items, it was possible to select everyone who gave even one 'extreme prejudice' answer for the intervention study. Had we used scales, many subjective decisions would have had to be made about inclusion and exclusion, and looking at the intercorrelations of items, and what is known about the relationship between extreme and less extreme scores, it seems doubtful whether the choice would have been very different from that actually made. This is not to say that other students attempting to replicate our study might not use other methods of selection; it is merely suggested that the choice was not irrational.

Ray suggests that all the intercorrelations might be due to acquiescence bias, but this does not account for the differential size of the correlations between different prejudices, which follow fairly obvious predictions, i.e. the highest correlations are between anti-Semitic, anti-democratic and anti-American prejudice, the lowest between anti-Semitic and anti-Arab prejudice. Clearly Ray is wrong in saying that "it is fully open to us to conclude that the correlation observed between items expressing different types of intergroup hostility was entirely due to a common factor of acquiescent bias".

Ray's criticisms, as voiced in the last paragraph of his paper, seem to be almost entirely semantic in nature. Prejudice against a group, aggression and hostility, particularly when the latter are implicit rather than explicit, are difficult to discriminate, and are essentially identical in meaning (Dovidio & Gaertner, 1986). It is inevitable that in a study like this the appearance of hostility should be implicit rather than explicit, covert rather than overt. It would indeed be interesting to conduct a study on overt aggression, but the evidence suggests that the relationship is close (Eysenck & Coulter, 1972). It would of course be interesting to repeat the study substituting overt aggressive acts for verbal expressions of attitudes, but this was not our intent. Like all the other criticisms voiced by Ray, this essentially suggests alternative and different studies from the one we published; whether these studies would be more or less revealing must be a moot point; only replications with planned changes in methodology will give us an answer to that question.

### REFERENCES

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