

## Research Report

### DIFFERENTIAL USE OF REPRODUCTIVE STRATEGIES BY HUMAN GROUPS?

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**Abstract**—*J.P. Rushton has presented a series of articles proposing that different racial groups, having evolved under different environmental conditions, have been differentially selected, resulting in genetic differences in their reproductive behavior, intelligence, and "social rule-following." Rushton's model is discussed with respect to two controversial points: criticisms of the nature of the data and analyses, and alternative explanations of the data. I concur with Rushton in the belief that different human groups utilize different strategies, but find the differential use of species-wide, environmentally contingent tactics a more parsimonious explanation than genetic differences.*

In two series of papers, one in *Personality & Individual Differences* (Rushton 1985, 1988a, b) and one in the *Journal of Research in Personality* (Rushton 1989a, b; Rushton & Bogaert 1987, 1988), J.P. Rushton has postulated that different human groups, specifically the "Mongoloid," "Caucasoid," and "Negroid" "races," exhibit different reproductive behavior, such that "Mongols" are more likely than "Caucasoids," who are more likely than "Negroids," to exhibit "restrained" sexual activity and childbearing, yet are also more likely to exhibit advanced and intensive parental care for those offspring they do produce. Rushton further posits that these reproductive behavior differences (a) are correlated with parallel differences in intelligence and "social rule-following," and (b) are genetic, having evolved as a correlated set of behaviors, as a result of differences in historical environmental conditions that the "races" presumably experienced.

Rushton's papers have been aggres-

sively criticized in both journals in which they appeared (Lynn 1989a, b; Zuckerman & Brody, 1988), as well as in other publications (Gabor & Roberts 1990; *Human Ethology Newsletter* 1989; Roberts & Gabor 1990; Silverman 1990; Wheeler 1989). The criticisms have ranged broadly, from questioning the credibility of, and possible biases, in the data, to lack of logic and/or faults in the logical structure of the arguments, to lack of discussion of alternative explanations, to ad hominem attacks on Rushton's personal politics and motives. That such criticisms exist is not surprising given Rushton's simultaneous juxtaposition of several already controversial issues—racial differences, sexual behavior, intelligence, and crime—into one theory. The underlying questions that have to be addressed are: to what extent is the theory scientifically valid? and to what extent are the criticisms valid?

#### NATURE OF THE DATA AND ANALYSES

One argument is that Rushton's model cannot be any better than the data and analyses that support it; several critics have diligently pointed out some of the inherent problems and biases in the evidence he presents. For example, there are limits to the interpretation of self-reported data on sex-related behaviors, and probable biases exist in crime statistics and ethnographic reports. In spite of my agreement with these specific criticisms, I do not believe we should throw out the baby with the bath water; few psychologists would want to discard all data derived from self-reports, whether surveys, questionnaires, or paper-and-pencil tests, and few anthropologists would want to throw out data simply because they are unaware of what biases might exist.

Rushton's evidence consists largely of a meta-analysis of a diverse body of data collected by different people, from different sources, and for different rea-

sons. The testing of sociobiological models, especially when applied to humans, often requires the use of such large, pre-existing data sets for meta-analysis or cross-cultural comparison, and although there are inherent problems with such analyses (Mealey & Young, 1985), they can be quite enlightening. Although Rushton's data are not as clean as one might desire, the biases are not uniquely his. Similar problems are shared by many studies that have not received such harsh criticism, suggesting that much of this controversy arises from Rushton's topic or interpretation rather than his data and method.

#### ALTERNATIVE INTERPRETATIONS

Even when original data are collected with specific hypotheses in mind, there often remain problems with interpretation of the results. The error I believe Rushton makes in this instance is to ascribe a pattern found at one level of analysis (groups) to a different level of explanation (genes). This type of error is frequent in the behavior-genetics literature where it appears in the form of claims that, where within-group differences have been demonstrated to be largely due to heritability, between-group differences as well must be due to heritability. An example of such an error would be the claim that, since income is strongly correlated with IQ in both the male population and the female population, the average difference in income between males and females must be due to, or at least correlated with, a parallel average difference in the IQ of the two groups. In the sociological literature, where it also appears, this error is termed "the ecological fallacy" (Singleton et al., 1988). Although the data Rushton cites seem to reveal a pattern across groups, I believe that his labeling of this pattern using evolutionary ecology's r/K terminology is misguided and leads to an erroneous interpretation.

The terms "r-selection" and "K-

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## Reproductive Strategies

selection" were originally introduced by MacArthur and Wilson (1967) in reference to the different kinds of environmental selection pressures that would be operating on species under low density, low competition conditions, versus high density, high competition conditions. Pianka's (1970) paper expanded on the r/K concept, introducing the now-common usage of the adjectives "r- and K-selected" as descriptions of differences between species; in this usage, the terms refer to the clusters of correlated attributes which tend to evolve together in response to density-independent versus density-dependent selection pressures. The central trait of this cluster of correlated attributes is reproductive strategy; to be brief: "r-selected species" use a high quantity, low quality strategy (i.e., they don't expend much energy to ensure the survival of each offspring), while "K-selected species" use a high quality, low quantity strategy. Some authors (e.g., Horn & Rubenstein, 1984) have extended the idea even further to describe within-species differences in reproductive strategy that may be due to genetic differences evolved in semi-isolated groups ("races") over time. Unfortunately, the usage of these terms to refer to within-species differences implies a genetic basis for differences that may in fact be due to the differential, facultative utilization by individuals of species-wide, environmentally contingent tactics (Crawford & Anderson, 1989). This progressive change in the use of the r/K terminology has led to increasing numbers of predictions and tests of the r/K model's theoretical implications, but at the cost of increasing numbers of (possibly incorrect) assumptions and an increasing number of anomalous and contradictory findings (Stearns, 1977).

Rushton acknowledges the different usages of the r/K terminology and the ensuing controversy over its constructual meaningfulness; he then explicitly states that his own usage is intended to be purely descriptive, not explanatory (see Rushton, 1985, p. 442, reiterated in Rushton, 1989a). However, after asserting that his usage is purely descriptive, he then claims that because Negroids have a higher dizygotic twinning rate than Caucasoids or Mongoloids, (in his view, the prima facie indicator of r-selection), his model therefore predicts a

variety of other attributes (Rushton & Bogaert, 1988, p. 260, reiterated more specifically in Rushton, 1989b). Such predictions could only follow logically if one were using the r/K selection model in its explanatory sense, i.e., the "predicted" traits would only be selected together (and therefore correlated) given a specified set of conditions; otherwise they would be selected independently.

In itself, the idea that humans utilize a variety of reproductive and lifestyle strategies should not be controversial: *Homo sapiens*, and perhaps *Homo erectus* and *Homo habilis* before us, have practised infanticide, used various forms of birth control and birth spacing, indulged in a wide variety of courtship and mating tactics, and exhibited the gamut of mating patterns and marriage systems. There is plenty of evidence that these reproductive behaviors differ between groups as well as between individuals; thus I do not intend to reject Rushton's position that group differences in reproductive activities exist. I suspect, however, that these "group" differences are likely due to the differential utilization by individuals of species-wide, environmentally contingent tactics, rather than to differential selection on, and genetics of, human groups; that these differences appear to be group differences rather than individual differences is a result of the widespread segregation of people of different races into different environments.

Even MacArthur and Wilson (1967) suggested that intense K-selection (almost epitomized by humans) would lead to multiple, facultative strategies, rather than to fixation of a single strategy. Twinning itself, which Rushton considers to be a primary indicator of r-selection, may be an environmentally contingent strategy: twinning is thought to be influenced by nutrition (Bulmer 1970), and many twin conceptions are apparently resorbed by the mothers early in pregnancy (Finberg & Birnholz, 1979; Levi 1976; Robinson & Caines, 1977; Schneider, Bessis, & Simmonet, 1979), suggesting that twinning (and non-twinning) may be facultative. Allen (1981; see also others, same volume) gives a particularly interesting presentation of data suggesting that twinning may be as closely related to psychological variables as to genetic ones. Rushton

(1988b) cites Allen, but not in this context. Other physiological variables Rushton examines may also have facultative components: examples are length of menstrual cycling (Cutler et al., 1986) and age of menarche (Surbey, 1987). The same is true of many of the behavioral variables, for example, early heterosexual activity (Hetherington, 1972) and marital stability (London, Kahn, & Pratt, 1988).

Mackey and Mealey (1988, in prep.) found that countries with high infant mortality were more likely than those with low infant mortality to be tropical than temperate, and tended to be populated more by people of "Negroid" descent than "Caucasian" or "Mongoloid" descent (to use Rushton's categories). This pattern is in keeping with Rushton's model, since high mortality is an attribute of r-selection and low mortality is an attribute of K-selection. A more parsimonious explanation would be that the pattern of infant mortality is due to differences between the groups' current environments: that in the relatively poor, lesser-developed countries, which are predominantly tropical and "Negroid," poor maternal nutrition leads to high overall infant mortality. Although it is possible that these differential conditions predominated over periods of evolutionary time and resulted in genetic differences, it is not necessary to postulate a genetic difference when an environmental one will suffice. In fact, the high population density that is correlated with reduced resources is correlated also (presumably) with increased competition, which would suggest K-selection in the poorer areas rather than r-selection, in direct contrast to Rushton's model.

Draper and Harpending (1982, 1988), Belsky and Draper (1987), and Blain and Barkow (1988) present an environmentally contingent model of personality development that postulates a sensitive period during childhood when reproductive and other social strategies are determined in response to prevailing social conditions. The key determining factors—household structure, parenting style, and perceived resource availability—exhibit group differences that could easily be culturally transmitted. This environmental model provides an alternative explanation of the pattern to which

Rushton refers without necessitating any underlying genetic variability.

Of course, a correlation of behavior with differential environmental conditions does not prove that those conditions cause the variation in behavior any more than correlations of behavior with genetic differences prove that the genes cause the behavior. To demonstrate whether genetic or environmental differences lead to behavioral differences between individuals or groups, one would have to isolate those elements artificially, as generally can be done only using selectively bred and reared lab animals; the closest one can get to such a design using human subjects is the study of identical twins raised apart. I have recently analyzed such data (obtained from the Minnesota Center for Twin and Adoption Studies). Although they confirm moderate heritabilities of previously studied personality variables such as introversion-extraversion and activity level, the results suggest that monozygotic (identical) twins are no more similar on what might be considered reproduction-related behaviors (dating frequency, how much the S's desired children, age at birth of first child) than dizygotic (fraternal) twins. The exceptions (age at first date and age at first marriage), did not in any case translate into actual reproductive differences, that is, total number of children. Most of the similarities between twins appeared equally across MZ and DZ twins, suggesting a large effect of shared extra-familial environmental factors, such as religion, birth cohort, and general locale of rearing. These results are more consistent with the environmental-cause model of individual and group differences than the genetic model.

## CONCLUSION

All in all, I find the pattern that Rushton presents interesting and worth pursuing; but his interpretation is not the only one compatible with existing data. Differential utilization of reproductive strategies may be environmentally contingent rather than genetic, and apparent group differences a result, therefore, of

the segregation of different human groups into different environments.

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