

HOW THE HUMAN GOT ITS SPOTS

A Critical Analysis of the Just So Stories of Evolutionary Psychology

By Henry D. Schlinger Jr.

In 1902 Rudyard Kipling published a children's book of stories and poems with the curious title, *Just So Stories*. They included such natural curiosities as "How the Elephant Got Its Trunk," "How the Rhinoceros Got Its Skin," and "How the Leopard Got Its Spots." The stories, of course, are pure fantasy, and "just so stories" has become a critical cliché for similarly fanciful tales that attempt to explain nature. The new field of evolutionary psychology, while different in many respects from its predecessor sociobiology, is still subject to the accusation of telling just so stories.

As a sampling from this new science, the following are headlines from recent articles or reviews of various books appealing to evolutionary explanations of human behavior:

Cheating Husband: Blame it on His Genes?

Is There a Gene for Compassion?

Is Prejudice Hereditary?

***A Scientist Weighs Evidence That the X chromosome
May Carry a Gene for Gayness.***

IQ: Is It Destiny?

Headlines such as these are meant to capture the attention and imagination of readers, and they usually do. They suggest that the books to which they refer are going to offer serious scientific evidence for their claims of an evolutionary explanation of much human social and intellectual behavior. Do these claims reflect the results of serious science or just more "pop sociobiology," as Kitcher (1985) calls it?

Most books on sociobiology appeared in the decade between about 1975 and 1985. Barash's 1977 *Sociobiology and Human Behavior*, Lumsden & Wilson's 1983 *Promethean Fire*, and especially E. O. Wilson's two great works, *Sociobiology: The New Synthesis* (1975) and *On Human Nature* (1978), created a new field of study of human behavior that forcefully challenged the hegemony of behavioral psychology that had reigned so long. Despite the existence of serious critical analyses of sociobiology (e.g., Bock, 1980; Futuyma, 1979; Gould, 1981; Kitcher, 1985; Sahlins, 1976), in the past few years, there has been an explosion of books offering evolutionary explanations for a variety of human behaviors, including intelligence, morality, mating, sexual preference, aggression, xenophobia, prejudice, and even our tendency to seek out various forms of nature, such as trips to zoos and visits to national parks. These books may be classified according to two distinct but related arguments about the evolution of human behavior: (a) individuals and groups that differ behaviorally in some way (e.g., IQ), do so because of underlying

genetic differences; and (b) invariant, universal human traits (e.g., morality, aggression) represent fixed expressions of the human genome (Futuyma, 1979).

Recent books that argue for genetic differences between groups of humans with respect to such characteristics as intelligence include *The Bell Curve: Intelligence and Class Structure in American Life* (1994) by Herrnstein and Murray, *Race, Evolution, and Behavior* (1995) by Rushton, and *The Decline of Intelligence in America: A Strategy for National Renewal* (1994) by Itzkoff. Books that make the case that there are distinctly human behaviors—collectively called human nature—that reflect a uniquely human evolutionary history, include *Homicide* (1988) by Daly and Wilson, *The Biophilia Hypothesis* (1993) edited by Kellert and Wilson, *The Moral Animal* (1994) by Wright, *The Evolution of Desire* (1994) by Buss, *Why We Get Sick: The New Science of Darwinian Medicine* (1994) by Nesse and Williams, *Eve's Rib: The Biological Roots of Sex Differences* (1994) by Poole, *The Science of Desire: The Search for the Gay Gene and the Biology of Behavior* (1994) by Hamer and Copeland, and *The Adapted Mind* (1992) by Barkow, Cosmides, and Tooby.

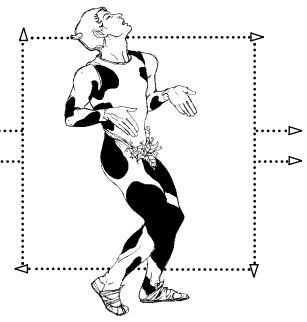
Both arguments on the evolution of human behavior rely to varying degrees on a combination of three types of supporting evidence:

1. Evolutionary logic supported by casual observations or statistical data.
2. Behavioral analogies and comparisons with animals.
3. Statistical analyses of data generated by non-experimental research methods.

Each of these types of evidence, while sometimes compelling and frequently interesting, is often flawed scientifically. This does not mean that the explanations themselves are wrong, only that the supporting evidence is insufficient. In many instances, an alternative, and much more plausible approach to understanding human behavior is that rather than selecting for specialized behavioral traits, human evolutionary history has selected for behavioral plasticity, or learning capacity (Futuyma, 1979). Experimental evidence from the literature on learning shows overwhelmingly the powerful influence of the environment in shaping human behavioral similarities and differences.

In the present essay I describe the three types of evidence with supporting examples from both evolutionary positions on human behavior and then critique them according to certain methodological criteria. I argue that,

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in most cases, a much more cautious and scientifically defensible position on the origin of many human behaviors is that they are a function of individual environmental, and not evolutionary, history.

Evolutionary Logic

One of the hallmarks of the scientific method is the interpretation of phenomena that have not been subjected to experimental analysis. Scientific interpretation is the use of already established principles of science to explain novel instances of the subject matter. Hence, the logical or mathematical use of Darwinian principles of selection to interpret human behavior could have a sound basis in science. The main questions are (a) whether the data presented for interpretation are both valid and reliable, and (b) whether the interpretations of human behavior as presented in recent books and articles represent an appropriate extension of Darwinian theory.

Theorists from both positions on the evolution of human behavior cite examples of evolutionary logic and supporting data that are problematic. Theorists who emphasize genetic differences between groups of humans (races) have employed evolutionary logic to explain differences in intelligence (Herrnstein & Murray, 1994; Itzkoff, 1994; Rushton, 1995a), brain and head size and aggressiveness (Rushton, 1995a), among other traits. Evolutionary psychologists have used evolutionary logic to explain, among other things, why people kill one another (Daly & Wilson, 1988), why mothers who have just given birth seem to mention their neonate's resemblance to the father more than to themselves (Daly & Wilson, 1982), why social rejection may produce feelings of insecurity (Wright, 1995), and why people seek out zoos and parks and easily develop phobias to natural objects, like spiders (Wilson, 1993). The data cited by these theorists consist of casual observation, personal reflection, and anecdote, as well as statistics derived from non-experimental studies. To illustrate, consider an example of the use of evolutionary logic from each of the two positions on the evolution of human behavior.

Rushton (1995a) uses evolutionary logic to support his claim that human racial groups evolved under conditions where different environmental pressures selected for differences in a wide range of physical and intellectual characteristics. Rushton suggests that an *r-K* reproductive strategy analysis combined with information on human evolution can be used to understand important behavioral differences between Mongoloids, Caucasoids, and Negroids, as he calls them. The *r*-strategies are those with high reproductive rates, and the *K*-strategies are those with high levels of parental investment in offspring. According to Rushton (1995a), "Mongoloid people are more *K*-selected than Caucasoids, who, in turn, are more *K*-selected than Negroids" (p. xiii). In other words, Mongoloids invest relatively more in the care of their offspring than Caucasoids who invest relatively more in the care of their offspring than Negroids. Rushton appeals to evolutionary logic to explain the presence of these different *r-K* strategies in different human racial groups. Specifically, Rushton claims

that the selection pressures in the hot African savanna, where Negroids evolved, were far different in terms of the required relationship between parental investment and high reproductive rates than selection pressures in the cold Arctic environment where Mongoloids evolved. Presumably, higher reproductive rates and lower rates of parental investment are more favorable in hotter climates, whereas the opposite is true in colder climates. According to Rushton, this is the evolutionary basis for the differences in *r-K* reproductive strategies supposedly observed in humans.

The first problem with Rushton's analysis concerns the reliability of the data offered to support his evolutionary logic. For example, he provides a table of the relative ranking of races on diverse variables such as physical maturation rate, including age of first sexual intercourse and pregnancy; reproductive effort, including relative frequency of two-egg twinning and of intercourse; personality, including aggressiveness and impulsivity; brain size; and intelligence (Rushton, 1995a, 1995b). The data for these rankings were generated by non-experimental research methods where average differences between groups were often very small. Moreover, there is no scientific evidence, other than correlations, to support many of Rushton's assumptions, including his assumption that brain size is functionally related to cognitive ability.

Rushton often relies on statistical analyses of aggregated data to bolster his claim that small differences between groups are significant. Even if we assume that the data cited by Rushton were derived from well-designed and well-controlled studies—a questionable assumption—his evolutionary interpretation of the data has several attendant problems. First, there is no way to test and thereby falsify his claim that these characteristics represent evolutionary adaptations. Rushton's evolutionary logic is not too dissimilar from that used by his sociobiological predecessors, as summarized by Futuyma (1979). He has simply imagined that higher reproductive rates and lower rates of parental investment must have conferred differential fitness in different climates, compared the predicted outcome with observations from correlational studies, and then concluded that these characteristics represent adaptive genetic traits. A second problem with Rushton's hypothesis is that his extension of the *r-K* reproductive strategy analysis (usually used to compare large differences between different species) to the small variations between groups within the human species, represents a "fatal scientific error" by assuming that behavioral differences between groups within one species can be accounted for by genetic differences (Tavris, 1995). It is not even clear that behavioral differences between *individuals* reflect genetic differences or, if they do, to what extent (Futuyma, 1979). A third problem is that Rushton's concept of race, which reflects that of Western culture—based on a few physical features such as skin color, hair form, and the epicanthal fold—is subjective (Futuyma, 1979). And finally, any reliable differences in Rushton's data are just as likely to be due to environmental variables as genetic ones. Still, Rushton (1995a) boldly contends that his

book will offer “new truths about racial group differences.”

Consider, now, an example of how evolutionary logic might be used to interpret some human characteristic from the perspective of evolutionary psychology. Robert Wright, a science journalist, writing in *The New Yorker* (March, 1995), illustrates how evolutionary psychologists would approach the explanation of some presumably universal human behavioral trait. Suppose, Wright asks, that social rejection early in a person’s life results in an enduring insecurity. According to Wright, we should ask whether this pattern “might have had a genetic payoff during evolution” (p. 71). Presumably, our ancestors who faced such rejection were less likely to reproduce unless they became more socially vigilant about nourishing their social ties as a result of the insecurity. Insecurity as a response to social rejection, then, may have been reproductively advantageous for humans. The assumptions inherent in Wright’s argument can be stated as follows: (a) human evolutionary history has selected a genetic “program” that is somehow sensitive to environmental input called “social rejection,” (b) this genetic program is especially sensitive to input early in an individual’s life, and (c) the behavioral response called insecurity is essentially the same for all people to this input.

There are several obvious problems with this example that are relevant to many such examples cited by evolutionary psychologists. The first problem is with the validity of the behavioral data. Wright simply assumes that insecurity, which is not objectively defined, is a general human response to early social rejection, which is also not objectively defined. Wright offers no evidence that his evolutionary model is based on precise behavioral observations. Rather, his analysis is based on common sense assumptions about human nature which have no scientific basis. A second problem deals with Wright’s evolutionary interpretation of the data. Even if such a reaction could be precisely measured and were observed in most humans as a result of a precisely defined set of environmental inputs, an evolutionary interpretation that it was adaptive is untestable because there is no crucial test that can falsify the hypothesis (Futuyma, 1979). Finally, an evolutionary explanation of the pattern of behavior in Wright’s example may not be the most parsimonious one. For example, it might be that the reaction to rejection that we refer to as “feelings of insecurity” might be a more general physiological response to the withholding or withdrawal of reinforcement following some behavior. The effect of such environmental operations is to simultaneously produce physiological responses and to alter the stimuli that define the situation such that they suppress the behavior under similar circumstances. These are the scientific principles of *operant extinction* and *punishment*. The “feeling of insecurity” may be a by-product of the withholding or withdrawal of reinforcement with no special selective advantage of its own.

Cross-Species Comparisons

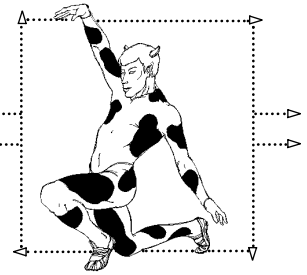
A second type of evidence frequently used to support evolutionary explanations of human similarities and differences consists of analogies or comparisons between nonhuman and human behavior. It is common linguistic

practice among humans, including scientists, to give names to things. When two or more forms of behavior are given the same name, it may seem reasonable to assume that they are alike functionally as well. Kitcher (1985) points out that because we have such a rich vocabulary for describing human behavior, it is easy to use this vocabulary to describe nonhuman behavior that resembles it. Once described in similar ways, it becomes easier to then move freely from the nonhuman instance back to the superficially similar human instance and to assume that both result from similar processes. According to Kitcher (1985), “vulgar anthropomorphism” is the original sin of pop sociobiologists, in that they neglect “to investigate the kinship of forms of behavior that are superficially similar” (p. 185). Even if scientists discovered a genetic basis for a behavior in an animal, which is rare, this does not mean that the human behavior that appears to be similar also has a genetic basis. As evolutionary biologists know, phenotypic similarity does not necessarily imply genotypic similarity.

Social theorists, like Rushton, who emphasize genetic differences between groups of humans typically point to between-species differences that are more than likely a function of differences in genes to make the case that within-species differences in humans are also a function of differences in genes. Rushton (1995a) employs an interesting kind of cross-species analogy to make a case for the genetic basis of human racial differences. First, he points out that significant differences in learning ability *between species* are due to genetic differences. Thus, mammals with larger brains, such as chimpanzees, rhesus monkeys, and spider monkeys, learn faster than mammals with smaller brains, such as marmosets, cats, gerbils, rats, and squirrels. Rushton then uses these comparisons to argue that *within-species* differences in human brain or head size are related to differences in intelligence, at least as measured by standardized IQ tests, and are likewise related to genetic differences. Rushton’s ultimate point is that blacks have statistically smaller heads (and brains) than whites and that this correlates positively with differences in intelligence between the two groups, at least as measured by standardized tests. It is interesting to note that of the 32 studies summarized by Rushton on head size and intelligence in humans, most found low correlations.

Rushton takes a reasonable between-species example and extends it to an insupportable within-species difference. Even if the measurements of brain size and intelligence can be defended as reliable, Rushton’s explanation of the behavioral differences is not the most parsimonious one, especially when one considers the myriad differences in environments on average between black and white children. Before genetic explanations of differences in learning ability between individuals or groups are proffered, environmental factors, such as nutrition, prenatal care, learning, and educational opportunities, should be investigated if, for no other reason than the variables are easier to test.

Another example of questionable cross-species analogizing by Rushton (1995a) concerns the *r-K* reproductive strategies described previously. According to Rushton, the great apes exemplify the extreme end of the *K*-strategy because they produce one infant every five or six years and



provide much parental care. At the other extreme are oysters who exemplify the *r*-strategy, producing 500 million eggs a year but providing no parental care. Although this scale is generally used to compare the life histories of widely disparate species, Rushton (1995a) applies it to the much smaller variations within the human species. Although Rushton believes that all humans are *K*-selected relative to other species, he also believes that some humans may be more so than others. He cites data showing that, compared to white women, black women average a shorter period of ovulation and produce more eggs per ovulation which is evidenced by their comparatively higher rate of two-egg twinning. His data also show that black women have comparatively lower intelligence than white women as measured by standardized tests. Rushton claims that the correlation between IQ and biological variables related to reproduction supports his view that the within-species variations in humans can be accounted for in the same way that between-species variations can. Even if the correlation can be proven to be valid, there are serious problems with Rushton's cross-species comparison. First, there is no biological justification for extending an analysis of between-species differences to within-species differences. Second, Rushton provides no evidence other than correlations that differences in IQ and certain biological variables between women represent adaptations resulting from natural selection. Third, simply demonstrating a correlation between two or more variables in no way clarifies causal relations.

Evolutionary psychologists, like their sociobiological predecessors, frequently employ cross-species analogies and comparisons to argue their case for the existence of universal human characteristics. For example, Daly and Wilson (1988) use an analogy with female ground squirrels to show how the concept of inclusive fitness may be used to understand sibling rivalry in humans. They argue that genetic relationship should be important to solidarity and social conflict. In other words, the closer the family relationship between two individuals, the more solidarity and the less conflict should exist between them. Daly and Wilson point out that such a theory has been tested in female ground squirrels who discriminate between their full sisters and half sisters when occupying adjacent territories as adults. Full sisters will apparently help each other whereas half sisters will exhibit more territorial aggression. They then suggest that the same prediction can be made with regard to human siblings; namely, that the intensity of sibling rivalry should reflect the likelihood of common paternity. In other words, full siblings should show less competition than half siblings. In their own words, "we might have evolved specialized psychological mechanisms whose function is to assess the likelihood of common paternity and to adjust the intensity of sibling competition accordingly," and some "psychologist should check it out" (1988, p. 11).

Cross-species analogies, such as the one offered by Daly and Wilson (1988), are intriguing suggesting as they do, that certain human characteristics that we seem to have in common with other species may be understood as part of our deeper human nature. There are serious problems with such analogies, however. The first problem is that the simi-

larity between human and nonhuman behaviors is subjective and is only suggested *after* it is believed that there may be a common genetic basis for both. In other words, behavioral similarity is often in the eyes of the beholder. Who is to say that territorial aggression among ground squirrels is anything but superficially similar to disagreements or fights among human half-siblings? The causes of these similar behaviors could be completely different. A second problem is that even if the behavior of human siblings could be compared to female ground squirrels, there is no independent evidence for the existence of an evolved "psychological mechanism" or any suggestion as to how it would work to "assess the likelihood of common paternity and to adjust the intensity of sibling competition accordingly." In the absence of such a suggestion, based on some kind of objective scientific evidence rather than inferences, Daly and Wilson's explanation is simply hypothetical.

Futuyma (1979) has pointed out several other problems with cross-species analogies. For example, even if behavioral generalizations could be supported by reliable observations, we are still left with the nagging question of whether behaviors between species that are superficially similar are functionally similar; that is, whether the same processes are responsible for both. If we discover the genetic bases of territorial aggression in female ground squirrels, does this mean that behaviors we refer to as "human sibling rivalry" also have a genetic basis? A simpler approach would be to consider first whether other factors, such as environmental ones, could produce the human behaviors of interest. Such an approach might lead us to ask, for example, whether there is as much sibling rivalry between half-siblings who are raised together from birth or infancy and who are not aware of their genetic relationship to each other as there is between siblings who know they are half-siblings. Other than the interesting evolutionary theorizing that superficially similar behaviors in different species may be functionally similar, evolutionary psychologists offer no direct scientific evidence that they are.

Correlative Analysis

It should be noted that social evolutionary theorists typically do not conduct experiments, nor do they, in most instances, cite experimental data. Rather, they rely almost exclusively on a combination of anecdotal and statistical evidence to make their case that there are species-specific behaviors in humans. Moreover, in almost no case is direct genetic evidence used to support evolutionary theories of human behavior (see below). Since genes are identified as playing a causal role in important similarities and differences between humans, a true experimental test of the hypothesis would necessarily involve direct manipulation of genes as independent variables. Such manipulations are only carried out by geneticists and, for obvious reasons, they have been constrained in such endeavors to working with relatively simple organisms, such as fruit flies with extremely short gestation periods, where the focus is more on structural than behavioral characteristics. Those who

write about the genetic bases of human behaviors are typically not geneticists, however. And because they cannot make their genetic case experimentally, these evolutionary theorists must rely on data generated by non-experimental, usually correlational, research methods. There are several problems with the ways in which some evolutionary theorists use correlative analysis.

Validity and Reliability of the Data. The first problem is the validity and reliability of the methods used to generate the actual data are often questionable. E. O. Wilson (1993) states that one mode of testing an evolutionary hypothesis “is the correlative analysis of knowledge and attitudes of peoples in diverse cultures” (p. 34). Knowledge and attitudes, poorly defined as they are, must be obtained from surveys and questionnaires. Methodological problems with such devices are well known among researchers. For instance, there are numerous ways in which researcher bias may affect the outcome, such as the sampling procedure used and the way in which questions on surveys and questionnaires are worded. Even when safeguards are included, inferences to larger populations (the ultimate goal of surveys or questionnaires) are questionable. Also, as most good researchers know, the reliability of verbal self reports is notoriously poor.

In addition to surveys and questionnaires, evolutionary theorists may use psychological tests to assess more general and presumably universal characteristics of populations. Rushton (1995a) provides an example of the use of such a test. His thesis of racial differences is based on the assumption that there is “a core of human nature” or character traits “around which individuals and groups consistently” differ. To wit, he cites a study conducted in the 1920s by Hartshome and May called the “Character Education Enquiry” in which 11,000 elementary and high school students were given a battery of 33 different tests of altruism, self-control, and honesty in various contexts (home, school, church, etc.). Children’s reputations with teachers and classmates were also obtained and then correlated with the scores on the battery of tests. Notwithstanding the problems with questionnaires, the only behavior measured by such tests is that of answering questions on the test. The actual behaviors called “altruistic” or “honest” are not measured in the context wherein one would normally call them altruistic or honest. This is not to say that we cannot discern something of value with such tests, but only that the test may correlate poorly with the behaviors of interest, and only a direct experimental approach can potentially yield a scientific understanding of the behaviors.

Of course, the most notorious type of test cited in the literature on evolutionary theories of human behavior is the IQ test. Volumes have been written on problems with intelligence tests, and I will not repeat them here. Suffice it to say that one problem with such tests is what they purport to measure. Rather than measuring some qualitatively distinct structure or process as defenders of such tests would have us believe, intelligence tests literally measure only the correctness of a variety of learned behaviors—answers to questions on the test—in a contrived context—the test taking situation (Schlinger, 1992). Alfred Binet knew this when he developed the first modern intelligence

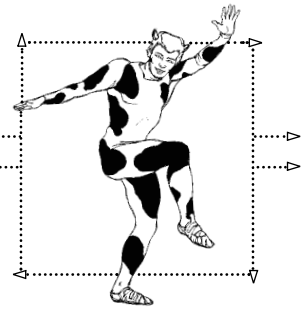
test (although he eschewed the use of the term “intelligence” in favor of the more descriptive and neutral, “intellectual level”). The challenge for serious scientists is to ask about the variables that affect the broad range of behaviors we describe as *intelligent*; and only an experimental analysis can answer such questions.

The Use of Statistics. A second problem with the use of correlative analyses by evolutionary theorists concerns the complex statistical tests employed to “make sense” of the data generated by surveys, questionnaires, psychological tests, and the like. The importance of correlative analyses in making the argument for genetic explanations of human behavior is underscored in the following quotation by Sir Francis Galton, which Rushton twice cited (1995a, b):

General impressions are never to be trusted. Unfortunately when they are of long standing they become fixed rules of life, and assume a prescriptive right not to be questioned. Consequently, those who are not accustomed to original inquiry entertain a hatred and a horror of statistics. They cannot endure the idea of submitting their sacred impressions to cold-blooded verification. But it is the triumph of scientific men to rise superior to such superstitions, to devise tests by which the value of beliefs may be ascertained, and to feel sufficiently masters of themselves to discard contemptuously whatever may be found untrue.

The most obvious problem with this quote and the approach to the study of individual differences that it fostered is the equation of statistics, in the absence of experimentation, with scientific practice. Although we may debate the role of inferential statistics in the natural sciences, it is true that Galton’s quote predated the application of the experimental method to the behavior of organisms by psychologists (e.g., Skinner, 1938). Rushton (1995a) and Herrnstein and Murray (1994), however, consider Galton to be the intellectual and scientific father of their genetic theories of racial differences. Rushton calls Galton “the originator of *scientific* research on individual differences” (1995a, p. 10, italics added). Herrnstein and Murray, who refer to the Galtonian tradition of intelligence testing as “the classic tradition,” claim: “By accepted standards of what constitutes scientific evidence and scientific proof, that classic tradition has in our view given the world a treasure of information...” (1994, p. 19). This is especially interesting coming from a scientist such as Herrnstein whose own scientific output consists almost exclusively of the use of within-subject experimental designs.

Authors such as Herrnstein, Murray, and Rushton point out that while individual scores on behavioral or psychological tests, for instance IQ tests, correlate poorly, the correlations become much higher when scores are aggregated. The principle of aggregation, according to Herrnstein and Murray (1993), is where the classic (Galtonian) tradition has the most to offer. The rationale for aggregating data is that “randomness in any one measure (error and specificity variance) is averaged out...leaving a clearer view of what a person’s true behavior is like” (Rushton, 1995a, p. 19). Also, relationships between individual tests or between scores on tests are more likely to emerge. Thus, aggregating data is supposed to correct for any errors in the



actual measurement of the variable(s) in question. The contradiction in this line of reasoning is that the further away one gets from the behavior of the individual, the less can be said about the individual. Herrnstein and Murray acknowledge that the practice of aggregating data does not necessarily permit the prediction, much less the understanding, of individual behavior. More importantly, aggregating data from different tests, or, worse, from different studies, is fraught with so many methodological problems as to render the results meaningless. For example, aggregating data masks differences in methodology (e.g., time, place, populations, sampling procedures, control procedures, measurement tools, etc.). Aggregating data, especially from different studies, can only mean that the results of any individual study were so equivocal that no conclusions could be drawn. Pooling data from different studies is only valid if the studies are methodologically interchangeable which, as I have implied, is a questionable assumption in the present case. Nevertheless, Rushton (1995a) describes instances where low correlations between individual tests were raised by aggregating data from many different tests as if this were sound scientific practice.

In criticizing formalized methods of research and statistics, B. F. Skinner (1972) advocated the use of the experimental method in the study of human behavior. Each approach leads to a different strategy for dealing with measurement error. In contrast to the strategy of aggregating scores from many individuals to increase the statistical reliability of the measurement device (e.g., IQ test) or the sensitivity of the statistical method (e.g., t Test), Skinner (1972) argued for refining direct experimental control over the behavior of individual subjects. In this way, the reliability of the independent variables is enhanced and sources of variability are eliminated *before* measurements are made rather than after, as is the case when researchers aggregate data. As Skinner (1972) wrote tongue-in-cheek, “No one goes to the circus to see the average dog jump through a hoop significantly oftener than untrained dogs raised under the same circumstances...” (p. 114).

Interpreting The Data. A third problem with the use of correlative analyses involves the interpretation of the data. Demonstrating that a correlation exists between two or more variables does not in any way clarify causal relations, although it may hint at possible ones. There is an oft-cited dictum among researchers: “Correlation does not imply causation” (Neale & Liebert, 1973). A correlation between two or more variables is often due to an unspecified process, or “third variable.” Those who argue for an evolutionary explanation of human behavior appeal to a third variable—the human genome. Although it is theoretically possible that some human social and intellectual behaviors represent fixed expressions of the human genome, a better explanation for the behaviors in question is one in which a different third variable is implicated—the environmental histories of individuals. In many of the examples cited by social evolutionary theorists, any one or more of the multitudinous environmental variables found in the individual histories of the subjects studied may produce the reported correlations. Just as behavioral similarities between individuals may reflect genotypical similarity, they may just as easi-

ly reflect environmental similarity. The correlational evidence offered by evolutionary theorists is simply insufficient to distinguish the biological from the environmental position. The challenge for scientists is to tease apart these possible determinants of behavior, and this cannot be accomplished using correlational methods. Only an experimental analysis can potentially reveal the variables of which human behavior is a function. Galton got it wrong. The “triumph of scientific men” occurs not when human behavior can be subjected to statistical correlation, but rather when it can be subjected to direct experimentation.

Whether one conducts experimental or correlational research in the first place reflects fundamental differences in the types of questions asked. And the types of questions asked reveal differences in the motivations of the researchers. Many authors who either conduct and/or cite correlational research on the relation between behavioral and genetic differences and similarities between groups of humans, do so to show what they already believe—that genetics plays a significant role in such characteristics as intelligence, aggression, and reproductive behavior. Hubbard and Wald (1994) have noted that “scientists only look for genetic components in behaviors which their society considers important and probably hereditary” (p. 93). For instance, they point out that even though European peoples read from left to right, whereas Semitic peoples read from right to left, no one has suggested that these are inherent racial differences. As Futuyma said (p. 473):

The history of scientists’ pronouncements on human genetics and behavior is, to a distressing extent, a history of the conventional societal attitudes on these subjects; science has served more as a defense of the *status quo* than as a force for change.

Genes

I have referred to the social theorizing discussed in this paper as evolutionary; and such a conception implicitly recognizes that what has evolved due to natural selection is a particular genotype that is different from other possible genotypes. In short, evolutionary theories are genetic theories and, as such, we should expect some supporting genetic evidence. According to Kitcher (1985), physical characteristics most susceptible to rigorous genetic analysis are not those that social evolutionary theorists find most interesting. For example, it was recently reported that scientists at the University of Basel in Switzerland have discovered the master control gene responsible for eye development in fruit flies. The scientists have been able to manipulate the gene directly so as to produce eyes in unusual places, like on the legs and thorax. Human geneticists, by comparison, are relegated to studying genetic variation that produces deleterious effects, such as metabolic disorders and defects in color vision. In other words, human geneticists are unable to manipulate the actual genes and must wait for natural genetic variation to produce outcomes that they can then investigate. The genetic

evidence most often cited by social evolutionary theorists comes from the field of behavior genetics. Contrary to their name, behavior geneticists do not directly study genes. Rather, they are constrained to examining correlations between poorly defined variables such as scores on intelligence or other psychological tests and family relationships. The reliability of the observations and measurements reported by behavior geneticists is questionable because of the many methodological problems inherent in such research. For example, several authors have pointed out problems with subject selection in research on separated identical twins (e.g., Horgan, 1993; Hubbard & Wald, 1994; Kamin, 1974; Lewontin, Rose, Kamin, 1984). Moreover, the fact that conclusions about the differences in genes must be based on family resemblance introduces a well-known confound: Family members resemble each other not only because they share genes but also because they share environments. Despite the perception that behavior geneticists have made impressive gains in demonstrating the genetic bases for a wide range of human conditions, such as aggression, homosexuality, intelligence, schizophrenia, and alcoholism, there have been an equal number of serious methodological critiques which, at the very least, temper the claims by behavior geneticists (e.g., Byne, 1994; Horgan, 1993; Kamin, 1974; Hubbard & Wald, 1994; Lewontin, Rose, & Kamin, 1984).

Some social evolutionary theorists argue their case based on a flawed interpretation of evolutionary and genetic logic. For example, Itzkoff (1985), who is neither an evolutionary biologist nor a geneticist, presents a case for the evolution of human intelligence as a function of the natural selection of the human brain. Itzkoff reasons that because so many biochemical combinations are involved in the growth and patterns of brain structure, slight variations can exist between close relatives and large variations between relatively isolated groups of humans. He concludes: "The brain evolved along a wide diversity of lines" producing differences in both "the quantity and quality of intelligence" (p. 23). He presents this rationale to support his claim that different groups of humans (blacks and whites) come into the world with different genetic potentials for intelligence. There are serious flaws in Itzkoff's reasoning, the most fatal of which is that there is simply insufficient evidence to support his conclusions that normal variation in intelligence has a genetic basis. Moreover, his argument is based on the assumption that there exists genetic variation within populations of humans, and that selection has operated differently in different human groups even though "there is insufficient evidence to conclude that normal variation in human behavioral traits has a genetic basis" (Futuyma, 1979). Finally, there is a broader principle of genetics that is often not fully appreciated by many social evolutionary theorists, as Futuyma notes (p. 476):

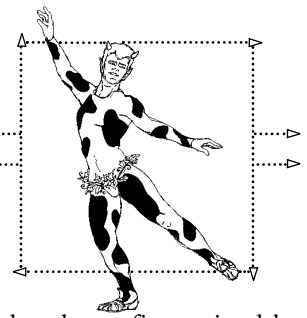
One cannot say that a universal trait...is either genetic or environmental, for *it is the expression of genes in a series of environments*. Genetics provides no means of investigating the inheritance of an invariant trait. Thus to postulate that it is genetic is to pose an untestable and meaningless hypothesis. The only question one can legitimately ask is, Is the trait highly canalized, or does it vary greatly under different environmental conditions, compared to other traits?

If certain behavioral traits, such as aggression, sibling rivalry, sex-role behavior, or intelligence were highly canalized, then, according to Futuyma, we would not expect them to be modifiable by environmental factors.

Environment

Contrary to most traditional conceptions of the environment, scientists who study the functional relationship between the behavior of organisms and environmental variables—behavior analysts—define *environment* functionally as all of the stimuli that enter into functional relationships with an organism's behavior at any one time (Schlinger, 1995). Behavior analysts view the environment as consisting of energy changes (stimuli) of various sorts that not only affect the sensory receptors of organisms but, more importantly, affect their behavior. Thus, the environment is not defined necessarily by its structure prior to the study of behavior, but rather after functional relations have been established by experimentation. In other words, behavior analysts define environment by how it functions to control behavior. The environmental history of an individual represents one category of ultimate behavioral causation; the other being the evolutionary history of the species to which the individual belongs.

Over the last 50 years, scientists who study learning have amassed volumes of testable, repeatable, experimental data demonstrating the powerful influence of environmental manipulations on a wide range of behaviors. Several scientific journals are devoted almost exclusively to direct experimentation on the effects of the environment. The *Journal of the Experimental Analysis of Behavior*, for example, has produced almost 40 years of data, including direct and systematic replication experiments. In none of these instances are data aggregated in order to achieve criteria of significance. In fact, in many experiments, little, if any, statistical analysis is needed to verify the reliability of the results. Internal validity is demonstrated time and time again by direct within-experiment refinement and control of objective independent variables. External validity of these findings has been consistently shown over the same 40-year period by successfully applying the scientific principles discovered in the experimental laboratory to problem human behavior. For example, the *Journal of Applied Behavior Analysis* has produced almost 30 years of experimental research on human behavioral problems, including compliance, crying, social interaction, cooperation, aggression, walking, reading and writing. Perhaps more convincing, numerous experiments have shown that behaviors previously thought to be impervious to environmental manipulation could be dramatically altered via operant conditioning, including psychotic behavior (Ayllon, 1963) mutism (Isaacs, Thomas, & Goldiamond, 1960), coma (Boyle & Greer, 1983; Fuller, 1949), and a wide range of physiological functions, such as diastolic and systolic blood pressure, Galvanic skin response, cardiac function, and asthma (Shapiro & Surwit, 1976), to mention a few. Moreover, the neurophysiological bases of basic learning processes have recently been uncovered, thus strengthening their status as scientific laws. For example, experimental evidence now shows that individual neurons



Nature-Nurture

Perhaps it would be appropriate to conclude with a word about nature-nurture, the phrase first coined by Galton. The issue of the nature or nurture of behavior is not as meaningless as some might suppose, as Dobzhansky asked (1964, p. 55): “To what extent are differences observed among people conditioned by the differences of their genotypes and by the differences between the environments in which people were born, grew and were brought up?”

The question about the genesis of a given behavior is an empirical question. The only truly scientific approach is to conduct experiments in an attempt to uncover functional relations between behavior and its determinants. The amount of data demonstrating the overwhelming effects of environment on behavior establishes the plausibility of environmental interpretations not only of behavioral similarities but also of behavioral differences between humans. Evolution has obviously played an important part in human behavior. But rather than selecting for behavioral rigidity, it has selected for behavioral plasticity (Dobzhansky, Ayala, Stebbins, & Valentine, 1977). As Futuyma concluded (p. 491):

On balance, the evidence for the modifiability of human behavior is so great that genetic constraints on our behavior hardly seem to exist. The dominant factor in recent human evolution has been the evolution of behavioral flexibility, the ability to learn and transmit culture.

Conclusion

The problem with evolutionary explanations of behavior is that the evidence proffered to support them is so fraught with methodological problems that it is simply insufficient to warrant any conclusions about the role of genes and, thus, evolution. In contrast, there is already a wealth of experimental evidence establishing the plausibility of an environmental/learning account of much human behavior. This is not to say that genes play no role in human behavioral differences or similarities, only that the jury is still out on the verdict regarding the extent and nature of that role. The only way to truly make a case for genetic influence on behavior is to control for environmental variables and manipulate genetic variables, which, at present, is simply not possible with humans. Finally, from a practical point of view, environmental explanations are more valuable than evolutionary ones because they suggest immediate ways in which behavior can be changed.

Evolutionary theorists certainly succeed in making an interesting and often compelling case that perhaps there is some deeper core of human nature that ties us all together and around which we as individuals, and maybe even as groups, differ. It is a case that appeals to many people, including the media, all of whom are hungry for some evidence that sheds light on our nature. Unfortunately, the case is replete with evidential problems, and will have to be retried if and when more substantial evidence can be obtained. Until then, we should rely on what we know scientifically about human behavior. □

can be operantly conditioned (Stein & Belluzzi, 1988; Stein, Xue, & Belluzzi, 1994). Such experiments demonstrate that the laws of operant conditioning discovered at the level of behavior-environment have their basis in neurophysiology.

Although volumes could be written summarizing the findings of the experimental science of behavior, suffice it to say that this is the only “cold-blooded verification” of theory that one should accept. Although not every human behavior that we find interesting can be subjected to experimental verification, a large corpus of experimental findings on basic learning processes is valuable in part because scientists can extrapolate from that foundation to novel behaviors. This is the essence of scientific interpretation (Palmer, 1991; Schlinger, 1995).

Some psychologists who espouse evolutionary theories of human behavior, however, cite non-experimental, and even non-quantitative approaches to the understanding of certain human behaviors as evidence against a behavior analytic interpretation. For example, Cosmides and Tooby (1987 and 1992) cite Chomsky extensively to make their argument that behaviorist approaches to language have been falsified and, therefore, cannot account for the acquisition of human language. Their conclusion is that evolutionarily adapted cognitive learning mechanisms constitute the only adequate explanation of human language acquisition. It is interesting that these citations consist solely of rationalist argument and not scientific experimentation and yet they are presented as if they are scientifically conclusive. Behavior analysts, in contrast, have not only provided substantive rebukes of Chomsky’s critique of behaviorist interpretations of language (MacCorquodale, 1970), but they have also argued persuasively that Chomsky’s own evolutionary account of language is untenable when held to Darwinian standards (Palmer, 1986; Dennett, 1995).

The susceptibility of human language to operant conditioning is no longer a debatable issue. During the past 50 years the operant control of verbal behavior has been demonstrated numerous times, including experiments on the operant conditioning of infant vocalizations (Poulson, 1983; Whitehurst, 1972), the content of conversation (Azrin, Holz, Ulrich, & Goldiamond, 1961), fluent requests (Rosenfeld & Baer, 1970), and grammatical forms, such as prepositional phrases (Lee, 1981) and plural morphemes (Guess, 1969). Experiments have also verified Skinner’s (1957) hypothesized functional verbal operants (see Oah & Dickinson, 1989 for a review). Moreover, behavior analytic principles have been used fruitfully to interpret a diverse group of studies on language development in infancy (Schlinger, 1995). The critical question regarding human language, or any complex human behavior for that matter, is whether plausible mechanisms or processes have been postulated. Operant learning principles constitute a plausible process both for verbal and nonverbal behavior, if for no other reason than they have already been shown experimentally to affect a wide range of human behaviors. Cognitive learning mechanisms, however, are not plausible in part because they are almost wholly inferred from the very behavior they are invoked to explain. Cognitive theorists cannot tell us what cognitive mechanisms look like or how they actually affect behavior.

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