

## WHAT'S WRONG WITH EVOLUTIONARY EXPLANATIONS OF HUMAN BEHAVIOR

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**ABSTRACT:** Recent books and articles have made strong claims for an evolutionary explanation of various human behaviors. These writings may be classified according to two positions on the evolution of human behavior. According to the first position, differences in behavior between groups of humans, (e.g., in intelligence) are said to be due to genetic differences. According to the second position, behavioral similarities among humans (e.g., in mating and aggression) are said to be due to genetic similarities. The evidence for these evolutionary explanations consists usually of a combination of evolutionary logic supported by general observations or correlations, behavioral analogies and comparisons with nonhuman species, and statistical analyses of data generated by non-experimental research methods. This paper presents a critique of this evidence and concludes that it is flawed scientifically and, therefore, it is insufficient to support an evolutionary explanation of human behavior. The paper concludes that environmental explanations represent an often more parsimonious alternative to evolutionary explanations, and, consequently, behavior analysts are in the best position to offer practical interventions for many of the behavioral problems that beset society. **KEYWORDS:** Human behavior, evolutionary theories; sociobiology, evolutionary psychology, behavior analysis.

In the past few years, there has been a flood of books and articles, by science journalists as well as academicians, offering evolutionary explanations for a variety of human characteristics, including intelligence, morality, mating, sexual preference, aggression, xenophobia, prejudice, and even our tendency to seek out various forms of nature, for example, by visiting zoos and national parks. These writings may be classified according to two related positions on the evolution of human behavior. The first is that human groups (i.e., races) differ behaviorally in certain ways because they possess different genes (Futuyma, 1979). This argument is manifested in the suggestion that the human species can be divided into distinct racial groups that are physically and behaviorally different because they are genetically different. The most popular example of this position is that human racial groups differ significantly in intelligence, at least as measured by standardized tests, because they have different genes (e.g., Herrnstein & Murray, 1994; Izkoff, 1994; Rushton, 1995a). A second position on the evolution of human behavior is that there are certain universal human

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behavioral traits that represent our common evolutionary history (Futuyma, 1979). This position resembles that of human sociobiologists during the 1970s and 1980s. Recently, however, the term "evolutionary psychology" has been used to refer to this approach to human nature (e.g., Cosmides & Tooby, 1987; Daly & Wilson, 1988; Symons, 1992; Tooby & Cosmides, 1989; Wright, 1994, 1995). Daly and Wilson (1988) suggest that the phrase "evolutionary psychology" be considered shorthand for theorizing about human psychology using modern evolutionary principles. The distinguishing feature of evolutionary psychology is that the object of study is not genetic differences between individuals or groups of humans, but rather genetic similarities—"species-typical mental adaptations" or "human nature" as Wright (1995) calls it. According to evolutionary psychologists, selection shapes not only our bodies, but our minds as well (Daly & Wilson, 1988). Evolutionary psychologists assume that there is a core of human nature that will emerge given environmental (mostly social) input that resembles that which was prevalent during human evolutionary history. The range of issues about human nature that evolutionary psychologists purport to explain is quite impressive indeed, including romance, love and sex; friendship and enmity; selfishness, self-sacrifice and guilt; social status; racism, xenophobia and war; deception and self-deception; sibling relationships; and parent-child relationships (Wright, 1994).

Both positions on the evolution of human behavior rely to varying degrees on a combination of three types of supporting evidence: (a) Evolutionary logic supported by casual observations or descriptive statistical data, (b) behavioral analogies and comparisons with nonhuman species, and (c) statistical analyses of data generated by non-experimental research methods. Each of these types of evidence, while 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). Data from the literature on learning shows overwhelmingly the powerful influence of the environment in shaping both human behavioral similarities and differences.

In the present article, 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, in most cases, a much more cautious and scientifically defensible position on the origin of much human behavior is that it is a direct function of individual environmental, and not evolutionary, history. A corollary is that many human behavioral problems considered to be genetically caused and, thus, relatively impervious to environmental change, can in fact be altered by arranging the appropriate environmental contingencies. I contend that behavior analysts are positioned to offer both alternative explanations and solutions for these behaviors.

### **Evolutionary Logic**

One important feature of the scientific method is the interpretation of phenomena

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that either have not been or cannot be 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 and/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 (i.e., races) have employed evolutionary logic to explain differences in intelligence (e.g., Herrnstein & Murray, 1994; Itzkoff, 1994; Rushton, 1995a), brain and head size (e.g., Rushton, 1995a) 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 parkland and easily develop phobias to natural objects, like snakes (Wilson, 1993). The data cited by these theorists consist of casual observation, personal reflection, and anecdote, as well as descriptive statistics derived from non-experimental studies. Let me illustrate with an example of the use of evolutionary logic from each of the two positions on the evolution of human behavior. Consider the biophilia hypothesis (Kellert & Wilson, 1993; Wilson, 1984) as an example of how evolutionary logic might be used to interpret some human behavioral similarities.

### **Evolutionary Logic and Behavioral Similarities**

E. O. Wilson says, "Biophilia, if it exists . . . is the innately emotional affiliation of human beings to other living organisms" (1993, p. 31). The biophilia hypothesis is, thus, an evolutionary explanation of human behaviors that may be said to value and affiliate with the natural world. Wilson says that even if there were no other supporting evidence, the existence of biophilia "would still be compelled by pure evolutionary logic" (1993, p. 32). Wilson offers the following casual observations in support of this logic: (a) more children and adults visit zoos than attend all major professional sports, (b) wealthy people build dwellings on or near water amidst parkland, and (c) urban dwellers frequently dream of snakes for reasons they cannot explain. Although a biophilia hypothesis may seem to be "compelled" by evolutionary logic, the validity of these observations is questionable. Moreover, there are other equally logical and more parsimonious explanations. For example, whether one attends a professional sporting event or goes to a zoo is surely heavily influenced by the culture in which one lives. And until such determinants of behavior are accounted for, evolutionary explanations should be offered cautiously.

It may be that the behavior of many people in our culture is reinforced by looking at untamed animals, whether in zoos and aquariums or on television. And

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it is not unreasonable to ask why this is so. A biophilia hypothesis would suggest that the tendency for some of our behaviors to be reinforced by seeing animals is an inherited behavioral trait. According to Wilson (1993), biophilia evolved as a function of a particular kind of gene-culture, or biocultural coevolution in which "the culture was elaborated under the influence of hereditary learning propensities while the genes prescribing the propensities were spread by natural selection in a cultural context" (p. 32). On the biological side, a certain genotype makes a particular behavior more likely, this behavior enhances reproductive fitness, and the genotype spreads through the population. On the cultural side, the human tendency to translate emotional feelings generated by this inherited genotype into dreams and myths sets the stage for art and religious belief. Consider Wilson's example, borrowed from biologist and art historian, Balaji Mundkur, of the human relation to snakes:

- 1. Poisonous snakes cause sickness and death in primates and other mammals throughout the world.**
- 2. Old World monkeys and apes generally combine a strong natural fear of snakes with fascination for these animals and the use of vocal communication, the latter including specialized sounds in a few species, all drawing attention of the group to the presence of snakes in the near vicinity. Thus alerted, the group follows the intruders until they leave.**
- 3. Human beings are genetically averse to snakes. They are quick to develop fear and even full-blown phobias with very little negative reinforcement. (Other phobic elements in the natural environment include dogs, spiders, closed spaces, running water, and heights. Few modern artifacts are as effective—even those most dangerous, such as guns, knives, automobiles, and electric wires.)**
- 4. In a manner true to their status as Old World primates, human beings too are fascinated by snakes. They pay admission to see captive specimens in zoos. They employ snakes profusely as metaphors and weave them into stories, myth, and religious symbolism. The serpent gods of cultures they have conceived all around the world are furthermore typically ambivalent. Often semihuman in form, they are poised to inflict vengeful death but also to bestow knowledge and power.**
- 5. People in diverse cultures dream more about serpents than about any other kind of animal, conjuring as they do a rich medley of dread and magical power. When shamans and religious prophets report such images, they invest them with mystery and symbolic authority. In what seems to be a logical consequence, serpents are also prominent agents in mythology and religion in a majority of cultures. (pp. 33-34)**

Wilson (1993) explains the biophilia hypothesis of the development of this relation between humans and snakes as the result of "constant exposure through evolutionary time to the malign influence of snakes, the repeated experience encoded by natural selection as a hereditary aversion and fascination, which in turn is manifested in the dreams and stories of evolving cultures" (p. 34). According to Wilson both the human fascination with snakes and the presence of serpents in cultural myths and individual dreams is a function of our genetic heritage.

Although the evolutionary logic used to support these assertions is interesting, there are several features of this analysis that should not go unchallenged. First, we should question whether there are grounds for assuming that animals whose behavior is described using the same terms are all doing the same thing (Kitcher, 1985). Stated more technically, we may ask whether phenotypically similar traits are genetically

homologous (Futuyma, 1979; Gould, 1981). For example, is the behavior of Old World primates to snakes anything more than superficially similar to that of humans? Second, we should question whether the evolutionary model is compared with actual behavior or, simply, "Are the conclusions of the model compared with precise observations" (Kitcher, 1985, p. 242)? For example, is it true, as Wilson (1993) suggests, that snake phobias are learned more easily than phobias to less "natural" events, or that snake phobias are relatively more prevalent in humans than other fears? Do many humans dream frequently about serpents, as Wilson (1984) claims? Are serpents prevalent in all human cultural myths?

It is logical, evolutionarily speaking, to assume that humans might have "developed" a genetic aversion to and fascination with an animal which, during human evolutionary development, was often harmful, if not lethal, to humans. Once again, We must ask whether there are accurate behavioral observations that support such an assumption. Although Wilson himself offers no supporting data, there are some twin studies which suggest heritability factors for animal fears (e.g., Rose, Miller, Pogue-Geile, & Cardwell, 1981; Torgersen, 1979). Some of these studies suffer from the same methodological problems that plague similar research. For example, Torgersen (1979) compared animal and social fears of monozygotic (MZ) and dizygotic (DZ) twins and showed that MZ twin partners were more similar than DZ twin partners in the strength of phobic fears, and that the MZ twin partners more often feared the same type of situations. The fears, however, were not assessed directly, but rather by a questionnaire. The results then had to be analyzed with a complex statistical test in order to make sense of them. Also, although the MZ twin and DZ twin pairs were raised together which should have controlled for environmental influences, there is always the possibility that MZ twins are treated more similarly by others than DZ twins (Scarr, 1968). There is also evidence that cultural transmission from parent to offspring may explain some of the variance in animal phobia measures (e.g., Davey, Forster, & Mayhew, 1993). And, finally, there is persuasive evidence that even Old-World monkeys do not show "a strong natural fear of snakes," as Balaji Mundkur claims, although such fearful behavior can be acquired rapidly through observational learning (Mineka, 1985).

Consider an alternative approach to the development of snake phobias. Psychologists know that the physiological and behavioral responses that are termed *phobias* can be produced by various complex experiences including Pavlovian conditioning and observational learning (Mineka, 1985). The result of each of these operations is that a previously neutral stimulus is altered so that it evokes both sympathetic autonomic responses and behaviors described collectively as fear. The question at hand is not whether humans can develop phobias to snakes or other objects, but whether these phobias are learned more easily or quickly than those to less "natural" events, such as cars or guns and, if so, then why. Let us assume that a child acquires a snake phobia after one experience with a snake or, better yet, after simply viewing the snake pit scene in the movie, *Raiders of the Lost Ark*. It might seem logical to assume that such rapid learning reflects a genetic predisposition, but there is another possible interpretation based on the principle of latent inhibition (Levis, 1979; Levis & Malloy, 1982).

The term *latent inhibition* (Lubow & Moore, 1959) is used when, in Pavlovian

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conditioning experiments, prior exposure to an otherwise neutral stimulus not in conjunction with a particular unconditional stimulus interferes with the ability of that neutral stimulus to then become a conditional stimulus after pairing with an unconditional stimulus. The implication of latent inhibition experiments is that novel stimuli, defined as those with which an organism has had little experience, are more likely to become conditional stimuli than familiar stimuli, defined as those with which an organism has had prior experience (Chance, 1994). For the present purpose, we can predict that a person would develop a phobia more rapidly to objects and events to which he or she has had either relatively less exposure or already established negative exposure. Such an interpretation might help to explain data showing faster conditioning, slower extinction and resistance to verbal instructions of fear-relevant stimuli such as snakes and spiders than of "neutral" stimuli such as flowers and mushrooms (e.g., Ohman & Soares, 1992; Soares & Ohman, 1993). In such experiments, the "neutral" stimuli may only be neutral as defined by the researchers. Most adults have a long, and usually positive, history with stimuli such as flowers, which might make fear conditioning to them slower to establish and quicker to extinguish. In contrast, most adults have a relatively shorter, and usually more negative, history with spiders and snakes. (In this context, a longer or shorter history means greater or fewer exposures to a stimulus.) The history with spiders and snakes includes not only observing others showing fear to them, but also having other people talk about those fears. Both of these experiences might endow the previously neutral function of the stimulus with CS-like (or EO- or S<sup>n</sup>-like) functions. Based on this analysis, we might expect snake phobias to develop more quickly and easily than phobias to cars or knives because for most people snakes are either relatively novel stimuli as defined above, or already established as negative.

Some authors have dismissed a latent inhibition interpretation of animal phobias in lieu of one in terms of preparedness (e.g., McNally, 1987). Mineka and Cook (1986), however, showed that a latent inhibition pre-treatment condition lessened somewhat snake fears acquired through modeling in rhesus monkeys, although not as much as an observational learning pre-treatment in which the monkeys observed a model explicitly not fearing a snake. Whether and how much such demonstrations can be generalized to humans remains to be seen. Certainly, humans have a richer observational learning history regarding animal fears than monkeys do. This is not to say that there is no possibility for a genetically based disposition to acquiring fears to certain objects or events, only that before jumping to such conclusions, scientists should look for simpler explanations.

People may also have a fascination with snakes and other "natural" phenomena, such as untamed animals, in the sense that they read and write stories about them, pay admission to see them in zoos, and go out of their way to watch them on television. This fascination may occur for reasons also related to their novelty. In other words, the novelty of these stimuli for many urban dwellers might make the sight of them more potent reinforcers than other, more familiar events in urban life. Putting wild animals in zoos and aquariums, for instance, might make the sight of them more interesting, and this might increase their reinforcing value. Finally, the behavior of looking at animals is probably also reinforced by seeing them move, which may reflect some genetic predisposition to look at things that spontaneously

move.

In the examples of biophilia offered by Wilson and his colleagues, evolutionary logic has been offered in manner designed to entice the educated reader and the media, but tied only loosely—if at all—to scientific principles of selection. Wilson (1993) makes an impassioned case for the conservation of biodiversity on our planet because he believes that humans have a genetic affiliation to and thus a need for it. As Wilson (1993) states, "the diversity of life has immense aesthetic and spiritual value" (pp. 38-39). Thus, according to Wilson, psychologists should with some urgency address the problem of what "will happen to the human psyche when such a defining part of the human evolutionary experience is diminished or erased" (p. 35). In his attempt to persuade the reader of the importance and urgency of these problems, Wilson appeals to evolutionary logic to support the biophilia hypothesis even though he admits such logic is poorly explored. While we may be sympathetic to Wilson's concerns for the earth and its diversity of species, the point is that the validity of the data he offers is questionable and, even if it were not, the evolutionary explanations may not be the best ones available. If environmental explanations are indeed more tenable, then the kinds of solutions Wilson desires may be more within reach in terms of current behavioral technologies.

### **Evolutionary Logic and Behavioral Differences**

Now consider an example of the use of evolutionary logic from the perspective of those who emphasize genetic differences between groups of humans. 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, Mongoloid people invest relatively more in the care of their offspring than Caucasoid people who invest relatively more in the care of their offspring than Negroid people. 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 interpretation concerns the reliability of the supporting data. For example, he provides a table of the relative ranking of races on

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diverse variables such as physical maturation rate, including ages 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). These data were generated by non-experimental research methods where average differences between groups were often very small. Moreover, there is no scientific evidence, other than these often low correlations, to support many of Rushton's assumptions, including his assumption that brain size within a species is functionally related to cognitive ability. Rushton often relies on statistical analyses of aggregated data (the problems of which I will address in a later section) to bolster his claim that small differences between groups are statistically 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 other 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, p. 476). He has simply guessed 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. Second, Rushton's 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 all 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 as it is on a few physical features such as skin color, hair form, and the epicanthal fold, is subjective (Futuyma, 1979). 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" (p. 4).

### **Cross-Species Analogies and Comparisons**

A second type of evidence frequently used to support evolutionary explanations of both human behavioral 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



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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 had indeed discovered a genetic basis for a behavior in a nonhuman animal, which as Kitcher (1985) suggests, may not be all that often, 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.

### **Cross-Species Comparisons of Behavioral Similarities**

Evolutionary psychologists, like their sociobiological predecessors, frequently employ cross-species analogies and comparisons to argue their case for the existence of universal human characteristics. Human and nonhuman comparisons are freely made in the area of mating behavior (e.g., Barash, 1977; Buss, 1994). For example, Buss (1994) says that, contrary to the widely held view that the pervasive patterns of conflict in human mating represent some failure on the part of the particular individuals or of the society, something deeper, more telling about human nature is involved when, for example, a man becomes angry at a woman who declines his advances, or when a wife feels frustrated at a husband who fails to help in the home. Of course, the "something deeper" Buss is referring to is an evolutionary heritage coded in our genes that has resulted from Darwinian principles of sexual selection.

In essence, Buss asserts that human mating behavior may be viewed as a set of sexual strategies that are adaptive solutions to mating problems: "Those in our evolutionary past who failed to mate successfully failed to become our ancestors" (Buss, 1994, p. 5). The sexual strategies can be classified according to the two key processes of sexual selection postulated by Darwin: Preferences for a mate and competition for a mate. Just as hunger mechanisms, such as taste buds that are sensitive to fat, have evolved to solve the problem of providing nutrients, and sweat glands and shivering mechanisms have evolved to solve the problem of extreme heat and cold, sexual strategies have evolved to solve mating problems (Buss, 1994).

In making his case for an evolutionary theory of human mating behavior, Buss (1994) employs several nonhuman analogies. One involves the mate preferences of the African village weaverbird. When the male weaverbird spots a female, he displays his nest by hanging himself upside down and vigorously flapping his wings. Sometimes the female approaches, enters the nest, and "inspects" the nesting materials. As she does so, the male sings from nearby. At any point in this elaborate pattern, the female may leave and perform the same behaviors with another male's nest. If a male's nest is rejected by several females, he will often tear it down and start over. According to Buss (1994)

**By exerting a preference for males who can build a superior nest, the female weaverbird solves the problems of protecting and provisioning her future chicks. Her preferences have evolved because they bestowed a reproductive advantage over other weaverbirds who had no preferences and who mated with any males who happened along" (p. 7).**

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Buss (1994) then writes, "Women, like weaverbirds, prefer men with desirable 'nests'" (p. 7). What he means is that in our own evolutionary history, women's preference for men who could commit to a long-term relationship would have been reproductively advantageous. If a woman chose a man who was "flighty, impulsive, or philandering," she would have to raise her children alone without benefit of the resources and protection of a man. Buss assumes that most women prefer a long-term stable mating relationship and that this preference is directly influenced by genes reflecting an evolutionary history in which such preferences, like those of female African weaverbirds, were reproductively advantageous.

Just as Buss compares women to African weaverbirds, he compares men to elephant seals in at least one important respect: They both compete to attract females. During mating season, male elephant seals battle with each other by using their sharp tusks. Although Buss does not offer a specific human analogy, it is certainly possible to find instances where human males fight over a female, even to the point where one party is injured or killed. This might represent an instance in which, as Buss (1994) suggests, certain sexual strategies are activated by particular cultural contexts, like the loss of a mate to another man. In addition to African village weaverbirds and elephant seals, Buss (1994) offers either explicit or implicit analogies between humans and a wide range of other insects and animals to demonstrate how mates are selected, attracted, retained, and replaced and to make the case that all of these mating strategies, including conflict between the sexes, represent the genetic heritage of all species.

Buss's analogies mirror those of earlier sociobiologists concerning sexually dimorphic behavior in humans (e.g., Barash, 1977); but there are several problems with them. One problem is that the similarity 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 choosing male African village weaverbirds by female birds is anything but superficially similar to the preference by some human women for men who can commit to a long term relationship? Even if scientists can agree that the behaviors are formally similar, it is not clear whether the behaviors are functionally similar; in other words, whether the same processes, in the present case, genes, are responsible for both. Thus, we should question whether a human female's preference for a man who can commit to a long-term relationship is the same as a female African village weaverbird's preference for a male who builds a good nest. We must consider the obvious environmental factors responsible for such behavior in women. Other than the interesting theorizing that superficially similar behaviors in different species may be functionally similar, Buss offers no scientific evidence that they are. As fiction such reports are interesting; as science, they are wanting.

Cross-species analogies, such as the ones offered by Buss (1994), 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. But we should not permit our intrigue to cloud our scientific judgment.

### **Cross-Species Comparisons of Behavioral Differences**

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 (cf. Gould, 1981) 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 makes it clear, however, that the correlations became more positive when the data from the studies were aggregated. (I will address the problems with aggregating correlational data in the next section.) 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 (see Hart and Risley, 1996 for persuasive data on the role of early verbal experience as a critical variable in both language skill and later intellectual performance).

### **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. 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

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structural than behavioral characteristics. Those who write about the genetic bases of human behaviors are typically not geneticists, however. Because they cannot make the genetic case experimentally, evolutionary theorists must rely on data generated by non-experimental, usually correlational, research methods.

### **Problems With Correlative Analyses**

#### *Validity and Reliability of the Data*

There are several problems with the ways in which some evolutionary theorists use correlative analysis. The first concerns the validity and reliability of the methods used to generate the raw data. 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, italics added). Obviously, 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, 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 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 Hartshorne 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 (e.g., home, school, church, etc.). Children's reputations with teachers and classmates were also obtained (presumably via questionnaire) and then correlated with the scores on the battery of tests. The only behavior measured by such tests, however, is that of answering questions on the test. The actual altruistic or honest behaviors were apparently 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. Only direct experimentation 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 intelligence, or 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, or their validity. 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

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a contrived context, namely, the test taking situation (Schlinger, 1992). Alfred Binet also 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 scientists is to ask about the variables that affect the broad range of behaviors called *intelligent*; and only an experimental analysis can answer such questions.

### *The Use of Statistics*

A second problem with the use of correlative analysis 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 analysis in making the argument for a genetic explanation of human behavior is underscored in the following quotation by Sir Francis Galton, which Rushton has twice cited (e.g., 1995a, 1995b):

**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. (1995b, p. 24)**

The most obvious problem with this quote and the approach to the study of individual differences that it fostered is the equation of statistical analysis with scientific practice. Although we may debate the role of 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. For instance, 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 latter quote is especially interesting because Herrnstein's own scientific output consisted almost exclusively of the use of within-subject experimental designs.

Authors such as Herrnstein and 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 (1994), 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,

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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 or 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. The two approaches lead to different strategies 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 in question. In this way, the reliability of the independent variables is enhanced and sources of variability are eliminated *before* measurements are made rather than afterwards, as is the case when researchers aggregate data.

### *Interpreting the Data*

Even if we assume that the methods for collecting and analyzing the data are valid and reliable, evolutionary interpretations are not the simplest or the most scientific ones possible. Thus, a third problem with the use of correlative analysis is the interpretation of the data. Demonstrating that a correlation exists between two or more variables does not in any way specify 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" (Neale & Liebert, 1973). Those who argue for an evolutionary explanation of human behavior appeal to the human genome as a third variable. Although it is theoretically possible that some human social and intellectual behaviors represent fixed expressions of the human genome, an alternative explanation is one in which a different third variable is implicated, namely, the environmental histories of the behaving individuals. In many of the examples cited by evolutionary theorists, any one or more of the multitudinous environmental variables found in the individual histories of the subjects studied may have produced the reported correlations. Just as behavioral similarities

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between individuals may reflect genotypical similarity, they may just as easily reflect environmental similarity. The correlational evidence offered by evolutionary theorists is simply insufficient to distinguish the biological from the environmental position (Futuyma, 1979). The challenge for scientists is to tease apart these possible determinants of human 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. Thus, Galton got it wrong. The "triumph of scientific men" (or women) 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 because they already believe that genetics plays a significant role in such human 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 (1979) has written:

**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. (p. 473)**

### Genes

I have referred to the 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 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

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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 with respect to the trait in question 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).

Finally, there is a broader principle of genetics that is often not fully appreciated by many social evolutionary theorists. As Futuyma (1979) has written:

**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? (p. 476)**

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

### **Implications for Cultural Practices**

Just as the books and articles espousing an evolutionary theory of human behavior follow from two arguments, so too do their conclusions. Recall that the two arguments are that (a) individuals and groups that differ behaviorally in some way (e.g., in intelligence) do so because of underlying genetic differences, and (b) invariant, universal human traits (e.g., morality, aggression) represent fixed expressions of the human genome. One conclusion following from the first argument is that a variety of cultural problems, including crime and poverty, are a direct function of an increasingly less intelligent population (Herrnstein & Murray, 1994; itzkoff, 1994). A corollary is that cultural practices designed to change intelligence, such as educational programs, have failed and will continue to fail (Jensen, 1969) because cognitive ability (intelligence) is no less than 40 percent and no more than 80 percent heritable (Herrnstein & Murray, 1994). Conclusions from the second argument are more sanguine: If there are universal human traits coded in our genome, then we should discover what they are and accept those aspects of our human nature, be they aggressiveness, homosexuality, morality, or xenophobia. Even



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if these conclusions are based on sound arguments or good science, which, as I have suggested, is questionable, the conclusions themselves do not follow logically from an understanding of genetics. For example, Futuyma (1979) points out that even if intelligence (IQ score) within populations were as much as 80 percent heritable, as Jensen (1969) argues it is, it does not follow that intelligence cannot be modified in a given environment. A trait can show high heritability in one environment, yet be modified greatly if the environment is changed:

**Because each genotype has a norm of reaction, a variety of phenotypes expressed under different environmental conditions, the heritability is not a fixed value; instead it depends on how greatly the environment varies. (Futuyma, 1979, p. 479)**

Thus, even if certain behaviors are influenced heavily by genes, such behaviors may still be modified by changing the environment. It is incumbent on behavior analytic scientists to offer alternative explanations for socially important behaviors and to make specific suggestions, based on laboratory findings, about how to best implement strategies to change the behaviors.

### **Rival Accounts: Behavior Analysis**

The point about alternative explanations raises the question of whether rival accounts have in fact been considered. Kitcher (1985) puts it bluntly:

**[i]f we are to resolve the 'major intellectual controversy of our generation,' then we want to know what variations in behavior could be produced given different social environments, (p. 243)**

In the present paper, I have offered alternative explanations for some of the behavioral examples used by evolutionary theorists to argue for an evolutionary explanation of human behavior. Of course, rival accounts of human behavior and differences in behavior between individuals is precisely the province of the science of behavior analysis. With the exception of B. F. Skinner, however, behavior analysts have generally failed to present their science to the public in the same way that other scientists have done for their sciences. For behavior analysts, alternative explanations of many of the human behaviors addressed by evolutionary theorists lie in the social and nonsocial environmental histories of individuals. A scientific understanding of such behavior would be better served if these more parsimonious explanations were investigated before evolutionary explanations are invoked. However, as I pointed out previously, evolutionary theorists already believe what the research they cite purportedly shows and, thus, they are not always interested in controlling for environmental variables, which can only be accomplished through experimentation. Behavior analysts, in contrast, already have a core of scientifically derived principles on which to base their analyses. Toward that end, behavior analysts must increase their efforts to bring an objective, scientific view of human behavior to the public. They can accomplish this by publicizing the findings of their science and using the established scientific principles of behavior-environment interaction to interpret human behavior deemed important by the culture. In the

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absence of such an effort, only evolutionary theories of human behavior and rival, but equally nonscientific cultural accounts, will be available, and the public will not have access to a truly natural science account of behavior.

Scientists have always wanted to tell the world about their discoveries, especially when those discoveries have had ramifications for cultural practices. Behavior analysts and other learning scientists have discovered much about how environmental variables affect human behavior. And many of those findings have been applied successfully to the amelioration of a variety of human behavioral problems. Unlike evolutionary theories of human behavior which rely on weak evolutionary logic, superficial behavioral analogies with nonhumans, or tests of questionable validity, the significance of which can only be revealed through statistical methods, environmental theories are logically sound, do not rely on analogical reasoning, and are based on direct experimental evidence. Publicizing these findings should at least ensure a more equal playing field in the popular press for a science of behavior. As Rushton has said, "the battle is over nothing less than how to conceptualize human nature" (1994a, p. 8). On this score he speaks the truth.

### Conclusion

Let me now return to the title of my paper: What's wrong with evolutionary explanations of behavior. In short, what is wrong with them is that the evidence proffered to support them is simply insufficient to warrant any conclusions about the direct role of genes and, thus, evolution, in human behavior. 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; however, the only way to truly make a case for genetic influences on behavior is to control for environmental variables and manipulate genetic variables. Finally, from a purely practical point of view, environmental explanations are more valuable than evolutionary ones because they suggest immediate ways in which behavior can be changed.

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