

# *Genes and Cultures—Boyd and Richerson*

## THE INTERTWINED ROLES OF GENES AND CULTURE IN HUMAN EVOLUTION

by *William Irons*

*Abstract.* This essay critiques dual-inheritance theory as presented in Peter Richerson and Robert Boyd's book *Not by Genes Alone: How Culture Transformed Human Evolution* (2005). The theory states that culture became prominent in human evolution because it allowed relatively rapid adaptation to changing environments by means of imitation. Imitating the behavior of other members of one's community produces adaptive behaviors more readily than either genetic evolution or individual learning. Imitation follows a number of patterns: imitating high-status individuals, imitating the most common forms of behavior, imitating behaviors perceived to be the most effective solutions to various problems relevant to survival. This process combined with occasional innovations in behavior lead to a process of cultural evolution involving populations of cultural variants. Different local human populations were associated with different local populations of cultural variants, and both the human and the cultural populations evolved over time. Human evolution cannot be understood without taking into account these parallel processes of genetic and cultural evolution. *Not by Genes Alone* traces the implication of dual-inheritance theory for understanding human evolution and refers to various bodies of evidence relevant to the theory.

*Keywords:* adaptation and maladaptation; cultural evolution; cultural group selection; dual inheritance theory

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The human species is a biological anomaly. Although humans are a product of evolution and are primates closely related to chimpanzees, bonobos,

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and the other great apes, they display a number of traits that are unique in the animal kingdom. Human beings have a wider geographic range than any other species, are ecologically dominant over most of the earth's land-mass, form vastly larger social groups and wider networks of cooperation than any other species, and have the unique traits of language and symbolic thought. Much of their behavior is shaped by culture—socially transmitted information—with the result that they behave very differently in different parts of the world even though they are remarkably similar genetically for a species with such a wide range.

Because they are so different, most social scientists and humanistic scholars assume that there is no point in trying to understand human behavior in the same ways that evolutionary biologists try to understand animal behavior. Humans and other animals are, in effect, separate universes, and each can be studied without reference to the other. This conventional wisdom was challenged in the 1970s by a number of biological scientists, most conspicuously Edward O. Wilson in his watershed book *Sociobiology: The New Synthesis* (1975). In the last chapter of this book Wilson suggested that human beings could be understood in terms of the same principle that governed animal behavior, principles he had presented in extensive detail in the earlier portion of his book. This book set off a heated controversy often referred to as the sociobiology debate. The central issue was the question, How unique is the human species? Are we so unique that scientists and humanistic scholars studying human beings can safely ignore biology and evolution, or can the principles that govern the evolution of other species be extended so as to shed light on human affairs? Some scientists, such as Wilson and Richard Alexander (1974; 1979) took the position that human behavior could be understood only if biological evolution were taken into account. Others argued that human beings, because of the massive influence of culture and because of their great behavioral flexibility, were unique, and it was useless to study their behavior in the same way that biologists studied animal behavior.

Richerson and Boyd published a book in 1985, *Culture and the Evolutionary Process*, in which they took a somewhat intermediate position between that of the so-called sociobiology camp and the human-behavior-is-decoupled-from-evolution camp. They labeled their perspective *dual-inheritance theory*, emphasizing that human beings have both a genetic and a cultural heritage. They argued that biological evolution has produced a unique capacity for culture in the human descent line and that culture itself began to evolve by natural selection. In fact they referred to the process as natural selection acting on cultural variants (what some call *memes*, using the label invented by Richard Dawkins). This process of natural selection acting on cultural variants then transformed the human species. Thus human biology and culture are inextricably intertwined, and neither can be understood without reference to the other. The 1985 book con-

tained extensive mathematical models that made it inaccessible to many potential readers, and the purpose of the more recent book is to present their ideas in terms that are easier for nonspecialists to understand as well as to update their thinking.

I found the first three chapters not very informative. They review a lot of well-known data to argue that culture really does exist and that it evolves. It is hard for me to believe that many serious scientists or other scholars need to be convinced of this. They cite a statement by Laura Betzig to the effect that culture is unimportant, but this is a maverick view by one scientist. They also cite statements by Alexander and David Buss to further buttress this idea that they are not demolishing a straw man in the first three chapters. Alexander and Buss argue that the processes of cultural transmission and change cannot be understood without reference to the biological nature of human beings. This is not the conventional wisdom of the social sciences, but it hardly amounts to saying that culture does not exist or does not evolve in the sense of changing over time. I seriously doubt that Alexander and Buss actually believe this. In fact I agree with Alexander's and Buss's statements, but this does not mean I need to be persuaded that culture exists and evolves.

The later chapters get down to the business of explaining Richerson and Boyd's ideas about the role of culture in human evolution and ways they think culture should be studied. This latter part of the book is very interesting in contrast to the first few chapters. Readers who already believe that culture exists and evolves could skim the first three chapters.

At the risk of oversimplifying, I would summarize the models of culture presented by Richerson and Boyd in this latter part as follows. Culture needs to be thought of as a population of cultural variants (memes or culture traits to some theorists) that are transmitted over the generations but also are subject to a process of natural selection by which some variants become more common than others. Human beings choose to imitate certain variants at the expense of others for a number of reasons. They tend to imitate the most common variants, which have a higher probability of being locally adaptive than less common variants. They tend to imitate more successful individuals in the population, and occasionally they exhibit content bias. That is, they understand the way one variant works in the local environment and can see that it is more adaptive than alternatives. The consequence is the development of cultural knowledge for dealing with local environments that consists of much more sophisticated cultural variants than a single individual could develop in a single lifetime. People also occasionally introduce new variants and, because of their intelligence and knowledge, produce innovations that, unlike genetic mutations, are not random but cluster near useful solutions to various problems. This process of intelligent innovation is called *guided variation*.

The authors use the kayak as an example. A kayak is a very effective tool for traveling over the sea in the Arctic in pursuit of seals. It represents the

cumulative knowledge of many generations of Inuit experimenting with locally available materials to improve on the design of earlier generations of kayaks. One can learn to build and use a kayak by imitation, but no single individual could invent one starting from scratch.

Why was this process of cultural adaptation favored in the human line but not in other descent lines? During the Pleistocene, environments fluctuated too rapidly for adaptation by genetic evolution but slowly enough for humans to adapt by the cumulative process of cultural evolution. Hence cultural evolution, the accumulation of more and more effective cultural variants, began. That humans entered these rapidly changing environments with large brains, flexible behavior, and sizeable social groups may also have helped to launch this process. Our early ancestors were preadapted for culture. Because of the nature of imitation, local populations tended to have a limited range of cultural variants, but populations of humans isolated from one another had distinct repertoires of cultural variants, and this set the stage for cultural group selection. Because most of the variation in culture traits was between local populations, natural selection on cultural variants took place mostly at the level of groups, that is, different populations of cultural variants. This in turn caused natural selection to favor genetic evolution for what Richerson and Boyd call the tribal instinct: a tendency to be cooperative within one's own group and to compete vigorously with members of other groups. Thus cultural group selection transformed human biology, making us more social.

Another dimension of their theory is the idea that, because of the nature of imitation, human cultures accumulate a large load of maladaptive traits. This is a cost of the ability to accumulate cultural improvements through guided variation and imitation. In contrast to contemporary behavioral ecologists, Richerson and Boyd's dual-inheritance models predict that culture will have a large number of maladaptive cultural variants.

Richerson and Boyd have given us a fascinating set of theories that deserve to be evaluated empirically. However, since 1985 relatively little has been done in the way of such evaluation. Much of the discussion in *Not by Genes Alone* is in terms of hypothetical examples or simplified models. More empirical work needs to be done. The authors do include ethnographic data and other data at various points in the book, but none of these data were gathered as part of research projects designed to directly test any of the authors' theories, and most of the data presented, although interesting, has a coarse-grained relationship to Richerson and Boyd's theories. They discuss current low fertility and the demographic transition extensively and argue that low fertility is maladaptive. I agree that it is maladaptive, but I think there is merit in the alternate explanation that it is a result of proximate mechanisms off track in a novel environment. Richerson and Boyd call this alternate theory the "big mistake" hypothesis, but behavioral ecologists and evolutionary psychologists see it as an example of mis-

match, failure of evolved adaptations to deal effectively with environmental novelty. Actually their claim that their theory predicts more maladaptation than behavioral ecology does is somewhat inexact, because behavioral ecologists also expect to find examples of mismatch in response to novel environments. Mismatch is a standard of evolutionary theory. They also expect that even without environmental novelty adaptations occasionally will fail to produce adaptive results. Adaptations are never perfect. In my opinion, Richerson and Boyd would have to find more long-established and more extensive mismatches to support their claim strongly. Also it is worth noting that the idea that current low fertility can be explained by mismatch has generated a lot of empirical research by human behavioral ecologists (see Mace 2000; Kaplan and Lancaster 2000; Low 2000; Luttbeg, Mulder, and Mangel 2000).

Richerson and Boyd argue that witchcraft, more ancient than extensive contraception, is another example of a maladaptive cultural variant. I found their argument here completely unpersuasive. Their argument is twofold. They present ethnographic evidence that a New Guinea group, the Gebusi, were on the verge of extinction because of extensive killing of witches within the group. This is a persuasive example of a maladaptive cultural situation, but the Gebusi are only one small group. The other argument is that witchcraft is generally maladaptive because it leads to the killing of innocent persons. I agree that belief in witchcraft, witchcraft accusations, and the killing of supposed witches are very undesirable. The issue they are addressing, however, is not social desirability by modern standards but adaptation. Social desirability and adaptation are very different things. As horrible as the killing of purported witches may be, it still may reflect a struggle for power and resources that is maladaptive for those killed but adaptive for those who do the killing. Socially just behavior and adaptive behavior are not always the same. The naturalistic fallacy is still a valid principle. I do not know what the final word on the adaptiveness or maladaptiveness of witchcraft will be, but arguing that it is generally maladaptive would need a much more detailed analysis that is open to the possibility that witchcraft beliefs are weapons of vicious competition between individuals and small kin groups that entail both winners and losers. The authors may be correct that there is more maladaptation in culture generally than behavioral ecologists believe, but their discussion of witchcraft does not come close to providing strong empirical support for this position. The extensive research on witchcraft by British social anthropologists strongly suggests, to me at least, that witchcraft beliefs and accusations are tools by which people seek to control the behavior of others and sometimes to eliminate rivals in social competition. Witchcraft accusations therefore are often adaptive for the accusers. (For good ethnographies of witchcraft in the British social anthropological tradition see Evans-Pritchard 1937; Middleton 1960.)

Richerson and Boyd mention several times that cooperation in large social groups is not possible on the basis of reciprocity, implying that only

their theory of cultural group selection, or some similar theory, can explain large human social groups. I am unconvinced for several reasons. First, game-theory studies of cooperation in large groups have offered some interesting and powerful models of large-scale cooperation among self-interested actors (see for example Nowak 2006, which contains references to much research published on this subject before the publication of Richerson and Boyd's *Not by Genes Alone* in 2005). Second, cohesion of large human social groups is achieved to some degree by coercion. Therefore, models that explain large-scale cooperation in humans need not rely on reciprocity alone. They cite research they did with Joseph Soltis on changes in the occurrence of named groups in highland New Guinea at different points in time as evidence that group selection is possible in traditional human social environments (Soltis, Boyd, and Richerson 1995). However, they have little to say about exactly which traits this pattern of group selection would favor.

A very similar situation exists among the Yanomamö of southern Venezuela, who have been thoroughly studied by Napoleon A. Chagnon. Among the Yanomamö, named groups disappear and new groups appear at a high rate. In this case the dynamics underlying this process by which some groups vanish and new ones appear are well described (see Chagnon 1974; 1997), and the process does not favor altruism by individuals toward the groups to which they belong (Chagnon 2008). The pressure caused by endemic warfare among Yanomamö villages was favoring cultural changes before the Yanomamö were overwhelmed by outside influence starting about 1980. The changes favored were creation of larger lineages by remembering deeper genealogies and the acceptance of village headmen with real authority including the authority to punish individuals who disrupt village cohesion (Chagnon 1974). The leaders pushing these changes were acting, according to Chagnon, with largely selfish motives, because these changes would enhance their own power. Less powerful individuals may not have gained from these changes, but their accepting them would be better explained as acquiescing through coercion rather than altruism (Chagnon 2008). It seems to me that the Yanomamö case would be one to examine in detail if one were addressing the question of possible group selection in traditional human social environments. In this case it is possible to identify the specific traits that are being favored by this form of group selection.

Richerson and Boyd (2005, 58–59) mention the tradition of studying cultural evolution pursued by Robert L. Carneiro, Allen W. Johnson, and Timothy Earle and immediately dismiss it as largely descriptive and not really about evolution. To me their dismissal is unjustified. It is true that they are very descriptive, but they do point to some empirically supported general long-term trends in cultural evolution. The transition from a forging culture to a modern industrial one certainly entails the creation of new

cultural variants and the disappearance of older ones on a grand scale. I fail to see how this is not cultural evolution as Richerson and Boyd define it. Carneiro (2003) discusses Richerson and Boyd's models of cultural evolution in comparison to his own with considerable profit. He suggests that Richerson and Boyd should pay more attention to content bias in studying cultural evolution, a suggestion I think is worth taking seriously. I would like to see Richerson and Boyd reciprocating by taking Carneiro's, Johnson's, and Earle's models seriously.

I have pointed to a number of problems with Richerson and Boyd's dual-inheritance theory of cultural evolution. Probably none of these criticisms will in the long run be fatal to their theory. The main problem, as I see it, is the lack of a serious attempt to subject these models to rigorous empirical testing. A good theory, in my opinion, should lead to new efforts to collect data specifically designed to test the theory, and so far Richerson, Boyd, and their coworkers have not done this on a large scale.

I recommend their book for readers who wish to get up-to-date on Richerson and Boyd's very interesting theoretical models, but not for those who wish to become aware of the full range of theories dealing with the evolution of culture and human behavior. A lot has happened since 1985. Theoretical models have been developed that have the potential for explaining the unique role of culture in human behavior. Richard Alexander (1987) developed the idea that indirect reciprocity can extend the role of reciprocal altruism and carry us a long way toward larger, more cohesive social groups. He framed his argument in terms of the origin of morality, but the argument can be easily extended to all of human culture. Later human behavioral ecologists, who are in effect somewhat modified intellectual descendants of human sociobiologists, have explored Thomas Schelling's commitment theory (Schelling 1960; Frank 1988) or its close analogue, the handicap theory of animal behavior developed by Amotz Zahavi (1975), to develop further theory about the role of culture in human life. Game theorists have proposed models of ways that self-interested individuals can develop strategies for large-scale cooperation. (As mentioned above, Nowak 2006 contains references to numerous relevant game-theory studies published before *Not by Genes Alone*.) Lee Cronk (1999) has suggested that human beings frequently use culture to manipulate other human beings, a possibility not entertained by Richerson and Boyd. Cronk also documents the fact that culture and behavior can be very inconsistent, as when the Mukogodo he studied say that they favor sons over daughters but in their behavior clearly favor daughters over sons. This suggests that the relationship between culture and behavior is more complex than Richerson and Boyd, and many cultural anthropologists, assume.

These alternative models also need more empirical evaluation, but many of them have been evaluated with observational data up to a point and do have some support. Such theories provide viable alternatives to Richerson

and Boyd's dual-inheritance theory. For readers interested in the total range of theoretical models and empirical research relevant to evolution and human behavior, I recommend Laland and Brown 2002.

How genes and cultural variants interact in human evolution is a complex question. At present we have many theories and no definitive answers. What is most needed, in my opinion, is good empirical research designed to evaluate the various theories of how this interaction works and how it has shaped human evolution. Richerson and Boyd's dual-inheritance theory should play a central role in this future process of empirical evaluation.

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