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Reprinted from *Human Reproductive Behaviour: A Darwinian Perspective* (eds. L. L. Betzig, M. Borgerhoff Mulder & P. W. Turke) © Cambridge University Press, 1987. Printed in Great Britain.

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Evolutionary approaches to human behavior: What does the future hold?

The important predictions in evolutionary biology derive from a concept of adaptation based on a selfish-gene model of natural selection and take this form: given that all organisms throughout their history have been subject to this kind of genetic bookkeeping, any given organism is expected to have certain properties and not have others. The properties are expected to form a closely-optimized strategy for the maximal proliferation of the genes that directed the development of the organism. No compromise with any other goal, like the survival of the species, is expected. Inspection of the organism will disclose whether it does or does not have the predicted properties.

Williams (1985: 12)

In the natural sciences a person is remembered for his best ideas; in the social sciences he is remembered for his worst.

Unknown

Introduction

At a small conference on evolutionary approaches to human behavior, at a midwestern university in 1981, a distinguished social anthropologist addressed himself explicitly to the graduate students in the audience. He advised them not to associate themselves with this 'fad' because, among other things, the anthropology panels of national granting agencies simply would not fund studies in this arena. The same man, although listed as a discussant, acknowledged that he had not attended any of the presentations and had not read any of the manuscripts. Similarly, at a conference that followed publication of George C. Williams' 1966 book, *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought*, a distinguished evolutionary biologist proclaimed Williams' volume to be 'a *bad book*!'

Two decades later, the biologist just mentioned would have liked to take credit himself for the basic idea in Williams' book – that natural selection is most effective at lower levels in the hierarchical organization of life. The social anthropologist, however, may not live long enough to experience a similar reversal. Not only is it easier to accept ideas about one's work that originate within one's own discipline, but biolo-

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gists are not as fearful as are social scientists that theoretical shifts conceal ideological goals or biases, or may promote them incidentally.

The ideas of Williams, and Hamilton's (1964) development of inclusive fitness theory, appeared mainly between 1957 and 1967 (see also Fisher 1958). By 1977, they had transformed evolutionary ecology, behavioral ecology, the study of social behavior, population genetics, and most aspects of evolutionary biology. After 20 years they are so thoroughly imbedded in biology as to be virtually unnoticed aspects of nearly every biologist's basic dogmas.

It will not be the same in the social sciences. In some human-oriented disciplines – such as sociology, economics, and political science – there is still only an occasional enthusiast, viewed by most of his colleagues as a more or less harmless oddity. In psychology there has been a little more activity, and only scattered hostility, the latter possibly because psychologists never thought evolution was very important to them, and still do not.

Anthropology is different. Anthropologists do a lot of describing, and they often do broad-scale comparative studies; so do biologists. Archaeological anthropologists try to reconstruct the long-term past from fragmentary evidence; so do biologists. Biological and physical anthropologists are necessarily concerned with organic or genetic evolution, as are biologists, though organic evolution has never been seen as central theory in anthropology because its relationship to culture has always been vague, and culture is the central theme of the long-term and broad-scale comparative studies of anthropologists.

Anthropologists long ago adopted the basic tenets of organic evolution from the discipline of biology and adapted and emphasized them according to their own needs. Biological anthropologists, for example, teach population genetics, although often with an emphasis on genetic drift that is surprising to biologists – because changes via drift carry little or no implication of better versus worse. The concept of adaptation as reproductive fitness has never been prominent in anthropology.

In social and cultural anthropology the term evolution took on its own special meaning, referring to patterns of cultural change, often without reference to mechanisms, or implications of genetic change. In the last several decades there have been assiduous efforts to steer clear of suggestions that contemporary cultural variations have anything to do with genetic variations among the peoples of the world. I think everyone senses the persisting tendency to turn any such opinions or possibilities to ends deleterious to groups with less power and influence by judging or characterizing them as less 'attractive' or 'inferior.'

In summary, anthropologists have believed that they understand evolution perfectly well, and that they had incorporated it correctly, and as far as they dared, into their discipline. This attitude, and concern about the use of evolution as ideology, have caused a hostility far more powerful and persistent than in most other social sciences, and far exceeding the alarm of those biologists who 20 years ago thought that they understood evolution perfectly well without the ideas developed by Williams and Hamilton. In my opinion, this hostility is a central deterrent to progress in the use of evolutionary theory to advance human self-understanding. Without it, more people in the human-oriented disciplines would be able to incorporate the ideas and findings of evolutionary biology into their thinking in a constructive way, without feeling that they necessarily have to cross an ideological and interdisciplinary fence, or alter their fundamental approach with respect to human behavior.

To the extent that the social sciences are indeed *sciences*, biases, fears, and interdisciplinary competitiveness have the potential for affecting evolutionary approaches to human behavior only on a short-term basis. Even temporary hostility, however, can debilitate the careers of young scientists.

What about the longer term? Can we accelerate the abatement of emotional rejections and unreasoned fears and hostility? If we assume that the hostility will pass, eventually, what then can we expect of the evolutionary approach? What would be the ideal culmination of its application? What is the importance of the papers in this volume? How do they measure up, scientifically? How do they differ from those appearing in earlier, similar volumes? I want to approach these questions by discussing, first, some of the reasons for disagreements and hostility between those utilizing evolutionary approaches and others who operate in the human-oriented disciplines.

The problem of interdisciplinary differences in goals and methods

Considerable confusion has occurred because the arena of evolution and human behavior, as with the discipline of anthropology, overlaps both science and the humanities. These two aspects of human endeavor differ strikingly in some respects, and when their practitioners argue directly with one another there is special perplexity because each group is unfamiliar with the other's methods and goals.

Scientists typically pursue topics within which there exist cores of factual discoveries – within which statements can be made that approach undeniability. The earth is round; night follows day; three species of American cicadas emerge from the ground every 17 years; secondary sex ratios in most mammals are slightly male-biased; etc. Moreover, the undeniable or factual parts of science are its centerpiece. All scientists work in reference to this centerpiece, theorizing and testing outward from it, and seeking to expand its base by using ideas and speculations and hypotheses to locate new facts. Perhaps because of the preoccupation of science with the undeniable, its method has come to embody the principle of testability or repeatability. The vertebrate paleontologist, George Gaylord Simpson, thus called science a self-correcting method of finding out about the universe.

Scientists, then, seeking to advance their own careers and in competition with one another, engage themselves in stripping away erroneous and imprecise structures, added to the core of factual knowledge by themselves or others, and replacing them with structures that, as they see it, are more nearly correct. They do this by hypothesizing and testing and arguing and criticizing. Because science is not ideal, its practitioners often try as well to dismantle perfectly valid aspects of the perceived factual centerpiece contributed by competitors. In science, though, the requirement of repeatability in method, and the adherence to topics for which undeniability or factuality is possible, mean that such overzealousness eventually will be exposed – by still other competitors engaged in advancing their own careers. The delay is often extensive, because of either lack of interest or the inertia of power differentials within the political structures of science; but one can be fairly sure that the next generation of scientists, competing among themselves, will locate many of the exaggerations and falsehoods of one's own generation and understand more clearly who got things straight and who did not, and who did it first.

One might say that the method of science, requiring repeatability and preoccupation with facts, amounts to an eventual *forcing* of agreement; anyone who denies established facts will lose his reputation and credibility if he cannot show *scientifically* that he is right – i.e. using means that are themselves repeatable and subject to falsification if that is possible.

Scientists who undertake evolutionary analysis of human behavior tend to suppose that those who approach the same subject from other perspectives – such as philosophy, theology, history, ethics, aesthetics, or other aspects of the humanities – have the same methods and goals as themselves. This is not the case. First, and most important, even though there are undeniable facts in the humanities, they are typically not the centerpiece. Quite the opposite, such disciplines are preoccupied with meaning, or value, and these concepts are interpreted individually – or at least not universally – and therefore lead toward diversity and disagreement rather than universality and undeniability. Practitioners of the humanities are concerned with what literature, art, music, aesthetics, religion – and even ethics and history sometimes – mean to *different* individuals or *different* groups. They are preoccupied with variations in interpretations, which arise out of differences in interests. The sciences literally avoid such topics, and so – at least until recently – have tended to stay out of the business of human behavior, particularly in realms of meaning and value, which are central to human social endeavors.

Whatever bodies of facts do underlie the activities of humanists, they are typically not regarded as the central issue. Even in history, which is often scientific, interpretations based on meanings or values specific to particular groups or individuals may be massively influential. While in science interpretations involving different meanings or values are seen as either irrelevant or as impediments to progress, in the humanities elucidating them may represent the principal goals. One of the reasons is that humans alone, among all of the objects in the universe available for study, change their behavior as a reaction to the *findings* of those analyzing them. Any human who knows this is virtually certain to alter what he says or thinks about human activities so as to cause responses in directions he himself desires; similarly, he may resist statements by others – factual or not – that he believes will alter human activities in ways contrary to his own interests. People are likely to suspect that those who analyze human activities choose their topics and adjust their methods and conclusions – consciously or not – so as to serve their own interests rather than those of others.

Controversies in science typically precede resolutions resulting from new facts or corrections of errors. This is possible because the continuing focus is on locating and establishing undeniable facts. Controversies in the humanities, on the other hand, may be endless because they concern the meaning of particular events or activities to different individuals or groups, whose interests in the matter may always differ.

The central aspect of scientific methodology is repeatability. The methodology of the humanities, on the other hand, may better be described as rhetoric (e.g. Raymond 1982). As a result, humanists who believe that scientists are entering the areas of meaning or value may be defensive, and scientists attacked by humanists may be perplexed or indignant. The humanist may believe that a scientist delving into human activities is trying wrongly to promote as universal some particular meaning, to destroy the notion of meaning or value as subject to interpretation, or to deny the meaning that particular humanist favors. Those with particular political beliefs, who also believe that scientists may be showing, or attempting to show, weaknesses in their ideology, may take up this kind of argument. The scientist attacked for such reasons, and by rhetoric as well, may in his turn see the attack as *ad hominem*, and, using the ethics of his own discipline, may regard it as reprehensible on that account.

Changes in the subject matter of the humanities are not cumulative in the way of changes in science (see also Smith 1984). They are better described as sequential changes in meaning, or in sophistication about meaning. They occur not necessarily because additional knowledge has been gained (even if it has), but because the social–cultural milieu has been altered.

Humanists typically are not chastised for out-and-out *errors* in criticism. What they write is typically not dissected into correct and incorrect aspects – into facts and errors. A competitor in the humanities is someone whose opinions, interpretations, or values differ from one's own. Attacking such a competitor – as, say, in a critique of a contribution to literature – is more likely to mean trying to destroy his credibility so that your own view will prevail. There may be no factual basis or criterion of repeatability to make one person wrong, another right. One wishes to substitute one's own interpretations, and one is also less likely to parade any core of accuracy that might exist in the other's viewpoint so as to build from it a cumulatively expanding factual structure. Humanists' careers are not so much ruined by errors, or even by evidence of deliberate misleading, as by having what they say be regarded as trivial or ridiculous, or irrelevant to most peoples' interests.

The critic or competitor, among humanists, packages the ideas of his competitors so they can be attacked as a unit, and in this and other ways personalizes his attack. Thus, he may speak of the 'programs' of particular individuals, or label endeavors headed by one or a few individuals and then set out to destroy the entire package so identified, in order to substitute his own, or just to show his devastating ability to identify triviality and explain generality in meaning. Because so much in the humanities depends on personal interpretations, the prestige that comes from this kind of demonstration is likely to be useful in the future. The method of destruction of a competitor's work or ideas, rather than including locating a core of 'truth' or correctness and building from it, is likely to involve identifying the weakest parts of the opponent's arguments and using them to argue that the entire package is useless. Leon Kamin, a social psychologist at Princeton, for example, writes in *Psychology Today* (1985) that the philosopher Philip Kitcher (1985), in his attack on Edward O. Wilson's 'early program,' 'Lumsden and Wilson's program,' 'Alexander's program,' and 'pop [read human] sociobiology' in general has shown, as Kitcher also claims, that 'the ladder of sociobiology is rotten at every rung.' Kitcher's methodology often fits the description of the humanist's approach given here as, for example, when he selects what he sees as the weakest 2 or 3 of a list of 25 predictions from Alexander's (1979) book and suggests that their weakness destroys not only all the others as well but the entire idea from which they arise. A scientist's approach would be to see, first, whether all of the predictions indeed arose from the same theory, then locate the strongest and 'riskiest' predictions and test them. As these were confirmed or rejected he might go on to test still others, but at some point in the procedure he would regard the underlying theory as provisionally either correct or incorrect. The weakest predictions would be of least, not most, interest to him. The reason is that the scientist is typically not *simply* trying to destroy the credibility of his competitor, or to use him to parade his own devastating wit or critical ability (although scientists often try to do these things along the way), but to get to the next line of investigation – to identify the next crucial hypothesis and test it (and to receive the credit due him for such accomplishments). Kitcher makes little effort to provide alternative theory, but in the spirit of the humanities simply criticizes.

One of the more interesting aspects of the current debate on the border between the sciences and the humanities, with evolutionary biologists on the 'science' side, is that a small group of scientists in acknowledged political agreement (Marxist) have adopted both the methodology and the language of the humanists. They attack the 'adaptationist's program' not by describing its strongest arguments and adjusting or building from them but by repeating its *weakest* supposed examples and tenets, always linked to the same people, whose names also label the 'programs' being attacked. Writings by others using the same theories – sometimes less assailable, and often less well publicized outside biology – are usually omitted entirely. This tactic appears as an extension of the idea of attacking the weakest arguments of a single author: Attack only the most vulnerable statements or works of the best publicized authors in a field easily represented as a package ('sociobiology') and imply by extension that everything else in the 'field' is wrong too. Let the everything elses remain unnoticed, if possible. It is ironic that Wilson's (1975) effort to promote the study of social behavior from an evolutionary viewpoint by calling it 'sociobiology,' and to promote his own views as the core of the 'discipline,' provided such critics with the ready-made, personalized package they needed. It is also ironic that Segerstrale (1986), in what she calls an '"in vivo" analysis of the sociobiology controversy,' becomes so embroiled in the opinions and reciprocal attacks of Wilson and Lewontin that she fails to cite a single one of the three authors (Hamilton 1964, Williams 1966, Trivers 1971) who provided the theoretical advances responsible for Wilson's (1975) book, hence the entire 'controversy.'

The problem of motivation

One of the reasons humanists and other nonbiologists resent probes by evolutionary biologists into human activities may be a feeling that by seeking generalities, or universals, biologists either detract from the humanists' central topics of meaning and value, with their flavor of diversity in interpretation, or else imply that any and all variations depend on genetic variation. Additionally, to ascribe universal goals to life effort, even in historical (evolutionary) terms, may imply things about motivation and consciousness, as well as about individual differences, that seem unacceptable to a humanist (or anyone!). Thus, to speak of a tendency or propensity produced by natural selection, or a lifetime shaped by evolution, seems to many to be genetic determinism that erases notions of free will, choice, and plasticity in behavior. To some it seems to justify evil or selfish acts as genetically determined or developmentally inescapable. This kind of skepticism leads such critics to deny even the possibility of an evolved human nature.

The concept of morality, which seems always to involve a core of mystery, can be used to illustrate this problem. Because morality involves conflicts of interest, it cannot easily be generalized into a universal despite virtually continual efforts by utilitarian philosophers to do that; morality does not derive its meaning from sets of universals or undeniable facts (Alexander 1987). A second, and considerably more troublesome source of mystery involves the question of motivation, hence consciousness or deliberateness. To serve one's own interests incidentally, or thwart those of another inadvertently, may not be judged immoral; to do either deliberately is likely to be so judged.

For example, the results of Buss (1985, 1987) suggest that human females are more concerned about the resources a potential mate controls than are males, while males are more concerned about various aspects of physical appearance and health in potential mates than are females. So long as such differences seem to depend on attitudes that are more or less unconscious and unfocused we view them with interest, and perhaps amusement. A little transformation into the conscious and deliberate, however, creates the images of 'golddigger' and 'male chauvinist.' In a sense those with an evolutionary approach, attempting to determine and bring into our conscious understanding the 'ultimate' goals of human lifetimes and their social activities, are threatening to turn something regarded as moral into something immoral; they threaten by suggesting that the forces which molded humans from non-humans would have caused us to be immoral if we had understood them all along. Such a change may well be seen as justifying immorality, or at least modelling humans as immoral, thus leaving us in painful doubt and thwarting manipulations toward greater group-cooperativeness that explicitly portray us as basically moral and unselfish.

Any vagueness, then, about the concept of 'motivation' in a discussion of morality as an evolved phenomenon is likely to be used by critics to support either accusations of genetic determinism or acceptance of immoral behavior as biological law. Unfortunately, such vagueness is likely to be present for a long time in efforts to explain how lifetimes (hence, somehow, motivation, consciousness, conscience, and all the rest) have been designed by evolution, whether or not the explainers are guilty of such implications (Alexander 1985, 1987).

This excursion, contrasting science and the humanities, is particularly relevant because most of the papers in this volume were written by anthropologists who regard themselves as scientists. But the members of the American Anthropological Assocation, at least, have long debated whether to call themselves scientists or humanists; and I suspect that much of the criticism of this volume will come from those who count themselves among the latter.

Is the evolutionary approach appropriate?

A particularly interesting case of resistance to evolutionary analyses of human behavior – perhaps including some confusion over the issues just discussed – is that of the evolutionary biologist, John Maynard Smith (1983, 1984, 1985). His (1985) review of the philosopher Philip Kitcher's (1985) critique of 'pop sociobiology' is instructive, and I will discuss both publications in some detail here.

Maynard Smith begins his review by referring (as did Kitcher) to 'efforts to apply biology to human affairs ... [that] ended up as justifications for racial, sexual, and class inequalities.' He says that Kitcher's and his own experiences have '... left us cautious about proposals to use biological theory to plan human institutions.' He adds that Kitcher is '... unsympathetic to the claim that evolutionary biology can guide political judgment ...' He suspects that Kitcher was 'unsympathetic before he started work on this book.' So do I. But this introduction is chaff thrown in the eyes of the reader: None of the three 'programs' Kitcher undertakes to demolish ventures into the realms whose history supposedly leave both Maynard Smith and Kitcher filled with trepidations, and neither Maynard Smith nor Kitcher has been able to accuse them of such. As with similar dire warnings by Gould and Lewontin (1979), Lewontin (1979), Kamin (1985), Lewontin *et al.* (1985), and other 'anti-sociobiologists,' this is a warmup to make the reader think he is about to hear something he is in fact not going to hear. All of this misleading nonsense uses the word 'biology' to mean 'genetic' or at least 'physiological,' rather than 'the discipline which studies life,' its virtually universal meaning outside philosophy and medicine and a few other disciplines (Alexander 1987). This particular brand of anti-sociobiology begins by trying to link modern evolutionary biologists either to 19th century racism or to more recent efforts to show that IQ differences are genetic, or both. No doubt Maynard Smith is correct when he says that 'Prejudices are inevitable.' But particular ways of using them are not therefore justified.

Maynard Smith assures us that Kitcher is authoritative and on the mark (at the end he will conclude that 'This is an admirable book') by telling us that 'Unlike some other authors he has undertaken a genuine study. He does understand the ideas he is criticizing. He has the biological background ... and the mathematical ability ... [and] above all he presents sociobiology in its strongest and most coherent form ...' Maynard Smith may be less convinced on the middle two points than he wishes us to believe, however, as he uses about 25% of his space to show that two of Kitcher's basic arguments are off-target: that hybrid inferiority and lowering of mean group fitness are principal reasons for the absence of optimality in evolutionary adaptation. The idea is to denigrate modern evolutionary approaches to adaptation. Maynard Smith notes that hybrid inferiority can scarcely explain absence of optimality in long and complexly-evolved traits involving many loci. He also points out that modern evolutionary biologists do not spend much time on the concept of mean group fitness. He could have mentioned that the people Kitcher attacks were in the first wave of those who adopted Williams' (1966) suggestion that adaptation is just better versus worse in the immediate situation. As Maynard Smith suggests, the investigators Kitcher attacks were also among the first to realize that mean group fitness is essentially an artifact of the arithmetic of population genetics, which for decades aided and abetted the misleading notion that selection acts primarily at the group level, despite Fisher's (1958) admonition that this was not what he was talking about in 1930.

Williams (1966) and Hamilton (1964) generated the entire approach that Kitcher criticizes, and a lack of potency in group selection was a fundamental assumption in that approach from the beginning. The idea of mean group fitness was for 30 or 40 years a source of considerable confusion to even the most astute biologists, and it is ironic that many people who think that evolutionary biology is not on solid ground until it is entirely underlaid with mathematical formulae still fail to realize that errors of this size and significance can appear and be perpetuated *because* of particular ways that mathematics may be used in biology. The mean fitness error gave rise to a misuse of the term 'genetic load' and led to what was called 'Haldane's Dilemma,' after an error of Maynard Smith's early mentor, the distinguished mathematical biologist, J. B. S. Haldane (see Brues 1964, 1969, Wallace 1968). The error arose (at least in part) because of the necessity (in mathematical equations) of assuming that the better genotype in a selective process always has a fitness of 1.0, the less fit genotype some fraction of this number. This necessity exists because otherwise numerical indicators of fitness will climb, causing equations dealing with selection to become impractical. Thus Haldane concluded that rapid evolution was too costly to occur – that it would lead to the extinction of whole populations. This result derived because introduction of beneficial mutants, at first rare, seemed to lower the 'mean fitness' of the population. Try it: multiply the fitness of a new mutant – 1.0 if it is beneficial – times the number of its copies and the fitness of the old allele being replaced (<1.0) times the number of its copies. You will see that the more beneficial the new allele is, and the rarer it is, the lower will be the mean fitness of the population after it is introduced. Now do the exercise with *several* beneficial new mutants, and you will understand Haldane's dilemma, and why Muller's (1950) concept of the 'genetic load' of mutants became perverted to include even beneficial alleles. Alice Brues (1964) first made this error plain and noted that, in this sense, evolving can never be more costly than not evolving.

At the end of his criticisms on these points Maynard Smith insists that Kitcher 'understands very well.' One wonders how he could tell. And one is again reminded of the old system, in natural history, of first establishing the authority of the person whose anecdotes you are about to espouse as the God's truth (Schneirla 1950). Maynard Smith is about to tell us that Kitcher's criticisms of those evolutionary biologists dealing with humans are on target – because, as Maynard Smith has repeatedly suggested in earlier papers (and to me personally), he cannot see how to explain humanity using evolutionary principles.

Maynard Smith and Kitcher both strongly support the evolutionary study of non-human organisms, and have virtually no criticisms of it. This is a kind of study that Maynard Smith himself has pursued for decades – in ways that Kitcher obviously approves of as much as Maynard Smith approves of Kitcher's criticisms of others. But both draw the line at applying the same approach to humans. Maynard Smith agrees that 'Man is an animal and has evolved by the same processes as other animals.' He says that 'The debate is between those who, while accepting that man is an animal, argue that he is such a peculiar animal that evolutionary biology can have little to say about his social behavior . . .' and those who think 'the study of human societies, just as of ant societies, must be rooted in biology.' The inclusion of 'just as of ant societies' in this statement is more chaff, designed to imply that anyone who thinks that humans can be analyzed using an evolutionary approach also thinks that humans are really just like ants. The use of 'biology' and 'rooted in biology' causes the reader to concentrate on 'genetic' rather than 'evolutionary.' The question is: If Kitcher and Maynard Smith think humans are exempt from an evolutionary approach because the species is so 'peculiar,' then what alternative would they suggest? Can they possibly believe that human social behavior – in all its peculiarity and uniqueness – came about as a result of forces alternative to evolution? What forces? How do any such forces *relate* to the evolution that produced humans? What approaches are permissible, and how do we decide? And since every species, not merely the human one, is unique, might not other species be so 'peculiar' as to be inaccessible via approaches that include the principles of organic evolution? If so, which ones, and how do we tell? (The British ecologist, David Lack, would have agreed with Kitcher and Maynard Smith, but as a devout Christian he had a ready alternative, and he invoked it – Lack 1965). I am particularly surprised at Maynard Smith here, because, in 1983, he reiterated (without reference) Conant's (1951) assertion that, in science, 'a theory is only overthrown by better theory, never merely by contradictory facts.' What is the theory alternative to an organic evolution guided chiefly by natural selection that might explain the peculiar human species? What theory does Kitcher (or any one else?) erect to replace an evolution guided largely by natural selection (see also Maynard Smith 1983, Williams 1985). (In the Maynard Smith article I cite here, it is his intent to demonstrate that evolution is guided chiefly by natural selection.) Is it some political prescription?

Maynard Smith agrees with Kitcher (and so do I) that there is 'no special underlying theory . . . no autonomous theory of the evolution of behaviour' – no need, one assumes they are both implying, for the term 'sociobiology.' While Maynard Smith allows that he does not like Kitcher's phrase 'pop sociobiology,' he does see the need for a label 'for the application of sociobiology to human beings, and I have no better one to offer.' One wonders not only why we need a special term for using evolution to study human behavior if we do not need one for using it to study the behavior of all other organisms, but also how obvious alternatives like 'human sociobiology' slipped past Maynard Smith, especially since he came so close to saying that particular one. But this would not be a derogatory term, and Kitcher's enterprise (or, as he and other philosophers – and Lewontin and Gould now, who have adopted such language – would term it, his 'program') seems to be to find weak spots in the evolutionary approach to human behavior and from them extrapolate (or try to get the reader to extrapolate) that the whole approach is useless, and even pernicious.

Although Maynard Smith concurs with the term 'pop sociobiology,' he argues that certain of the uses to which Kitcher puts it are 'unfair.' One senses that the scientist in Maynard Smith emerged at that point, causing uneasiness with an approach he had not yet quite put into perspective. Yet, he also makes the (to me) incredibly sweeping statement that he finds Edward O. Wilson's arguments 'generally ill-formulated and empty of content.'

But Maynard Smith is not quite through yet with the human (pop) sociobiologists. He appears to question the assumption of an 'unconscious relationship-calculator and fitness-maximizer influencing our conscious actions.' He actually lays the questioning on Kitcher and says that 'If I were Alexander [and were confronted with this questioning], I would reply that, if the claim is true, then it is up to psychologists to discover the mechanism.' But Maynard Smith's apparent reluctance in regard to such mechanisms – if I am correct in reading such a reluctance out of his statements here and elsewhere – is almost beyond credibility. All of his own work – for example, on evolutionarily stable strategies – demands the assumption of just such calculators in every species right across the animal and plant kingdoms. Apparently, Maynard Smith believes either that (1) any such calculators in humans would have to be conscious (since *some* things are conscious), and thus obvious, or that (2) humans either lost the ability to act reproductively or never possessed it. He says that the claim that 'people behave so as to maximize their inclusive fitness . . . is probably false,' and that he finds it 'hard to believe' that the things that '. . . sociobiologists . . . say . . . about real societies . . . are right.'

Kitcher contends that 'folk psychology' is a better predictor of human behavior than evolutionary theories based on inclusive-fitness-maximizing. He does not take up the question why it should be so. Nor does Maynard Smith, apparently, see any connection between this exercise of Kitcher's and his own rejection of the idea of sophisticated reproductive cost–benefit assessment by humans. I suggest that Kitcher's 'folk psychology' (which Betzig, pers. comm., describes as 'Kitcher's intuition') works as well as it does *because* humans have evolved to be reproductive cost–benefit analyzers. But conscious application of evolutionary hypotheses does better most of the time. Moreover, as those with evolutionary approaches refine their knowledge and their science, their hypotheses will improve in relation to 'folk psychology.'

No one doubts that rapid and dramatic technological and social changes cause humans to make many and important 'evolutionary mistakes' – i.e. to fail to maximize their inclusive fitness because the environment is novel. But this is a far cry from assuming that they are not *evolved* to maximize fitness, as evolutionary biologists claim. All through history non-human organisms have made the kinds of mistakes that Maynard Smith implies exempt humans from the evolutionary approach.

Some of the resistance to evolutionary approaches must represent honest fear of the effects of mistakes. Fear of mistakes, however, must associate with *all imperfect* efforts, and certainly all efforts to understand humans – not just those of evolutionary biologists – involve mistakes that have to be corrected. But I cannot agree with those who assert that evolutionists are *less* scientific than their predecessors in the social sciences. We are all being exhorted (appropriately) to develop testable hypotheses and test them, but the truth is that hypotheses of any shape or form – and testable hypotheses *a fortiori* – have always been virtually absent from some of the very subdisciplines of the social sciences from which the admonitions are coming. I return to this question later.

A third source of reticence, not to be taken lightly, is conflict with an accepted ideology. Perhaps all three sources of reservations exist in all people, one more important in some, another in others. One can only hope that they will all be diminished, dissipated, or used constructively, as our ability to incorporate evolutionary theory in the quest for self-understanding grows.

The infanticide controversy

Kitcher (1985) also criticizes Dickemann's (1979) analysis of infanticide. His argument is basically that of Harris (1979), though he does not cite Harris: 'Strictly speaking . . . daughters are an *economic* loss. How this translates into the "currency" of reproduction is a matter for investigation . . .' (p. 316). 'Whether female infanticide is a strategy that maximizes inclusive fitness for upper-class parents is an entirely open question' (p. 326). Kitcher's ultimate conclusion is the same as that of Harris '. . . the long-term interests of the genes are being sacrificed in the cause of status, influence, and perhaps money in the immediate future.' His view is that 'greed' runs the system, and that greed need not have anything to do with reproduction. (This view denies the significance of evolution but provides no workable or complete alternative theory.)

Like Dickemann and Harris, Kitcher suggests that high-ranking families use sons to consolidate and maintain their wealth and rank. No one has doubted this. In Dickemann's model this leads to more descendants, also of high rank and great wealth, via male offspring. Harris and Kitcher deny that there is any reason for expecting wealth and rank to be related to reproduction.

Kitcher discusses what he calls 'details' that cause it to be 'very difficult to discover an analysis that yields anything like Dickemann's conclusions.' Kitcher's 'details' seem to me to be entirely inadequate to the task he assigns them. First, he asserts that '*If nothing else changes . . .*' parents who kill their daughters decrease the total number of their offspring. Later he acknowledges that something does change – namely, women relieved of nursing ovulate sooner. This fact diminishes his point, but he tries to reinflate it by asking why women in Asia do not employ wet-nurses, as they did in Victorian England. By implication they were not trying to maximize reproductive success in the first place. Here he bypasses a traditional form of parsimony. Evolution of both genes and culture proceeds not from nothingness but from last year's model (some modern authors have started calling this 'phylogenetic inertia'). One cannot expect the best strategy imaginable to be in place for every organism everywhere. As Dawkins (1976) put it, no one expects pigs to have wings, even if they would be useful now and then (a main reason is that the stages required to get there may not themselves be adaptive). Moreover, wet-nurses are not likely to be free of charge, so Kitcher also cannot ignore the expense of providing them for daughters. The fact is that in these societies daughters are costly to high-ranking families in multiple ways, and they interfere with the production of sons. As far as I can see, no one had discredited this proposition.

Kitcher seems in this instance to have missed the significance of his own admonition (p. 328): 'The general moral is ... we need to trace the history of cultural institutions, recognizing how institutions affect the dispositions of those who grow up in societies dominated by them and how, in turn, those dispositions modify existing institutions. In tracing this history, we shall suppose that human beings have propensities that lead them, when they grow up in certain social and physical environments, to acquire as adults the desires and aspirations with which we are familiar.'

No one knows why wet-nurses were not employed in India, because we have not analyzed either history or the current situation well enough. Perhaps the intensity of desire for many children, well-known in India, is so great and so universal that it is difficult or impossible to get anyone to nurse another's child. Perhaps something else. Whatever the case, Kitcher's implication that admonitions such as his 'general moral' (above) are not a part of Dickemann's view of things is invidious, as this quote from Dickemann (1979: 327) shows: 'The ultimate test of the applicability of this model must depend, then, not only on a larger sample of more refined demographic data than I possess, but also on specification of the interaction between demographic structures and their ecological and historical contexts, something which is not attempted here.' If every study of human behavior by an evolutionist ended with such a caveat it would not denigrate the evolutionary approach.

Is the evolutionary approach flawed?

The evolutionary approach assumes that lifetimes evolve as reproductive events or sequences. This has been a basic tenet since Darwin (1859). Accordingly, we should consider Kitcher's (1985) admonition that 'It cannot be taken for granted that economic gains will translate into reproductive gains.' Harris' (1979) statements are similar. These statements could be revised to say: 'It cannot be taken for granted that access to resources correlates with reproductive success.' Arguments that rank or status has nothing to do with reproductive success, if correct, would render impotent the entire current evolutionary approach to human behavior. This argument (e.g. Vining 1986) is widely regarded as the strongest general attack on evolutionists' assumptions. I am interested here in how the assumption is generated and whether or not it is justified (see also the introduction to this volume, and Betzig 1986).

I will begin with the following statements, which I regard as general observations or accepted facts.

1. People do strive to control resources. That is, people seek wealth and material possessions; they seek access to the 'good things of life.' They also seek status, good reputation, and power, which they use to gain and maintain control of resources.
2. Non-humans also strive to control resources: food, waterholes, territories, nests, shelter, overwintering sites, mates.
3. Non-humans seem always to use resources in reproduction, except for failures from incomplete information (an animal didn't know that a particular food item was poisonous), miscalculations (an animal wrongly judged an opponent defeatable), or novel environments (as with moths around electric lights).
4. Humans, on the other hand, frequently do not use resources in reproduction, and moreover do not generally believe that reproduction is the sole significance of either resource control or the proximate mechanisms of resource control.

5. All organisms have evolved, including humans.
6. Because of the rapidity of change in human culture, humans have probably injected uniquely great amounts of novelty into their environments. Novelty can cause the proximate mechanisms of reproductive success (such as pleasure, resource control, or pleasure that leads to or results from resource control) to lead to activities that are not reproductive. When the proximate mechanisms of reproductive behavior are multilayered and complex, as they evidently are to a greater degree in humans than in any other species, this evolutionary 'mis-firing' can be extremely confusing and misleading.

Vining (1986) provides an extensive review of data indicating that 'fertility differentials among contemporary humans are not consistent' with the postulate that 'individuals exploit favorable environments to increase their genetic representation in the next generation.' He notes: 'That wealth, power and rank are ceaselessly and obsessively striven for, as well as monitored in others, is obvious to all observers of human behavior ... That modern humans exploit what they are able to obtain in the way of status and rank to produce, and to help relatives to produce, more offspring than those of lower rank and status seems clearly *not* to be the case. In fact, precisely the opposite appears to be true. The striving for, if not the actual possession of, status and power, seems, on average, to deter rather than to stimulate reproductive effort in humans.'

I expect that Vining is correct – at least sufficiently correct to establish the paradox he discusses – although I doubt that he or anyone else has shown that parents do not use their wealth, power, and rank (hereafter designated as WPR) to help and encourage their *children* to produce more offspring than others do (raising questions about his phrases 'helping relatives to produce' and 'in the next generation'). I would like to analyze Vining's finding with respect to background theory and intervening or proximate variables:

First, the significance of reproductive success in evolution does not depend on demonstrations of the relationship of reproductive success to any particular traits, but on irrefutable logic. A farmer I know, asked about the possibility that a young bull he wished to sell was sterile, hesitated, and then finally commented: 'Well – he comes from a long line of fertile ancestors.' None of us – no living creature – had a single sterile ancestor, right back to the beginning of life. This logic can be taken a step further. None of us had a single ancestor who was unable or unwilling to put forth whatever minimal effort was required for success in reproduction while it was alive – and that effort would have to include securing a mate, copulating with it, and also very likely giving some parental care.

Population genetics and probabilities enter the picture when we argue that those variants which leave the greatest numbers of successful descendants in any short run of generations are most likely to be represented (genetically) after a long run of generations. (Note that this model of natural selection is not exactly the same as Vining's.) Except in cases of actual reversals of environmental influences, this effect is probably expected by all modern biologists (I stress that an important consequence involves the nature of long-term cumulative changes in traits).

Therefore, when Vining and others tell us that data on reproductive success indicate that widespread or universal traits – such as striving for WPR – correlate negatively with reproductive success (RS), biologists are not likely to abandon the postulates that (1) evolution is universal and inevitable in all forms of life, (2) natural selection is its principal guiding force, or (3) natural selection leads to the accumulation of genes that contribute to traits yielding higher reproductive success. Rather, biologists are likely to ask: (1) Have certain features of the human environment reversed, fracturing the correlation between RS and WPR? And (2) which particular combinations of human traits have interacted with features of the (especially social) environment, and how, to effect such reversals? If we are somewhat like moths flying non-reproductively or anti-reproductively around electric lights, then precisely how and why did it come about? How much must we understand about human behavior to comprehend how and why the unexpected happened? Will we be able to use this new comprehension to extend our self-understanding? Vining implies not by saying that sociobiology has little to offer sociology because of a negative association between resource acquisition and reproductive success in contemporary society and that sociobiology must, for the moment, confine itself to the study of 'constants of human nature ...' I answer the last two questions affirmatively, and I disagree with Vining, as I expect do most or all of those concerned with incorporating evolutionary theory into our efforts at self-understanding. (See also, Williams 1985, who notes that we are not currently testing Darwin's theory but testing how it applies. We do not abandon the basic paradigm after one or a few tests fail, but seek reasons for the failure.)

Wealth, power, and rank are outcomes that must be striven for consciously – perhaps in ways that utilize all of our most distinctive human mental attributes (intelligence, consciousness, foresight, self-awareness, free will, memory, conscience, morality, and so forth). Indeed, socially competitive striving within and between competitive groups is likely the context in which we evolved our human attributes (Alexander 1979, 1987).

Contrarily, it is not necessary (typically) to strive (intelligently, consciously, deliberately) for children; rather, it is necessary to strive in these fashions to *avoid* them (as an aspect of spacing them optimally, toward the end of maximizing RS or any other goal). In other words, striving for pleasure typically leads automatically to the production of children *unless conscious, deliberate, and sometimes intelligent steps are taken to prevent it.*

In the course of diverging from other primates, humans evidently evolved toward increased parental investment (PI), meaning that they have benefited from spacing births increasingly widely and from giving more PI to each offspring. The basic evidence for this assumption is that human offspring seem to require and receive more PI – and to receive it across a longer period – than the offspring of other primates. As Darwin noted, each organism's resources are finite, and what a parent gives to one offspring it cannot give to another. The human kind and degree of social striving for WPR is usually seen – and I believe accurately so – as a heritage of this trend toward increased PI.

In social striving for WPR, the hostile forces – the environments of success or failure – are composed primarily of other humans. What is involved is an evolutionary unending race to see who can best use his consciousness, purpose, intelligence, etc. to secure resources. Because the major or sole competitors are conspecific – for humans in ways that may be unique – they will always be no more than a jump behind, *no matter what extremes are achieved during the race* (other species are less likely to be principal hostile forces than for non-human forms with less overall control over their environments). The same unique human attributes listed above, the striving in which they are used, and the resources gained by their use provide WPR to offspring, grand-offspring, and sometimes collateral relatives. Vining does not claim that persons with more WPR do not use them to *aid* the offspring and grandchildren that are produced – only that they are not used to *produce* more. Descendants of people with WPR typically do themselves also have WPR, and last wills and testaments tend to leave WPR to relatives in ways that closely approximate inclusive-fitness-maximizing (IFM).

Except for a small proportion of prospective parents who discover to their dismay that the usual activities of wedded bliss do not yield offspring, *production* of children, then, is typically not striven for in the conscious, deliberate, intelligent sense. What is striven for is the pleasure of sex, or wedded bliss, or orgasms, or sexual harmony, or emotional intimacy, or long-term bonds. These somewhat less intelligent, less uniquely human kinds of striving are generally sufficient not only to produce children, but, I suggest, to produce more children than, during most of history, an ordinary pair of spouses could save or make reproductively successful. Everyone may use uniquely human attributes in efforts to secure and keep desirable mates, but this process primarily contributes to PI – i.e. is part of the effort to make offspring successful, and not specifically an effort to produce them.

It is expensive to conceive babies and then lose them, either as infants or later. Where the environment has been predictably spare across millenia, then, we expect physiological traits or not-so-conscious behaviors to lead to wide spacing of babies, and some evidence supports the expectation (Konnor and Worthman 1980). But where the environment may vary dramatically, we might expect those unique attributes of intelligence, consciousness, and foresight to come into play when wider spacing is called for, and to recede in favor of letting nature take its course when times are better.

So we have a circumstance in which intelligence, consciousness, and purpose are brought into play to (1) gain WPR, (2) increase the interval between babies, so as to increase PI for each baby, and (3) provide and distribute resources in the interests of one's offspring, other descendants, and collateral relatives. The same attributes are not being stressed in *producing* babies.

Now we bring on the scene three major changes: (1) more and better resources for use in PI than were ever available before, (2) partly coincidentally, more pleasure outlets (and perhaps better ones), and (3) easier and cheaper ways to prevent babies while yet enjoying the glories of sex.

We might expect the initial result to be an increase in the birth rate because of relaxation of concern for spacing babies widely enough to ensure their survival. Whether or not this increase would persist would depend on (1) how important *relative* amounts of WPR are to descendants, (2) pleasurable uses of WPR (such as sexual access to more desirable, and perhaps more, partners), and (3) how much striving is necessary to prevent offspring while indulging in the pleasurable activities that would normally lead to babies.

If humans assessed the probability of raising babies to be successful in terms of *absolute* resources available, then, as technology and agriculture increased available resources, we would expect birth rates to rise because mated pairs relaxed their efforts to space babies widely. If, on the other hand, offspring success is largely determined by what is available to them relative to the offspring of others (for example, if success depends on WPR, which is relative and dependent on social competition), then something different might happen. Parents might assess the advisability of producing offspring by not the *absolute* amount of available resources but the *relative* amounts of those resources they control, or are likely to control in the relevant future, especially compared to others of about the same ages and stages (see also Turke, unpublished data). Thus, in a time and place of relative affluence, but in which the likelihood of securing property, jobs, and other resources comparable to the best or even most of society are diminishing, efforts to avoid children might be enhanced.

More than this, if certain kinds of pleasures come to accord with success in efforts to secure WPR – as we would expect over long terms in any species in which WPR correlate with RS – then the pleasures may be sought as their own reward so long as reproduction ensues and its success is roughly proportional to success in attaining WPR. Striving for WPR thus becomes an alternative to baby production, or comes into conflict with it, when baby production conflicts with baby success. This will be true for nearly all individuals early in sexual maturity – for varying periods depending on circumstances – so that tendencies and abilities to restrict or avoid baby production may be expected universally in humans at some times in their lives or under some circumstances. Given this circumstance dramatic increases in pleasure possibilities associated with WPR – especially when coincident with novel and easy methods of baby prevention – may be expected to reduce baby production in ways contrary to IFM, and when baby production and/or rearing conflict with maximizing pleasure from novel availability of phenotypic indulgence.

To say it again, if sufficient pleasure derives from WPR, if babies interfere with this pleasure, if we are given easy ways to prevent babies, and if judgments about optimal amounts of PI depend on how much neighbors and associates are giving to their offspring, then the effect Vining has identified may be precisely the one that would be predicted in modern affluent humans, from biological evolutionary theory.

I would not be surprised if all of the above conditions are met in contemporary society, and are largely responsible for the steps in what has been called the 'demographic transition.' Even if this is not so, generating hypotheses of this kind about variables that intervene between Darwinian predictions and reproductive or genetic survival outcomes, and figuring out ways to test them, is the appropriate route for biologists and other scientists interested in explaining the kind of paradox that Vining has reviewed and developed.

Two conclusions follow: First, investigators studying non-human species assume, with validity, that striving for resource control represents striving for reproductive success. Second, investigators studying humans can also assume, with validity, that striving for resource control by humans represents striving for reproductive success – at least in terms of the history of human environments, whether or not such striving actually correlates with reproductive success now. This second conclusion is acceptable, I believe, unless and until one or more of the following becomes true:

1. Humans are shown not to have evolved by the same rules as other organisms.
2. Evolution is shown not be guided principally by natural selection.
3. Human environments are shown not to involve massive novelty of the sort leading to disruption of the connection between resources control and reproductive success.
4. Investigations on humans repeatedly lead to results that do not make sense in light of the above reasoning or from the conclusion that striving for resource control in humans represents striving for reproductive success in confusingly novel environments.

Skeptics like Kitcher, Harris, and Vining would apparently require that every scientist who wishes to apply evolutionary theory to humans must first prove all over again that it is valid, specifically when applied to humans. If they mean, by this kind of parsimony, that failure to elucidate intervening variables can create a kind of selectionist optimism that expects pigs to fly, even without wings, then I agree completely. For example, showing that high-ranking parents exert more parental effort with sons and low-ranking parents more with daughters (e.g. Dickemann 1979, Betzig and Turke 1986) is provocative and positive support for a Trivers–Willard (1973) interpretation (i.e. that in polygynous species, high-ranking or unusually healthy parents can gain reproductively by favoring males), and for any non-human species it would probably be accepted as a satisfactory conclusion. With humans, we are more interested in how they come to do it, what they think about why they do it, and whether it is really true. Anything humans do can also become a model for future behavior, whether or not one openly subscribes to any version of the naturalistic fallacy. None of these problems makes Dickemann's or Betzig and Turke's kinds of findings any less scientific than the same results with a non-human species. They only make it less likely that the interpretation is precisely correct, and also make us more concerned that we understand the situation correctly.

Are evolutionists who study human behavior unscientific?

Skeptics of evolutionary approaches to human behavior often argue that investigators adopting this approach do not follow scientific procedures: They are said to utilize unacceptable forms of '*ad hoc*' hypothesizing, and to tell 'just-so' stories.

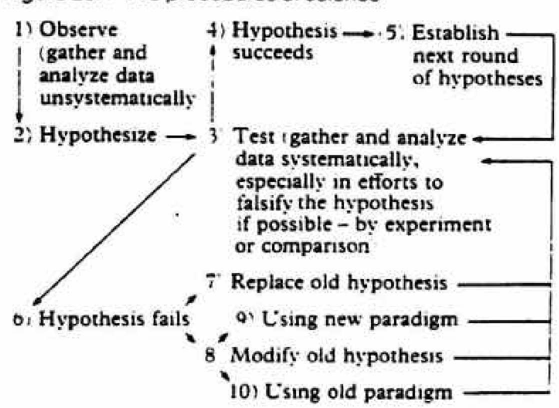
I have diagrammed the procedures of science in Figure 20.1. To what degree do evolutionists in general, and the contributors to this volume in particular, operate outside such procedures?

The ideal sequence in the diagram in Figure 20.1 is probably (1) (2) (3) (4) (5) (3) (4) (5) etc., with publication at (4). Also ideally, each bout of hypothesizing and testing involves all reasonable hypotheses matched against one another, and explicit efforts to falsify each. Also ideally, steps (8) (9) (10) will involve some additional data-gathering after (6).

Step (10), using the old paradigm, is only valid as a test when the test (3) cannot be considered a test of the paradigm itself. If several or numerous tests within a particular paradigm fail, then step (10) becomes less reasonable and step (9) is indicated.

Many 'theoretical' papers involve publication at step (2), or, at least, publication without specific tests using data gathered for the purpose. Data gathered for other purposes can be used, if the original purpose was unlikely to bias with respect to the hypothesis being tested. Sometimes tests using 'logic' are merely tests using

Figure 20.1 The procedures of science



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well-established facts (data gathered for other purposes and for one reason or another regarded as correct). For example, in a recent discussion about how it can be demonstrated that natural selection is the principal guiding force of evolution, a philosopher challenged the value of my statement (Alexander 1979) that in living forms changing the direction of selection seems always to change the direction of change. I regarded this apparent fact as a strong test because it seems to deny the role of principal guiding force to two of the three possible candidates (drift and mutations) while simultaneously supporting the third (selection). It is thus a 'test' based on already gathered 'data.' Once stated it can be made stronger, of course (or falsified), by checking in the future to see if it always holds true, and if not whether or not there is a pattern to the exceptions that explicitly denies the application.

'*Ad hoc*' hypothesizing appears to be an accusation levelled at people who employ step (8), especially going through step (10). In evolutionary studies, the aim of critics of this procedure often appears to be denigration of the entire evolutionary paradigm. As Williams (1985) put it, these critics are the kind of people who, if their car or watch stopped running, might suppose that there is something wrong with the basic laws of physics. Contrary to such critics, '*ad hoc* hypothesizing' is how science has always worked. Williams (1985) notes that some critics have the 'mistaken view that predictions are tested to check on the truth of general theory, when they are rather being tested to check on the truth of a particular understanding of the phenomena involved.'

Evolution is a simple theory. Applying it to understand life, however – which is the most complicated phenomenon we have yet encountered in the universe – is one of the most difficult tasks before us. We must not be deterred by those whose skepticism or rejection rests on a refusal to school themselves in the intricacies of biological science, and who, as a result, interpret every difficulty as evidence of failure of the basic theory.

Evolutionary studies of human behavior often appear in the sequences 6 → 8 → 9 → or 6 → 8 → 10 (Figure 20.1) because investigation of human behavior is probably the most difficult procedure in the universe to make scientific. In no other scientific enterprise do we use the attributes we wish to study to do the study, and study an organism designed to deceive us. Moreover, when we study humans, we study the most extremely complex item yet known to us.

Ways of verifying (testing) hypotheses

The goal in science is to identify hypotheses and demonstrate their correctness. The manner of testing hypotheses is the crux of the matter in determining the value of conclusions. The three general methods by which one can do this amount to supporting what in the end remains as the most likely correct hypothesis. The three procedures, from least effective to most, are these:

1. Locate data that agree with the hypothesis. The weaknesses of this approach are that (a) the data may also agree with other unmentioned hypotheses and (b) no direct effort is being made to identify the Achilles heel of the hypothesis. When alternative procedures are difficult, however, supporting data can have some (even if trivial) significance: When I began a list of general statements about human kinship systems (Alexander 1977), I argued that, even if each alone is trivial, and even if some are not really predictions anymore, to discover that, say, 100 such generalizations (or principles) support an explanation from natural selection would not be trivial (e.g. Alexander 1979).

2. Attempt to list all alternative hypotheses and seek to support them, eventually choosing the one that seems best supported. This method, employed by Hoogland and Sherman, both together (1976) and separately (1977), has certain weaknesses and has been criticized by most philosophers of science, including Ghiselin (1969). First, it tends to lead to the false impression that all reasonable hypotheses have been considered, when this end may never be accomplished. Second, it leads to the use of 'straw men,' either deliberately or unconsciously, and to a less than rigorous test on this account. Indeed, almost by definition, as with the first procedure (above), it amounts to supporting one's consciously or unconsciously favorite hypothesis. Third, it fails to focus on the most

likely hypothesis and to explore its specific weaknesses. Finally, it is usually a tedious and expensive method of locating the best explanation for a given phenomenon, because it fails to capitalize upon what is already known about the relative merits of existing hypotheses, even sometimes including the pretense that from the start all are more or less equally acceptable. The weakness of this approach is also shown by considering the case in which there is but a single hypothesis: Presumably, all one could then do is to dream up alternatives, however weak, against which to 'test' it.

3. Explicitly seek to falsify the hypothesis. This method is the most widely used and respected in science. It is commonly called the Popperian approach (Popper 1963), but Darwin employed it repeatedly, explicitly, and generally (see Alexander 1979). Its chief virtues are that it seeks the most vulnerable requirements of the proposition and exploits them to the utmost, and it does not require the pretension that all alternative hypotheses have been erected and tested. As Popper said, the most 'risky' tests are the best. Darwin (1859) sought the worst cases known from nature and postulated situations that *if* they existed would falsify evolution by natural selection. He knew that, if his hypothesis was correct, it should explain all *observable* cases but not all *imaginable* ones (Alexander 1979).

In earlier papers (Alexander 1974, 1977, 1979) I analyzed the avunculate and the asymmetrical treatment of cross and parallel cousins to falsify the hypothesis that *these practices show* that evolution is not relevant to the patterning of human sociality. I did this by generating and supporting a reasonable hypothesis from natural selection to meet explicit and particular objections denying the role of 'biology' (i.e. relatedness, and a history of natural selection) in the structuring of kinship systems (e.g. White 1947, Schneider 1961, Sahlins 1976).

Then I supported the selection hypothesis (both the broad one and its specific subsets) by showing supportive correlations from published data for both the avunculate and the treatment of cross and parallel cousins (additional data provided by Kurland 1979, Gaulin and Schlegel 1980, and Flinn 1981). Finally I added support for the general selective hypothesis by showing that genetic asymmetries also coincide with asymmetrical cousin treatment, predicting successfully the societies in which each kind of cousin marriage is likely to be stressed, and the frequencies and distribution of all four kinds of cousin marriages, and tying together the avunculate and cousin marriages in cases where confidence of paternity is not low. The avunculate and asymmetry in cousin marriages are outstanding complexities of human kinship systems in general, and no explanation for them with any generality of predictiveness had previously been proposed. In effect, I falsified the hypothesis that these cases show that natural selection is inconsistent with the patterns of human sociality. I did not either suggest that low confidence of paternity provides explanation for all cases of either the avunculate or cousin treatment or claim that the suggested explanations were indeed the correct ones. Others and I have continued to seek (and to find) additional factors involved in both phenomena (Alexander 1977, 1979, Kurland 1979, Hartung 1976, Gaulin and Schlegel 1980, Flinn 1981, Flinn and Low 1986).

Recently, another question involving the avunculate was posed by Laura Betzig and Paul Turke (pers. comm.). Why do chiefs on Pacific Islands, in societies with uxorilocality, and evident high confidence of paternity, pass chiefship to their sister's sons rather than to their own? The answer appears to involve the necessity of passing chiefship to a male who is not only a close relative of the chief but also a successor who will be logical and acceptable to those over whom he will be chief. Because men live in the 'clan territories' of their wives (chiefs do not live with their 'subjects'), their sons do not grow up in the region which will require a successor to the chief. Sister's sons are the closest male relatives who do (see also Flinn 1981).

The general points that (1) sister's children are a man's closest relatives in the next generation other than spouse's children and (2) as confidence of paternity diminishes sister's children are increasingly appropriate recipients of beneficence – and may become the *most* appropriate recipients (Alexander 1977, 1979, Kurland 1979) – remain the same, and one or both is involved in every hypothesis generated about the avunculate so far. (The question of why uxorilocality prevails in such regions remains unsolved – but see Ember and Ember 1971, Ember 1974.)

The continuing need for better hypotheses

Biologists using evolutionary approaches to human behavior may accept hypotheses that are too simple – because they appear to be consistent with evolutionary theory – and they often do not work hard enough to use intervening (or proximate) variables to help test and refine their hypotheses (see also Williams 1985, Symons 1987). For example, I wonder if culture has not driven the system of female-biased infanticide that Dickemann discusses even farther than she argues, in a direction positively correlated with reproductive success. Dickemann cites reports that the Jhareja subcaste of India killed *all* female infants at birth. In Indian society rank is evidently regarded as of crucial importance to everyone. Lower-ranking people sacrifice everything possible to elevate the rank of their descendants, and high-ranking people sacrifice to keep their rank.

Infanticide is evidently biased toward females not only in stratified societies but virtually everywhere it occurs. Regardless of the reasons, male offspring – probably males in general – are more highly regarded. Recent reports of a rise in female infanticide in China with efforts to restrict families to one child are an illustration. So are the controversies over using amniocentesis to sex embryos and discard some, primarily females. Even if we do not know the reasons for the phenomenon, we may ask: Where in the world has there not been a tendency to give special congratulations to the parents who produce a son?

Suppose that one's family is currently ranked at the top in a society in which rank is unusually precious, and that extreme effort is required to maintain top ranking. Might not the mere presence of female children be a negative influence? If a top-ranked family had *only* daughters surely its position would be threatened. And would it not be enhanced by the production of only sons?

I am suggesting that rank may be so precious, and so tied to maleness, that in stratified societies culture may sometimes carry efforts to bias offspring toward maleness virtually into the form of a potlatch, in which one cannot afford to lose rank even if the only way to avoid that is to kill all of one's daughters, even at seemingly great (reproductive) expense. Rather than rank and wealth resulting in female infanticide because of the low value or net costliness of females, females may be killed sometimes strictly to *preserve* rank.

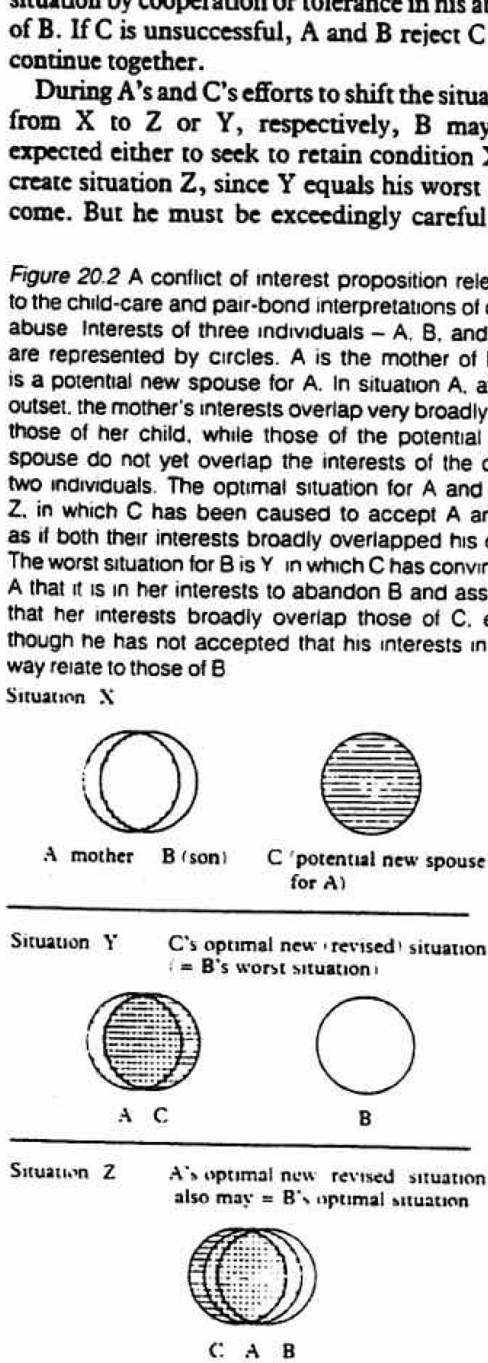
Whether or not this scenario has validity I suggest that, rather than the long-term interests of genes '. . . being sacrificed in the cause of status, influence, and perhaps money in the immediate future . . .,' what *look like* the *short-term* interests of the genes are being sacrificed in favor of their *long-term* interests by the acquiring and maintenance of status, influence, and wealth. I look forward to the unravelling of the intervening variables, proximate mechanisms, and history that will explain the rest of the situation. 'Special genes' for female-biased infanticide, incidentally, are not a part of the hypothesis – only abilities to imitate success and anti-imitate failure, and their application – Flinn and Alexander 1982).

I also wonder if Daly and Wilson (1981, 1983), in their studies of child abuse, may have failed to take into account a slightly different hypothesis than the one they tested. Perhaps the effect on the *pair bond* of having a child related to one parent but not to the other is decisive in predicting abuse (by *either* the real parent or the non-parent, or by the two jointly), not the relatedness *per se* or the fact that one spouse is unrelated to the child, so that the child will not yield reproductive benefits for the unrelated adult. The pair bond is the system whereby a very great proportion of each adult's lifetime reproduction will be realized. This 'pair-bond' hypothesis accounts for Daly and Wilson's report that adopted children are not abused more than children living with two real parents; because the relationship of the adoptee with its new parents is symmetrical, adoptees are even likely to *reinforce* the pair bond. The pair-bond hypothesis also predicts that either spouse or both may be involved in the abuse when only one spouse is the parent, as newspaper accounts indicate is true; Daly and Wilson's hypothesis does not seem able to account for this fact.

Consider Figure 20.2, which diagrams a conflict of interest proposition relevant to the child care and pair-bond interpretations of child abuse. A and C each strive to create the optimal situation for themselves. A seeks to create Situation Z, drawing C into her circle of interests without losing B. C seeks to draw A into his circle of interests without B coming along. B seeks to follow his mother, thus also to insinuate himself into C's circle of interests. If C convinces A that he is more important to her than B, he may abuse B (in the extreme eliminating him from the situation by killing him); and he may seek to get A to affirm her acceptance of this situation by cooperation or tolerance in his abuse of B. If C is unsuccessful, A and B reject C and continue together.

During A's and C's efforts to shift the situation from X to Z or Y, respectively, B may be expected either to seek to retain condition X or create situation Z, since Y equals his worst outcome. But he must be exceedingly careful not

Figure 20.2 A conflict of interest proposition relevant to the child-care and pair-bond interpretations of child abuse. Interests of three individuals – A, B, and C – are represented by circles. A is the mother of B. C is a potential new spouse for A. In situation A, at the outset, the mother's interests overlap very broadly with those of her child, while those of the potential new spouse do not yet overlap the interests of the other two individuals. The optimal situation for A and B is Z, in which C has been caused to accept A and B as if both their interests broadly overlapped his own. The worst situation for B is Y, in which C has convinced A that it is in her interests to abandon B and assume that her interests broadly overlap those of C, even though he has not accepted that his interests in any way relate to those of B



to create situation Y by promoting a close relationship between A and C which could itself lead to his own estrangement. He avoids this by both ingratiating himself to C and slowing the progress of overlap of A and C's interests so it does not get too far ahead of his relationship with C.

A, on the other hand, may be expected to 'lead' in the convergence of her interests with those of C more than B wants her to because she is somewhat less afraid of situation Y.

Are the studies reported in this volume scientific?

How would the studies reported in this volume differ if they had not incorporated an evolutionary approach?

First, and perhaps contrary to the suppositions of some, they would probably have been less scientific (less repeatable, less likely to contribute to a *cumulative* growth of knowledge). The reason is that nearly every one of these studies, whatever else its shortcomings, is quantitative, involving the systematic gathering and analyzing of data. Cultural and social anthropology have typically been based on evidence derived from interviews and expressed qualitatively and as interpretations. Such accounts are not useful unless the work is repeated more systematically; they are not reliable, or comparable to other studies. Quantitative work is not by any means restricted to those taking evolutionary approaches, but it is part of the heritage of the discipline of biology.

Second, the papers here all assume that the ultimate (evolved) function underlying all behavior is maximizing reproductive success, which means, usually in some complexly indirect way, maximizing the likelihood of survival of one's genetic materials. This assumption replaces several others in human-oriented disciplines, namely: (1) no assumption of function at all, (2) maximizing the likelihood of personal survival, (3) maximizing some particular proximate rewards (e.g. pleasure) and minimizing some particular proximate punishments (e.g. pain), (4) maximizing the likelihood of survival of some other individual (e.g. spouse, friend, leader, child), (5) maximizing the likelihood of survival, happiness, or success of one's social group (however defined), (6) maximizing morality, 'goodness,' social-service, or altruism, and (7) maximizing the good (or happiness) of the greatest (possible) number of humans (see Alexander 1987). The new assumption from recent evolutionary biology may be wrong. I suspect not, but if it is that should become apparent as studies like these – Williams' (1985) 'inspection of the organism' – multiply. In any case, the assumption of RS maximization as function is at the moment the only hypothesis consistent with the theory that virtually everyone agrees accounts for life on earth.

Third, all of the chapters that deal with social interactions make efforts to include actual genealogical relations, and treat variations in genetic relatedness as an important variable. Spousal and other reciprocal interactions of non-kin or distant kin are distinguished from interactions among close kin. Although genealogies have always been a part of ethnographic analysis of kinship systems and patterns of social behavior, until the late 1970s they were not related to Hamilton's (1964) notion of inclusive fitness maximization. Until the middle and late 1970s, no study of the social behavior of non-human species relied upon individual identification coupled with complete genealogical histories. Biologists now realize that without such information they are blind to the significance of most social activities. Anthropologists have always known that kinship was crucial in human sociality, but without Hamilton's insights they could not fully understand why. There was no comprehensive set of theoretical paradigms for interpreting kinship systems. These papers show that those days are gone forever.

Fourth, all authors in this volume explicitly seek to test one or more hypotheses. If their tests or their hypotheses are less than ideal, critics should compare their efforts to studies of human sociality in the past. Most, I believe, are without identifiable or explicit hypotheses at all, let alone tests.

Let us review some of the procedures employed by the authors in this volume, with an eye to comparing them both with earlier work and with the ideals of scientific procedure.

Chagnon (Chapter 2) *describes* kinship patterns. In this sense he is in the tradition of anthropological ethnographies. He also has an enormous amount of systematically gathered data amenable to statistical analysis, and he assumes efforts to maximize inclusive fitness (IFM). In these senses he is not traditional. He found one thing that is easy to interpret as consistent with IFM (that adult males call more young women by kin terms indicating potential wife than is really the case). He found two things he cannot as easily explain: (1) men classify kin faster than do women and (2) men use 'vague endearing' terms less often than women. For the amount of work he carried out, these might be seen as meager results. I would say, rather, that they attest to the expense and difficulty of analyzing human sociality; and we can be sure that much more will be extracted from this study.

Gaulin and Hoffman (Chapter 7) review evidence that human males and females differ in 'spatial ability' and conclude that they do, and that androgens trigger the divergence. They argue, from both human and non-human data, that this difference has arisen in conjunction with more extensive mate-seeking by males. Curiously, they do not explicitly discuss effects of the largely male activities of war and hunting in this context.

Crook and Crook (Chapter 5) propose that the monomarital system of Tibetan polyandry, and its family size and structure, are imposed by parents on their children so as to maintain the functionality of the farm by never producing more children than can subsist and rear children on it. This is the model I proposed in 1974, explaining fraternal polyandry as a combination of parental manipulation and kin selection in the maintenance of control of a non-partable resource. Polyandry would then be a consequence of the *importance* of the farm, the labor required to extract its resources, and the fact that farms have been historically divided to a minimal size for family success.

Crook and Crook's calculation of the fitness of males in polyandrous marriages seems incomplete in certain regards. Thus, it appears to refer to numbers of descendant and collateral relatives (offspring and nephews and nieces) rather than the results of effort exerted on behalf of such relatives. For polyandry the problem is complex for at least three reasons. First, a man may help nephews and nieces in other households, as well as in those in his own (polyandrous) household. Second, younger brothers almost certainly do not have equal access to their older brother's wife. Third, a younger brother who would have no children if he did not join his brother polyandrously would add to his fitness just by helping nieces and nephews, without having any children of his own.

Betzig (Chapter 2) found that chiefs on Ifaluk are deferred to, do less physical labor, and give their relatives disproportionate amounts of goods they redistribute. Although she reports the chiefs' constant admonitions to the whole group, and acknowledges the possibility of a 'managerial' benefit, she is reluctant to suggest that what they do is 'productive work.'

To accept this point, one would have to conclude that academics, inventors, foremen, administrators, executives, generals, and leaders and thinkers of all sorts are less 'productive' than the people they oversee or to whom they transmit their thoughts, and that what they do is mostly leisure, and exploitative. This need not be true. If an organizer or inventor causes a cooperative group to double its production, that person has been as *productive* as all other group members combined. If all who are exempt from physical labor are thereby exploitative, one would have to assume that corporations which hire executives are being altruistic. One must also consider the probable value of chiefs in developing and maintaining group unity in connection with the almost perpetual war that typified this region up to only 50 years or so ago.

In this instance Betzig, like others in this volume, has simply slammed up against one of the shortcomings of behavioral scans that are especially severe in studies of humans. Behavioral scans cannot tell what is going on in people's minds, and more goes on there, presumably, than is true for the members of any other species. Unfortunately, interviews and related methods do not necessarily inform us accurately either. For various reasons, little might be gained by asking either chiefs or others to describe the chief's value, or lack of it, to the group as a whole or to the individuals asked. (A second shortcoming of the behavioral scan method is that individual observations on different behaviors cannot be compared in quantitative terms. Effort – in terms of calories or risk – may be grossly different for behaviors that are entered as equivalents in a behavioral scan sample.) Of the studies reported here, those of Flinn (Chapter 11) and Kaplan and Hill (Chapters 17 and 18) seem to me to be good examples of explicit and formal use of both behavioral scans and other sources of information, such as interviews.

Borgerhoff Mulder (Chapter 3) assumes that variations in bridewealth among Kipsigi reflect differences in benefits to brides and grooms and their respective parents. She tests to see if in the Kipsigis reproductive success is involved. Her suggestion that 'low price paid for older brides is not due to some unspecific undesirability which leads to their delayed marriage' leaves us wondering why these women married later.

Borgerhoff Mulder says that she predicted that a groom's father living far from a bride's father will have to pay more for a bride and that her data support the hypothesis. She made the prediction because brides living close to their natal home may give more of their labor to their parents than to their husbands (or their husbands' families?). One wonders whether other, different hypotheses – just as compatible with evolutionary theory – are equally or better supported. Borgerhoff Mulder deals with some aspects of this question, but I have reservations. It is my experience that people who sell almost anything – objects, produce, domestic animals, or any kind of goods for which the price is subject to barter – are likely to charge less when they sell to relatives or friends, including neighbors, even when the item in question has no possibility of somehow itself returning to be useful to the seller. Neighbors and friends are people with whom one tends to have continuing – even lifetime – reciprocal interactions (this is true of relatives too, but the additional variable of genetic overlap is involved there). Is the lower price part of a continuing reciprocal interaction? This is especially pertinent since Borgerhoff Mulder says (Chapter 3) that affines 'help each other with agricultural work and preparations for large ceremonies but this cooperation derives more from their common *kokwet* membership [i.e. the fact of their geographic proximity] than from affinity *per se*.'

I also wonder if Borgerhoff Mulder's finding that the distance women live from their fathers is not associated with desertion by wives or wife-beating eliminates the hypothesis that parents are better able to assist and keep track of nearby daughters and grandchildren, in ways contributing to the parents' reproductive success. (Berté

Table 20.1 *Types of scientific 'investigations*

Sequence	Kind of data: Subjective	Objective
Hypothesis generated first	Data must be gathered 'blind'	Data do not necessarily need to be gathered 'blind'
Data gathered first	Hypothesis must be generated independently	

(Chapter 4) found that among K'ekchi Indians the cost of securing a bride for one's son is slightly higher if the bride comes from another village; the reason here, however, is that the cost of the wedding feast is higher owing apparently to the necessity of supplying food for more people.)

As I have stressed elsewhere (Alexander 1979), evolution is a simple process, not particularly difficult to understand, which means that it will be extremely difficult to apply broadly to problems as complex as understanding human social behavior. Anyone who finds it easy to assume that the first Darwinian hypothesis he thinks of is good enough should review the torturous efforts of biologists to use Darwinian theory to understand some comparatively simple attributes of life, such as sex ratios, sexuality, or outbreeding.

To digress a moment, the most serious scientific questions about behavioral studies involve when and how predictions from hypotheses are made and data purporting to test the predictions are gathered. Data can be divided into two classes, those involving more or less unequivocal decisions of a yes–no nature (e.g. did the subject pull his ear or not) and those involving highly subjective decisions (e.g. distinguishing different levels of agonism, especially when the grades of agonism cannot be separated by absolute breaks).

Data can be gathered before or after a hypothesis is generated and its predictions established; each sequence has advantages and disadvantages. Anyone who locates data already gathered to test a hypothesis generated later, *and independently*, is lucky indeed, especially if (1) he can prove that the data were unavailable to him, or unanalyzed, so that the hypothesis was indeed generated independently of them and (2) the data really are appropriate to test the particular hypothesis involved. If these two conditions can be satisfied, whether the data are subjective or objective (involve unequivocal decisions) is not important.

If, on the other hand, data are gathered explicitly to test a hypothesis already generated, then if the data involve subjective decisions they must be gathered by observers who are 'blind' to the hypothesis. Otherwise there is no obligation to accept the study as valid and no excuse for publishing it. This is particularly true when one outcome will further the career of the investigator substantially more than the alternative. Deliberate distortion need not be involved, for evidently investigators may deceive themselves without knowing it. If the data involve unequivocal yes–no observations we tend to be less concerned about 'blind' observations because deliberate distortion is necessary and the costs of this activity, if it is detected, typically involve termination of a career.

On this basis we are presented, then, with three kinds of 'scientific' investigations' as set out in Table 20.1.

This volume presents us with all three kinds of studies, and a few problems in respect to some of them. Crook and Crook, Brown and Hotra, Boone, Voland, Gaulin and Hoffman, and Low utilize data gathered prior to their studies. Low (Chapter 6), for example, uses data from the existing ethnographic literature to test a hypothesis generated orally by William D. Hamilton in 1981: that degree of polygyny in humans may correlate with variations in the severity of certain kinds of parasites. Low's data are sometimes subjective, but there can be no doubt that the data and the hypothesis are independent; and the tests are easily repeated. Low also deals with the problem of human uniqueness, here with respect to the amount of female choice that is likely involved in mate selection in polygynous societies, as compared to other organisms. Her conclusions, as we might expect, are not entirely unequivocal; but they point the way to further studies.

Chagnon, Betzig, Borgerhoff Mulder, Berté, Turke, Flinn, Essock-Vitale and McGuire, Hewlett, and Hill and Kaplan all gathered their own data. At least in some cases, the data were gathered with an evolutionary hypothesis in mind, or under the general assumption that evolution by natural selection has molded human behavior. All of the authors in this volume seem favorably inclined to this hypothesis (as am I), and when they seem to be performing tests of it they appear to be attempting to convince an audience that has not yet accepted it. What these authors are really interested in, and taking up, is the *next* level of investigation – namely, how knowing about evolution and assuming that selection has operated on human behavior can lead to predictions and findings that are unlikely to have been uncovered using any other general approach. This 'next level' of investigation includes not only what people do in particular circumstances, and how such things relate to the eventualities of culture, but, as well, what are their conscious motivations and reflections about what they are doing and why do the two realms connect as they do. As Symons (1987, pers. comm.) emphasizes (see also Ghiselin 1969, Trivers 1971, 1985, Alexander 1979, 1987), a Darwinian psychology lies ahead of us, and it will unfold as multiple levels of proximateness in (1) life function, (2) the structure of human effort and design of lifetimes, and (3) the makeup and activities of the human psyche.

The distinction between testing general evolutionary theory and seeking the next level of hypothesis is not trivial. *Ad hoc* hypothesizing is one of the most prominent complaints registered against evolutionary biologists examining human behavior. It consists of generating a new hypothesis when the old one has failed, *while continuing to adhere to the original general paradigm* (i.e. the sequence, 6, 8, 9, 10, Figure 20.1). The people who accuse evolutionary biologists of this supposed scientific sin are saying that if one hypothesis derived from considerations of natural selection is wrong, then the investigator is only being scientific if he discards the entire evolutionary paradigm. As Williams (1985) notes, however, if an automobile fails to run, 'Any sane engineer would first look for trivial rather than profound causes of failure, and test them in the expectation of confirming at least one.' The reason is that motors *usually run*; and in biology the motors of explaining organisms by natural selection have been running very well indeed, for a very long time.

The papers in this volume are, of course, not all methodologically flawless, and one of the problems comes from comparing the sequence of arguments or findings as given in the published articles with the actual sequence of discoveries in the investigations themselves. This is not an isolated problem in science. I remember reading Karl von Frisch's (1954) book and marvelling at the logical sequence of his thoughts and his experiments – until it dawned on me that he was telling a story. No long-term investigation works so flawlessly. No investigator plans so perfectly. All scientists reorder the parts of their investigations and findings so that the narrative of their publications will be briefer and more understandable to readers (and editors!). But reordering can also needlessly obscure what really happened, in ways that make results look more convincing than they would if the details were known to the reader. This is how all of the old falsehoods arose about the singularity of 'scientific method' that used to be taught in American high schools.

Suppose, for example, that it was found that the best Ache hunters (cf. Hill and Kaplan) hunt for longer periods than less successful hunters, not shorter ones, and that originally this puzzled the investigators, who knew that meat is divided evenly back at the camp. Subsequently the idea arose that hunting prowess might be important in sexual competition. From this hypothesis the investigators predict that men who hunt more successfully are also more successfully sexually. This hypothesis is confirmed. So it amounts to a test – not necessarily definitive – of the earlier hypothesis that hunting prowess is important in sexual selection. But the investigators could not legitimately turn the sequence around and say that they had originally predicted that hunting prowess is important in sexual competition and then argue that they had secured *two* results supporting the prediction – namely, not only that better hunters are sexually more successful but, as well, that better hunters hunt longer even though their catches are going to be divided evenly among the entire group later. There is an irony in the fact that if the investigators in this case reported the findings and hypotheses in the precise sequence in which they occurred, the original (and puzzling) finding that Ache hunters hunt longer would provide *some* additional support for the hypothesis that prowess in hunting is important in sexual selection because the discovery was independent of any such hypothesis, and made in the absence of any apparent explanation. In my opinion such a sequence should be reported, and the reader has a right to expect that he has not been confused by shifts in sequences of actual events during an investigation.

This particular reporting sequence is probably common in science, and it is probably the most serious methodological criticism I can level at any of the papers in this volume. Unless my biases are causing me to be a poor critic, that speaks well for the level of effort exhibited here.

Considering the procedural problems that I have discussed so far, it may not be surprising that those who attempt to study human behavior find it exceptionally difficult to be scientific (i.e. to do repeatable, quantitative studies) and simultaneously to avoid being trivial. Unfortunately, as I have stressed, most people also would agree that errors in studies of human behavior are more important than errors in analyses of the behavior of other organisms. These two problems are simply crosses that all students of humans have to bear.

How do these papers compare with those presented in earlier volumes – say, Chagnon and Irons (1979) or Alexander and Tinkle (1981)? I think the big difference is that a higher proportion of the papers is empirical as opposed to theoretical, and a higher proportion use data gathered by the investigators themselves. It would be nice if we could say that there are now many times as many investigators active in this general arena, but in truth we cannot. The number of people involved is still disappointingly small. It would be wonderful if we could say that new quantitative field studies are being generated on every hand, but we cannot. There are probably more new studies represented here, and more quantitative data, than in any previous anthropological symposium volume. Some of the data, however, derive from information gathered years ago, and either unpublished until now or else used one or more times already. Why is this true? It is true in part because the distinguished social anthropologist I quoted at the outset was right: The anthropology panels of the US National Science Foundation have not funded much work of the sort reported here, and most of these authors have extensively used their own personal funds to complete their research. Nevertheless, of the studies reported in this volume, six acknowledge support from US federal grant sources, three were supported by US private foundations, and one by a non-US foundation.

But that same distinguished social anthropologist could not have been more wrong than to apply the word 'fad' to the use of an evolutionary approach to examine human behavior. Perhaps the current hiatus with respect to acceptance of use of evolution in studies of human behavior will last many more years. Perhaps not. But it will be passed through. And as that process takes place it will become increasingly advantageous for young social scientists, competing for positions in the tight academic job market by producing notable research results, to know how to employ evolutionary arguments and hypotheses when they are useful and how to bypass or discard them when they are not. The people contributing to this volume have demonstrated that they know how to do these things.

Conclusion

There is probably no area of investigation in which it is more difficult to be scientific than that involving human behavior. Pitfalls and hostilities lurk at every turn. The expense and effort required to secure significant data on important questions is, to say the least, daunting. But if we evolutionists persevere, our academic descendants will probably prevail. My own view of the optimal outcome would be for the significance of evolution to become so widely known and so thoroughly imbedded in the understanding of all those working in human-oriented disciplines, that its tenets can be employed, without fanfare, when they are useful, and ignored or discarded when they are not. This is in fact the situation that prevails throughout most of the biological sciences, even if there are also flurries of dismay and controversy there when significant shortcomings are discovered in theory or its application.

References

Alexander, R. D. (1974) The evolution of social behavior. *Annual Review of Ecology and Systematics*, 5, 325–83.

Alexander, R. D. (1977) Natural selection and the analysis of human sociality. In *Changing Scenes in the Natural Sciences: 1776–1976*, ed. C. E. Goulden, pp. 283–337. Bicentennial Symposium Monograph, Philosophical Academy of Natural Science Special Publication 12.

Alexander, R. D. (1978) Natural selection and societal laws. In *The Foundations of Ethics, Vol. 3, Morals, Science, and Society*, ed. T. Engelhardt and D. Callahan. pp. 138–82. Hastings-on-Hudson, NY: Hastings Institute.

Alexander, R. D. (1979) *Darwinism and Human Affairs*. Seattle: University of Washington Press.

Alexander, R. D. (1985) Genes, consciousness, and behavior theory. In *A Century of Psychology as Science*, eds. S. Koch and D. E. Leary, pp. 783–802. New York: McGraw-Hill.

Alexander, R. D. (1987) *The Biology of Moral Systems*. Hawthorne, NY: Aldine-deGruyter.

Alexander, R. D. and Tinkle, D. W. (eds.) (1981) *Natural Selection and Social Behavior: Recent Research and New Theory*. New York: Chiron Press.

Barash, D. (1977) *Sociobiology and Behavior*. New York: Elsevier.

Betzig, L. L. (1986) *Despotism and Differential Reproduction: A Darwinian View of History*. Hawthorne, NY: Aldine-deGruyter.

Betzig, L. L. and Turke, P. (1986) Parental investment by sex on Ifaluk. *Ethology and Sociobiology*, 7, 29–37.

Brues, A. (1964) The cost of evolving vs. The cost of not evolving. *Evolution*, 18, 379.

Brues, A. (1969) Genetic load and its varieties. *Science*, 164, 1130.

Buss, D. M. (1985) Human mate selection. *American Scientist*, 73, 47–51.

Buss, D. M. (1987) Sex differences in human mate selection criteria. An evolutionary perspective. In *Sociobiology and Psychology: Ideas, Issues, and Applications*, eds. C. Crawford, M. Smith, and D. Krebs. Hillsdale, NJ: Erlbaum (in press).

Chagnon, N. A. and Irons, W. (eds.) (1979) *Evolutionary Biology and Human Social Behavior: An Anthropological Perspective*. North Scituate, MA: Duxbury Press.

Conant, J. R. (1951) *On Understanding Science*. New York: The New American Library (Mentor).

Daly, M. and Wilson, M. (1981) Abuse and neglect of children in evolutionary perspective. In *Natural Selection and Social Behavior: Recent Research and New Theory*, eds. R. D. Alexander and D. W. Tinkle, pp. 405–16. New York: Chiron Press.

Daly, M. and Wilson, M. (1983) *Sex, Evolution, and Behavior*, 2nd edition. Boston: Willard Grant Press.

Darwin, C. (1859) *On the Origin of Species*. A facsimile of the first edition with an introduction by Ernst Mayr, published in 1967. Cambridge, MA: Harvard University Press.

Dawkins, R. (1976) *The Selfish Gene*. New York: Oxford University Press.

Dickemann, M. (1979) The reproductive structure of stratified human societies: a preliminary model. In *Evolutionary Biology and Human Social Organization: An Anthropological Perspective*, eds. N. A. Chagnon and W. G. Irons, pp. 321–67. North Scituate, MA: Duxbury Press.

Ember, C. R. (1974) An evaluation of alternative theories of matrilocal versus patrilocal residence. *Behavior Science Research*, 9, 135–49.

Ember, M. and Ember, C. R. (1971) The conditions favoring matrilocal versus patrilocal residence. *American Anthropologist*, 73, 571–94.

Fisher, R. A. (1930 [1958]) *The Genetical Theory of Natural Selection*. 2nd edition, 1958. New York: Dover.

Flinn, M. V. (1981) Uterine versus agnatic kinship and associated cousin marriage preferences: an evolutionary biological analysis. In *Natural Selection and Social Behavior: New Research and Theory*, eds. R. D. Alexander and D. W. Tinkle. New York: Chiron Press.

Flinn, M. V. and Alexander, R. D. (1982) Culture theory: The developing synthesis from biology. *Human Ecology*, 10, 383–400.

Frisch, K. von (1954) *The Dancing Bees: An Account of the Life and Senses of the Honey Bee* (trans. Dora Ilse). London: Methuen and Co.

Gaulin, J. C. and Schlegel, A. (1980) Paternal confidence and paternal investment: a cross-cultural test of a sociobiological hypothesis. *Ethnology and Sociobiology*, 1, 301–9.

Ghiselin, M. T. (1969) *The Triumph of the Darwinian Method*. Berkeley: University of California Press.

Gould, S. J. and Lewontin, R. C. (1979) The spandrels of San Marco and the panglossian paradigm: A critique of the adaptationist program. *Proceedings of the Royal Society, London, B*, 205, 581–98.

Haldane, J. B. S. (1957) The cost of natural selection. *Journal of Genetics*, 55, 511.

Hamilton, W. D. (1964) The genetical evolution of social behavior. I, II, *Journal of Theoretical Biology*, 7, 1–52.

Harris, M. (1979) *Cultural Materialism: The Struggle for a Science of Culture*. New York: Random House.

Hartung, J. (1976) On natural selection and the inheritance of wealth. *Current Anthropology*, 17, 607–22.

Hoogland, J. L. (1977) The evolution of coloniality in white-tailed and black-tailed prairie dogs (Sciuridae: *Cynomys leucurus* and *C. ludovicianus*). Ph.D. Dissertation, University of Michigan.

Hoogland, J. L. and Sherman, P. W. (1976) Advantages and Disadvantages of bank swallow (*Tiparia riparia*) coloniality. *Evolutionary Monographs*, 46, 33–58.

Kamin, L. (1985) Genes and behavior: The missing link. *Psychology Today*, 19, 76–8.

Kitcher, P. (1985) *Vaulting Ambition*. MIT Press.

Konner, M. and Worthman, C. (1980) Nursing frequency, gonadal function, and birth spacing among !Kung hunter–gatherers. *Science*, 207, 788–91.

Kurland, J. A. (1979) Paternity, mother's brother, and human sociality. In *Evolutionary Biology and Human Social Behavior: An Anthropological Perspective*, ed. N. A. Chagnon and W. G. Irons, pp. 145–80. North Scituate, MA: Duxbury Press.

Lack, D. (1965) Natural selection and human nature. In *Biology and Personality: Frontier Problems in Science, Philosophy, and Religion*, ed. I. T. Ramsey, pp. 40–8. New York: Barnes and Noble Inc.

Lewontin, R. C. (1979) Sociobiology as an adaptationist program. *Behavioural Sciences*, **24**, 5–14.

Lewontin, R. C., Rose, S. and Kamin, L. J. (1985) *Not in Our Genes: Biology, Ideology, and Human Nature*. New York: Pantheon Books.

Maynard Smith, J. (1983) Current controversies in evolutionary biology. In *Dimensions of Darwinism*, ed. M. Grene, pp. 273–86. Cambridge: Cambridge University Press.

Maynard Smith, J. (1984) Science and myth. *Natural History*, **93**, 10–25.

Maynard Smith, J. (1985) Biology and the behaviour of man. *Nature*, **318**, 121–2.

Muller, H. J. (1950) Our load of mutations. *American Journal of Human Genetics*, **2**, 111–76.

Popper, K. (1963) Science: conjectures and refutations. In *The Growth of Scientific Knowledge*, pp. 33–7. New York: Harper Torch Books.

Raymond, J. C. (1982) Rhetoric: The methodology of the humanities. *College English*, **44** (8), 777–83.

Sahlins, M. D. (1976) *The Use and Abuse of Biology: An Anthropological Critique of Sociobiology*. Ann Arbor: University of Michigan Press.

Schneider, D. (1961) Introduction. In *Matrilineal Kinship*, eds. D. M. Schneider and K. Gough, pp. 1–29. Berkeley: University of California Press.

Schneirla, T. C. (1950) The relationship between observation and experimentation in the field study of behavior. *Annals of the New York Academy of Science*, **51**, 1032–44.

Segerstrale, U. (1986) Colleagues in conflict: An 'in vivo' analysis of the sociobiology controversy. *Biology and Philosophy*, **1**, 53–87.

Sherman, P. W. (1977) Nepotism and the evaluation of alarm cells. *Science*, **197**, 1246–53.

Smith, P. (1984) *Dissenting Opinions: Selected Essays*. San Francisco: North Point Press.

Symons, D. (1987) If we're all Darwinians, what's the fuss about? In *Sociobiology and Psychology: Ideas, Issues, and Applications*, eds. C. B. Crawford, M. F. Smith and D. L. Krebs. Hillsdale, NJ: Erlbaum (in press).

Trivers, R. L. (1971) The evolution of reciprocal altruism. *Quarterly Review of Biology*, **46**, 35–57.

Trivers, R. L. (1985) *Social Evolution*. Menlo Park, CA: Benjamin/Cummings.

Trivers, R. L. and Willard, D. E. (1973) Natural selection of parental ability to vary the sex ratio of offspring. *Science*, **179**, 90–8.

Vining, D. R. (1986) Social versus reproductive success: the central theoretical problem of human sociobiology. *Behavioral and Brain Sciences*, **9**, 167–87.

Wallace, B. (1968) *Topics in Population Biology*. New York: W. W. Norton.

White, L. (1947) The expansion of the scope of science. *Journal Washington Academy of Sciences*, **37**, 181–210.

Williams, G. C. (1966) *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought*. Princeton: Princeton University Press.

Williiams, G. C. (1985) *A Defense of Reductionism in Biology*. *Oxford Surveys in Evolutionary Biology*, **2**, 1–27.

Wilson, E. O. (1975) *Sociology: The New Synthesis*. Cambridge, MA: Harvard University Press.

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The discussion of the subject of this volume, 'Human reproductive behaviour', necessarily involves the frequent use of some very common words, which occur either in pairs (e.g. brother–sister, father–mother, husband–wife, male–female, man–woman) or singly (e.g. child, family, kin, marriage, offspring). In order to avoid printing long strings of useless page numbers after these headings (keywords), they have, wherever possible, been lumped together, in two ways. Thus, 'mothers, 41–6' means that the word 'mother' occurs *at least once* on each of the pages mentioned, and 'mother, 144–228 *passim*' means that the word 'mother' occurs sporadically in these pages but not on every one of them.

An extra indentation in the run-on sub-headings indicates the beginning of a particularly important sub-heading and (subsequently) the resumption of the ordinary sub-headings.

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