The Evolution of Human Sociality

A Darwinian Conflict Perspective

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Nature has planted in our minds an insatiable longing to see the truth.

Cicero

Knowledge is what we get when an observer, preferably a scientifically trained observer, provides us with a copy of reality that we can all recognize.

Christopher Lasch

Do not become archivists of facts. Try to penetrate the secret of their occurrence, persistently search for the laws which govern them.

Ivan Pavlov

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Preface

The Evolution of Human Sociality is an attempt at a broad theoretical synthesis within the field of sociology and its closely allied sister discipline of anthropology. It is a labor of love that has resulted from nearly a quarter-century of sustained thought. I explicate and critique all of the major theoretical approaches and try to draw what I consider the most useful elements of the best of these into a synthesized theory that I call *Darwinian conflict theory*. This theory, more properly called a theoretical perspective or strategy, in the most general sense is a synthesis of the tradition of economic and ecological materialism and conflict theory stemming from Marx and the tradition of biological materialism deriving from Darwin. I originally called this theory *synthetic materialism* (see Sanderson, 1998a, 1998b, 1999:403-08), but I have gradually come to feel that the name is too bland and uninformative. But everyone knows who Darwin was and why he is important, and conflict theory is a term that resonates well with sociologists and is immediately recognizable to them. And, in a sense, the new name is more accurate since Darwinian and neo-Darwinian evolutionary biology and sociobiology – the theoretical foundation of the book – represent a type of conflict theory in which people are competing in a vast game of survival, resource acquisition, and reproduction.

Approximately the first half of the book is taken up with critiques of existing theoretical approaches. More than anything else, I am concerned with the "logic of explanation" of each major approach. I am concerned, for example, with prototypical Marxist and functionalist modes of explanation, not with such things as the myriad characteristics of the functionalist school launched by Talcott Parsons or whether Marxists have started incorporating Weberian ideas into their theories. Another way of putting it is to say that I am concerned with the basic type of explanatory logic that each strategy carries, its most characteristic mode of explaining the social world. My discussion of each theoretical strategy is therefore quite deliberately highly restricted. This then leads to the full elaboration, in formal propositional form, of the synthetic theory.

The second half of the book lays out in as much detail as space allows the large amount of evidence, both qualitative and quantitative, that I claim supports Darwinian conflict theory. To a large extent this is the most important part of the book in terms of getting readers to be open to my approach. The substantive areas concerning which the evidence is drawn are those that have been of crucial concern to both sociology and anthropology for much of their existence. A significant omission is the topic of ethnicity. I leave this topic aside here because of space limitations and also because the topic is so important, so vast, and so emotionally charged that I feel it deserves a book of its own.

I am grateful to Donald Brown, Andre Gunder Frank, Art Alderson, Jerome Barkow, Alexandra Maryanski, Joseph Lopreato, Penny Anthon Green, Herbert Hunter, Alex Heckert, and Ray Scupin for their comments on early versions of the main ideas behind this book as they existed in various conference papers. Pierre van den Berghe and Bruce Lerro read the entire

manuscript in its penultimate form and I am most grateful to them for having done so. Timothy Crippen read one of the early conference papers, the chapter on sociobiology, and all of the evidence chapters and made many useful comments that I have been able to incorporate into the final revisions.

I want to single out Pierre van den Berghe for special mention. He has influenced my thinking in this book more than any other single individual. When his book *Man in Society: A Biosocial View* first appeared in 1975, I was immediately taken by it and used it for several years as a text in my introductory course. Later he returned the favor by using various editions of my *Macrosociology: An Introduction to Human Societies* in his course on comparative societies until his retirement a few years ago. Pierre and I began an intellectual correspondence in the late 1970s that has continued down to this day, a correspondence that has been a source of great satisfaction for me. For many years he argued that most of my thinking was highly compatible with sociobiology and urged me to "take the plunge" and incorporate sociobiological ideas more fully. Eventually the light bulb went on and I did, as this book testifies. Pierre has the crucial traits a good sociologist should have: irreverence, iconoclasm, wit, a resistance to intellectual fads, a methodological eclecticism, a willingness to put together the best of the best theoretical schools, and a penchant for the truth rather than the popularity of one's ideas. Had Pierre not existed, this book would not exist, at least not in its current form. I would therefore have had to invent him or not write the book. Pierre also suggested the book's title, which I gladly accepted over my two original titles in the interest of reaching the broadest possible audience.

Randall Collins, another sociologist for whom I have great admiration, does not like the theoretical synthesis this book creates even though I have tried to convince him that it is highly compatible with his own version of conflict theory and, in fact, serves as the necessary grounding for that theory. Randy even went so far as to urge me not to write the book, saying that there would be hostility to it from mainstream sociologists. It is the first time he has not been complimentary of one of my books. For better or worse, I have not heeded his advice. He is undoubtedly right: Many sociologists will reject the main arguments of this book. However, many of the leading ideas in modern thought were initially rejected, even in markedly hostile form, when they were first proposed. One thinks, for example, of Galileo or Darwin. I am not so presumptuous as to put myself in the company of these great men; I am simply saying that I believe that the most controversial ideas of this book will eventually lose their controversial status and become widely accepted throughout the social sciences. Indeed, the trend is already apparent, if not in my own discipline of sociology.

For a quarter of a century I have taught undergraduate and graduate courses in sociological theory at Indiana University of Pennsylvania. This has provided me with an enormous amount of intellectual stimulation and allowed me the opportunity to think through many of the ideas developed in this book much more thoroughly than would otherwise have been possible. I am extremely grateful to all of the students in these classes for having made all of this possible, especially to those who really challenged me to articulate and justify my arguments. I am also grateful to Indiana University of Pennsylvania for granting me a sabbatical leave during the Fall semester of 1999 so I could finish the book's penultimate draft.

I am very pleased that my editor at Rowman & Littlefield, Dean Birkenkamp, agreed to publish this book and demonstrated such enthusiasm for it. Dean is easily the nicest editor in publishing and one of the best. I hope the book can live up to his expectations. Finally, I wish to thank my graduate assistant, Mary Reilly, for her extremely careful checking of the bibliography, which permitted me to correct several errors and provide missing references.

When I was about 11 or 12 years old I discovered biology and it was love at first sight. By the end of high school I somehow got diverted into psychology, and then in my first year of college into sociology. I was better at biology than at any other subject I ever studied, and perhaps should have stayed in it. But I have no regrets, because I have found the study of society fascinating. I have, in a sense, now come full circle, reuniting sociology with my first great love. It makes me very happy to be able to contribute to the unification of the biological and social sciences.

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Most of the epigrams used throughout this book have been drawn from Thiessen (1998).

Portions of Chapter 10 were published in Stephen K. Sanderson and Joshua Dubrow, "Fertility decline in the modern world and in the original demographic transition: Testing three theories with cross-national data." *Population and Environment* 21:511-37, 2000. Portions of Chapter 13 were published in Stephen K. Sanderson, "Explaining monogamy and polygyny in human societies: Comment on Kanazawa and Still." *Social Forces* 2001.

Chapter 8

Sociobiological Explanations

It is universally acknowledged that there is a great uniformity among the actions of men, in all nations and ages, and that human nature remains the same in its principles and operations. The same motives always produce the same actions; the same events follow from the same causes.

David Hume

Theories that go counter to the facts of human nature are foredoomed.

Edith Hamilton

All great truths begin as blasphemies.

George Bernard Shaw

The *tabula* of human nature was never *rasa* and now it is being read.

William D. Hamilton

BASIC PRINCIPLES

Sociobiology may be said to have officially begun in 1975 with the publication of Edward O. Wilson's *Sociobiology: The New Synthesis.*[1] Wilson, a zoologist, defined sociobiology as "the study of the biological basis for social behavior in all animals." The vast majority of his book was taken up with a discussion of animals other than humans, and only the final chapter was devoted to humans. Here Wilson speculated about the biological foundations of human behavior. Sociobiology is to a considerable degree a reaction against what later came to be called the Standard Social Science Model (SSSM). The SSSM includes a wide variety of theoretical perspectives, but the basic assumption is that human social organization has little or nothing to do with human biology. The behavior of humans everywhere is largely determined by social and cultural influences external to individuals and their genetics. In its most extreme form, the SSSM holds that the mind is a *tabula rasa*, or blank slate, on which society and culture write their script. Sociobiologists hold that this view, which has been the dominant view among all social scientists for most of the twentieth century, is wrong, or at least a gross exaggeration and oversimplification.

Some social scientists were beginning to take the biological foundations of human behavior seriously before Wilson wrote his famous book. In 1971 Lionel Tiger and Robin Fox, two anthropologists, wrote *The Imperial Animal*, and in 1975 Pierre van den Berghe, a well-known sociologist, wrote *Man in Society: A Biosocial View.* These were what might be termed "protosociobiological" works. Tiger and Fox argued that humans come equipped with a *biogrammar*, or a basic set of biological templates that

predispose their behavior along certain lines. Van den Berghe made the same point, referring to the human biological predispositions as *Anlagen*. The lists of predispositions offered by these authors overlap extensively, and they can be combined into one list as follows:

1. *Aggression*: Humans are by nature rather aggressive organisms, which means that they are prone to use violence, or the threat of violence, as a means of settling disputes and attaining desired ends.
2. *Hierarchy*: Humans are prone to organize themselves in rank-ordered systems, or systems in which the competition for status is of paramount importance.
3. *Male domination*: There is a strong tendency for males of the species to exert authority over females and to predominate in higher-status positions.
4. *Mother-infant bonding*: Women are biologically specialized for parenting to a degree that is significantly greater than men's tendency toward parental care.
5. *Territoriality*: Human individuals tend to be territorial creatures in the sense that they define and defend regional spaces all the way from small personal spaces to huge national states.
6. *Incest avoidance*: The general avoidance of incest among humans, widely considered to be a human universal, is rooted in a biological tendency that exists because of incest's harmful biological consequences.

It is critical to note that these social scientists stress that these are biological *predispositions* or *tendencies*, not rigid instincts, and as such interact in important ways with the social, cultural, and physical environment to produce the actual behavior that is observed in human societies. Social organization and behavior are always *biosocial* in the sense that they result from the complex interaction of the biological and the sociocultural. For example, despite a human tendency toward aggression, we see that, although some societies display very high levels of aggressive behavior, others display much lower levels of such behavior. The assumption is that some sociocultural conditions (such as resource competition) trigger high levels of aggression, whereas other sociocultural conditions (such as resource abundance) elicit only low levels of aggression. Similarly, despite a human tendency toward hierarchy, we see that some societies (such as most hunter-gatherers) have only minimal hierarchies, whereas others (such as agrarian and industrial societies) have much more elaborate hierarchies. Think of it this way. The sociocultural and physical environments are like the volume control knob on a radio; they can turn predispositions to higher levels or they can tone them down. In a few instances they might be able to turn these predispositions off; despite natural human sex drives, for example, there have been many priesthoods whose members have taken vows of celibacy. However, the predispositions tend to express themselves to at least some extent in all environments.

The sociobiology proper that was born in 1975 was rooted in an explicit theory known as the *theory of inclusive fitness* or *kin selection*, which is a specific dimension of neo-Darwinian evolutionary biology. As is well known, the central concept in Darwin's theory was that of *natural selection*. Darwin argued that in every population in every generation more offspring are produced than available resources can sustain. There thus results a *struggle for survival* among organisms, and it is the fittest organisms that have the best chance of surviving. Organisms are fit to the extent that they have the anatomical, physiological, and behavioral traits better suited, or better adapted, to coping with the conditions of a particular environment. Moreover, those organisms whose traits reveal relative adaptive advantage leave more offspring and thus also enjoy greater reproductive success. In Darwinian and neo-Darwinian terms, they have greater "fitness."

Darwin assumed that the organism was the unit of selection and that the traits of every organism evolved in accord with selfish reproductive interests. The assumption of selfishness was the only assumption that made sense, because organisms are competing in a game of survival and reproduction. However, Darwin also knew that organisms also behaved altruistically, often making sacrifices (including giving up their own lives), for the benefit of conspecifics. We know, and Darwin did also, that this kind of behavior is highly characteristic of the social insects, who have soldier castes biologically specialized for defending the hive at the cost of their own lives. At higher phylogenetic levels, we find a wide variety of altruistic behaviors. This fact of altruism seemed to be in contradiction to what Darwin was saying, and constituted a conundrum that took many years to solve. An elegant solution to the puzzle of the evolution of self-sacrifice was proposed in 1964 by the geneticist William Hamilton, considered by some the greatest

theoretical biologist since Darwin. Hamilton argued that what created the puzzle was the assumption that it was the organism that was the unit of fitness. Hamilton argued that it was actually the *gene* that was the unit of fitness and that natural selection operates so as to preserve or eliminate genes rather than entire organisms. Hamilton claimed that there was a form of fitness called *inclusive fitness*. An organism's fitness is not simply a matter of its own survival, but involves the total number of its genes that are represented in the gene pool of its species. Organisms are more fit than others when they have a larger representation of their genes in the gene pool.

Inclusive fitness is sometimes called *kin selection* because it assumes that organisms tend to favor kin, and favor close kin more than distant kin, because they share genes in common with kin and an increasing number of genes in common as kinship becomes closer. Parents and their offspring share one-half of their genes, as do, on average, full siblings. Grandparents and grandchildren share one-quarter of their genes, and first cousins share, on average, one-eighth. Hamilton argued that organisms behaved so as to promote their inclusive fitness, and this could sometimes lead the organism to give up its own life. For example, if a mother bird warns three of her chicks that a predator is approaching them and in so doing diverts the predator's attention to her and causes him to eat her, she nevertheless saves three half-units of her genes, for a total of one and a half units. Since she is dead, we have to subtract one unit of her genes (those inside her own body), but this leaves her with a net gain of one-half unit. The conclusion is that by warning her offspring she actually benefitted genetically despite causing her own death because she optimized the number of copies of her genes that she left in the gene pool. This leads to the further observation that what is altruism on the surface level (the level of actual behavior) is actually selfishness - genetic selfishness - at a deeper level (the level of the adaptive mechanisms that are triggering the behavior).

How does all this apply to human behavior? The basic argument is that *many features of human behavior and social organization result from efforts made by individuals to maximize their inclusive fitness.* No assumption is being made that humans have any conscious understanding that they are motivated in such a way; they are simply acting, to a large degree unconsciously, in accordance with evolved brain modules that arose during the course of hominid evolution. Let's look at the implications of this argument. The most fundamental implication is that men and women will have different ways of promoting their inclusive fitness, and thus different *reproductive strategies.* Males can maximize their inclusive fitness by inseminating as many females as possible, and thus we should expect men to have difficulty confining themselves to monogamous relationships. Instead, they should greatly desire a wide variety of sexual partners. Females, on the other hand, can have only a limited number of offspring in a lifetime, and it does not matter whether these offspring are produced by one man or several. Their inclusive fitness can best be served by choosing good mates, i.e., mates who will stick around and provide for the offspring. Philandering can actually reduce a woman's reproductive fitness in that it can reduce a man's commitment to her.[2]

But there is more to the story. If a man provides for offspring, he wants to be sure that these offspring are his. Aside from prereproductive death, the worst possible outcome with respect to a man's reproductive interests would be for him to provide for another man's offspring, thinking they are his own, and thus promote that man's inclusive fitness at the cost of his. Therefore, men should behave in ways so as to maximize their confidence that the offspring they are rearing are their own. Men attempt to maximize their confidence of paternity primarily through one or another type of mate guarding behavior, such as sequestering women (done throughout the Islamic world and in many other places and times) or fitting them with chastity belts when the men are away, and mate guarding behavior is driven by the evolved adaptation of male sexual jealousy. (These mate guarding behaviors are known as *anticuckoldry strategies*; a cuckolded man is one who has been deceived by his mate into believing that the offspring he is providing for are his own.)

An important qualification to the above is essential. It seems clear that in some societies individuals engage in behaviors that do not appear to promote their inclusive fitness. For this reason Joseph Lopreato (1989) has formulated what he calls the *modified maximization principle.* This principle holds that (1989:129) "organisms are predisposed to behave so as to maximize their inclusive fitness, but this predisposition is conditioned by the quest for creature comforts, by self-deception, and by autonomization

of phenotype from genotype." As Timothy Crippen (1994:315) has stated, this principle "suggests that human behavior maximizes fitness unless its reproductive consequences are subverted by the desire to accumulate resources that engender pleasure, by self-denying or ascetic tendencies often stimulated by sacred beliefs and practices, and/or by motivations that once produced fitness maximizing behaviors (e.g., motivations underlying sexual activity), but that now are harnessed in the service of non-maximizing behaviors (e.g., sexual activity between individuals using some method of contraception)." This means that proximate constraints may subvert the tendency to maximize fitness. Crippen goes on to say that "for individuals living in demographically immense, class-divided, and technologically advanced societies, the proximate motives connected with the pursuit of pleasure and the aversion to pain may be harnessed in the service of behaviors that have little, and perhaps even negative, influence on an individual's inclusive fitness" (1994:318).

It is often assumed, especially by its critics, that sociobiology assumes some sort of *genetic determinism*, or direct one-to-one correspondence between genes and behavior. In fact, nothing could be further from the truth. Sociobiologists have always stressed the complex interaction between the biogram and the various and sundry features of the natural and social environment. Social behavior is always a *biosocial* phenomenon. Much of what is in the human biogram is *facultative* in nature, and humans are by far the most facultative of species (R. Alexander, 1990; Sober and Wilson, 1998). Facultative traits are those whose expression depends upon the organism's ability to carefully assess the environment and then respond with the appropriate adaptive behavior. These traits involve conditional strategies that give behavior considerable flexibility (R. Alexander, 1990). Richard Alexander (1990) nominates polyandry as a good example of a facultative trait. There are certainly no "genes for polyandry," but there are genes involved in building brains that direct individuals to assess their environment in such a way that they may adopt polyandrous marriage when it is the best way of promoting one's inclusive fitness. Likewise, there are certainly no "genes for cross-cousin marriage," but there are genes that lead to cross-cousin marriage's being adaptive under certain types of environmental conditions. Reproductive behavior also appears to be highly facultative, with individuals seemingly adjusting their fertility levels to the extent to which offspring are likely to survive. Where infant and child mortality are high, fertility levels tend to be high in order to produce a certain number of surviving offspring, but when the survival prospects of children are good the same number of surviving children can be produced by a lower level of overall fertility (Carey and Lopreato, 1995). Sexual maturation also appears to be facultatively controlled. It has been discovered that in economically marginal and stressful environments girls reach puberty earlier, and thus start reproduction earlier, presumably as an inclusive fitness promoting strategy (Chisholm, 1999). One final example of a facultative trait involves parental investment in offspring. Throughout the world's societies parental investment in sons usually exceeds parental investment in daughters, generally because sons have greater potential reproductive success. However, in a number of societies, or segments of societies, parents can promote their reproductive success better by investing more in daughters (Dickemann, 1979a; Cronk, 1989, 1991, 1999). Among the Mukogodo of Kenya, for example, daughters are shown clear favoritism, and the reason appears to be because girls have much better marital prospects than boys. Girls are usually able to marry, often having a good chance of obtaining high-status men from neighboring societies as husbands. Boys, on the other hand, frequently have a great deal of difficulty accumulating enough livestock to provide the bridewealth payments necessary to secure a wife. They may marry late in life, or never (Cronk, 1989, 1991, 1999).

What kind of evidence would support the basic predictions of sociobiology? One kind would be evidence showing that male sexual possession of females and males' desires for sexual variety are extremely common, if not universal, behaviors. Donald Symons's book *The Evolution of Human Sexuality* (1979), a sociobiological classic, presents a wealth of such evidence. We should also find evidence all over the world that "blood is thicker than water," i.e., that everywhere kin are favored over nonkin, and this is what we do find. Donald Brown (1991) argues persuasively that if a pattern of human behavior can be shown to be a true human universal or near-universal - found in all or virtually all societies - then there is a strong presumption that it rests on a solid biological foundation. (This should not be taken to imply that sociobiology is only relevant to explaining universals or near-universals. As the discussion of facultative

traits showed, sociobiology is also extremely relevant to explaining a wide range of cultural variations. However, universals or near-universals are especially good evidence that a cultural trait is biologically rooted.) Below is a list of human universals or near-universals, not placed in any particular order of importance or meant to be exhaustive (Brown, 1991; Pinker, 1994):

1. Gossip, lying, misleading, verbal humor, humorous insults, poetic and rhetorical speech forms, storytelling, metaphor.
2. Binary distinctions.
3. Coy flirtations with the eyes; masking, modifying, and mimicing facial expressions; displays of affection.
4. Nonlinguistic vocal communication such as cries and squeals.
5. Adornment of bodies and arrangement of hair.
6. Drugs, both medicinal and recreational.
7. Male monopolization of political leadership, high-status positions, and warfare.
8. Great interest in the topic of sex; standards of sexual attractiveness.
9. Male sexual jealousy.
10. Sex as a service provided by women to men.
11. Husbands usually older than wives.
12. Men more aroused by visual sexual stimuli than women.
13. More aggression and violence by men.
14. Incest avoidance.
15. Language.
16. Abstract thinking.
17. Family, marriage, and kinship systems; families built around a mother and children, usually the biological mother and one or more men.
18. Kin terminologies; distinguishing of close kin from distant kin; favoring of close kin.
19. Facial expressions relating to happiness, sadness, anger, fear, surprise, disgust, and contempt.
20. Tool making and technology.
21. Making or using fire.
22. Group living and sociability.
23. Groups defined by locality or territory.
24. Socialization practices; children copying their elders.
25. Prestige seeking and allocation.
26. A division of labor based minimally on age and sex.
27. Conceptions of gender and gender roles.
28. Social cooperation.
29. Female monopolization of child care.
30. Political organization and leadership.
31. Social norms and values, and punishment for violations; sense of right and wrong; etiquette.
32. Standards of sexual modesty, sex generally in private, fondness for sweets, food taboos, discreetness in elimination of bodily wastes.
33. Ingroup-outgroup antagonisms.
34. Religion.
35. Worldviews.
36. Rituals.
37. Concepts of property.
38. Music and dance.
39. Art.
40. Symbolism.
41. Ethnocentrism.
42. Modes of economic production, distribution, and exchange.
43. Birth control and population regulation.

Alice Rossi (1984) argues that a pattern of social behavior can be suspected of having a biological basis if two or more of the following criteria are met:

1. There are consistent correlations between a behavior and a physiological sex attribute (body structure, sex chromosome type, hormonal type).
2. The pattern is found in infants and young children prior to the occurrence of major socialization influences, or the pattern emerges with the onset of puberty.
3. The pattern is stable across cultures.
4. Similar behavior patterns are found across species, especially the higher primates.

As we shall see in the chapters ahead, there are many patterns of human social behavior that meet at least two of these criteria and a fair number that meet all four.

TWO CONTROVERSIES

The Level of Selection

Our discussion thus far has clearly implied that sociobiology views the unit of selection as most commonly the genes contained within organisms (while not discounting evolution at the level of the individual organism itself). And, indeed,this is so. Darwin himself saw selection occurring at the level of the organism, which was why altruistic behavior puzzled him. In the early 1960s V.C. Wynne-Edwards (1962) set forth an argument for selection occurring at a higher level, or for *group selection.* Wynne-Edwards argued that organisms and species have the traits they do because these traits have evolved to benefit entire populations. For example, he explained the roosting behavior of birds as a behavior that allows a bird population to assess its population density and adjust it accordingly so that the entire population benefits. Although Wynne-Edwards is commonly cited as the chief proponent of group selection at this time, a number of evolutionary biologists in fact accepted it, at least as one possible mode of selection (Segerstråle, 2000).

However, just four years later the evolutionary biologist George Williams published a famous book, *Adaptation and Natural Selection* (1966), in which he claimed that adaptation should never be explained at a level higher than that truly necessary and that most selection occurred at the level of the gene or the individual. He did not rule out the possibility of group selection, but thought it extremely rare. Williams's argument was echoed a decade later by Richard Dawkins, whose equally famous book *The Selfish Gene* (1976) helped to launch sociobiology. Dawkins argued that selection occurred basically at the level of the gene, and that organisms were largely vehicles for getting genes passed from one generation to the next: An organism is just a gene's way of making another gene.

The ideas of Williams and Dawkins have become widely accepted throughout evolutionary biology and sociobiology and are essentially the received wisdom. But the idea of group selection has not gone away. It has been pushed most vigorously by David Sloan Wilson and Elliott Sober (1994; Sober and Wilson, 1998). Wilson and Sober argue that selection is a complex multilevel phenomenon, but it is clearly evolution at the level of the group as a whole that interests them most. Much of human social life, they claim, can only be explained as the result of processes of between-group selection. They note, for example, that in tribal societies there is often relentless competition between lineages, which "compete against each other as corporate units, much as we expect individuals to compete when they are allowed to act as free agents" (Sober and Wilson, 1998:174). In discussing social norms, they hold that "most traditional human societies appear designed to suppress within-group processes that are dysfunctional for the group, and as a result natural selection has operated and adaptations have accumulated at the group level. Human social groups often function as adaptive units and are perceived as such by indigenous people and the ehnographers who study them" (Sober and Wilson, 1998:192). Sober and Wilson also refer to Raymond Kelly's (1985) work on the interaction between the Nuer and the Dinka, African pastoral societies. Conflict between these societies was gradually resolved in favor of the political supremacy of the Nuer, and for Sober and Wilson this could only have been because of between-group selection. The Nuer were adaptively superior *as a group,* and this is why they prevailed. The authors are convinced that some human groups are

so well designed at the group level that they had to have evolved by group selection; nothing else could explain such excellent design. Another sociobiologist to invoke group selection is Kevin MacDonald. In a series of works (MacDonald, 1994, 1998a, 1998b), he has argued that the Jews have been one of the world's most successful ethnic groups because they have followed what he calls a "group evolutionary strategy." Individual Jews have made sacrifices in favor of the group, and the group has prospered as a result.

Few sociobiologists have found arguments like these convincing, and I certainly agree. The mistake these authors are making is a failure to draw a very simple distinction between behavior *that evolves because it benefits the group* and behavior *that has the consequence of benefitting the group.* They provide no evidence to show that the former is occurring; in their examples, the latter is in fact occurring, but only because the groups in question are aggregations of individuals. The group benefits simply because the individuals within it benefit. Sober and Wilson and MacDonald reify the group in that they see it as having a distinct existence from its constituent members.

Most sociobiologists, then, reject the notion of group selection and see selection occurring at the level of genes or individual organisms. Indeed, it is not hard to see why selection should be occurring at this level. Genuine altruism cannot constitute what John Maynard Smith (1974, 1982) has called an *evolutionarily stable strategy.* A population of genuine altruists could not remain stable because it could always be invaded and displaced by selfish strategists. Organisms that sacrifice their genes for the good of others that do not share them will find that their genes for genuine altruism will quickly disappear from the gene pool for the simple reason that the altruists will be out-reproduced by organisms that are reproductively selfish (Low, 2000). And not only is the concept of group selection logically dubious, but there is no real empirical evidence to support it. As Bobbi Low (2000:160) has remarked, "If group good at the expense of individual fitness were relatively powerful, we should expect genetic sacrifice to be common. In fact, it is so rare as to be undetectable in populations of any organism."

Darwinian Psychology versus Darwinian Social Science

Sociobiologists stress the *adaptive* character of many features of human behavior, i.e., that behavior evolves because it is useful in promoting the reproductive interests of each individual. Many sociobiologists take the view that *adaptations pertain only to the Environment of Evolutionary Adaptedness, or EEA.* This ancestral environment is the one in which modern humans evolved over the past 100,000-200,000 years, during which time they lived exclusively in small hunter-gatherer groups. The argument is that our neurobiology evolved to adapt humans to the ancestral environment, but in the last 10,000 years there has been enormous social evolution and today people live in extremely complex, state-level societies, many of them highly industrialized and urbanized. As a consequence, there is no reason to assume that their behavior will necessarily be adaptive under these conditions. Those sociobiologists holding this view have taken to calling themselves *Darwinian psychologists* or *evolutionary psychologists*, even though many of them are not affiliated with the discipline of psychology (Symons, 1989, 1990, 1992; Daly and Wilson, 1984, 1986; Barkow, 1989; Tooby and Cosmides, 1990).

To a large extent the Darwinian psychologists are reacting against another group that has been called *Darwinian anthropologists* or *Darwinian social scientists* (e.g., R. Alexander, 1990; Turke, 1990), which in fact probably includes the majority of social scientists working today under the broad label of sociobiology. The Darwinian social scientists assume that people in modern environments are, just like their distant ancestors, striving to maximize their inclusive fitness and thus that most behavior is adaptive even under these conditions. This contrasts with the claim of the Darwinian psychologists that it is a waste of time to try to ascertain whether people in modern environments are trying to maximize their inclusive fitness. Because these environments are very different from the ancestral environment, there is little reason to assume that behavior that evolved in the EEA would continue to be adaptive. Moreover, what do the Darwinian social scientists do if they discover that people in modern environments are not acting to maximize their reproductive success? Do they thereby reach the negative conclusion that sociobiology cannot be applied to behavior in such environments? They should not, since it can still be argued that

people are behaving in accordance with evolved psychological mechanisms - ancient adaptations - that drive their behavior. For example, men in modern industrial societies still appear to seek sexual variety, but they do not wish to impregnate the women with whom they are having extramarital affairs, for this would lead to unwanted complications in their lives. Therefore, they are not seeking to maximize their reproductive success but rather are behaving in accordance with a psychological mechanism - "mate with attractive women you haven't mated with before" - that would have promoted reproductive success under ancestral conditions. Donald Symons (1989, 1990, 1992), one of the leading proponents of Darwinian psychology, claims that what we should be trying to study is the nature of *evolved psychological mechanisms*, not behavior. As he puts it, (1989:137; emphasis added), "In the study of adaptation, the key issue is not whether or not a given phenotypic feature influences reproductive success, but rather *whether differential reproductive success historically influenced the form of the phenotypic feature.*"

Important replies to the argument of the Darwinian psychologists have been made by Richard Alexander (1990) and Paul Turke (1990). Alexander agrees that it is important to study adaptive design but argues that it cannot be studied except by direct reference to behavior: "Underlying adaptive design cannot be studied directly. One studies behavioral and other outcomes, judges their reproductive significance . . . and then infers the underlying physiological and morphological designs. Information about psychological and physiological mechanisms can be gained only by observing ultimate expressions of the phenotype" (R. Alexander, 1990:247). Turke makes a similar point and also notes that, in fact, much human behavior in environments other than the ancestral environment has been shown to be reproductively adaptive. Indeed, "notwithstanding the *potentially* disruptive effect of environmental novelty, because long-term directional and stabilizing selection are widely evident in all forms of life, it generally is reasonable to at least consider hypotheses that hold that the mechanisms underlying behavior, as well as the behavior itself, will develop in a manner that leads to adaptive outcomes" (Turke, 1990:316). Turke goes on to argue that it is important to determine when behavior in modern environments is or is not adaptive, because this throws important light on the nature of underlying psychobiological mechanisms.

My own position on this unresolved debate is that the Darwinian psychologists are suggesting a position that is too narrow and restrictive. We certainly need to be sensitive to the fact that a good deal of behavior in modern social environments may be nonadaptive or even maladaptive and only explicable in terms of adaptations that arose in the ancestral environment. Nonetheless, Turke makes a convincing point when he argues that much modern behavior is adaptive and that the study of modern adaptiveness vs. nonadaptiveness is important in yielding an understanding of underlying psychobiological mechanisms (cf. Janicki and Krebs, 1998). Charles Crawford (1998) argues that most environments since the ancestral environment probably have not differed that much from it in terms of the production of adaptive behavior. As Crawford notes, since the social changes over the past 10,000 years have been the result of human action, it is reasonable to assume that these changes have reflected human limitations and predilections and thus have been adaptive products. Most sociobiologists follow in the tradition of Darwinian social science, and in later chapters we shall examine many of the research findings they have produced that demonstrate the adaptiveness of much modern behavior.

SOCIOBIOLOGY AND THE PROBLEM OF PREFERENCES

In Chapter 6 I argued that because rational choice theory appears unable to deal with preferences we will have to turn to other theoretical approaches for help in this regard. It was suggested that the most promising approach for the identification of basic human preferences is sociobiology. As Eric Alden Smith and Bruce Winterhalder (1992a:49-50) have pointed out, considerable evidence "suggests strongly that some significant portion of the preferences and beliefs exhibited by humans in diverse times and places have been shaped directly or indirectly by natural selection." Larry Arnhart (1998) has identified twenty basic "categories of desire" that he believes are a fundamental part of the human biogram:

1. *A complete life*: Humans generally desire life, and a complete or long life, and can only be fully happy if they live out their full life-span.

2. *Parental care*: Humans generally desire to care for their children, and children desire the care of adults. Despite the burdens of child care, parents are normally highly motivated to provide it.
3. *Sexual identity*: Sex is the most important dimension of personal identity, and humans strongly desire to categorize themselves as male or female. Women tend to be more nurturant than men, and men are more inclined than women to attain dominance and seek high-status positions.
4. *Sexual mating*: Humans strongly desire sexual coupling, and every society displays intense interest in sexuality. Men generally prefer to mate with young, attractive women, whereas women seek to mate with men who have high status and economic resources.
5. *Familial bonding*: Humans generally desire to live within families, the core of which is a mother with her children. All societies provide some arrangement for marriage, and kin relations are among the most important relations in every society, if not the most important.
6. *Friendship*: Humans generally seek social relationships based on mutual affection and shared interests, and humans can have enduring friendships with only a few people.
7. *Social ranking*: Humans generally seek social recognition through ranking in comparison with others, and they attain status by way of gaining prestige, honor, and fame.
8. *Justice as reciprocity*: Humans have a natural sense that justice requires returning benefit for benefit and injury for injury. Humans are inclined to feel the emotions of gratitude, love, and benevolence in response to the benefits conferred on them by others.
9. *Political rule*: Humans are political animals by nature; they have a natural tendency to struggle for power and control.
10. *War*: Humans generally desire to engage in war when such a course of action will advance their interests.
11. *Health*: Humans generally desire to live lives that provide adequately for their bodily needs. Much of social life is devoted to satisfying the desires that are fundamental to a healthy life.
12. *Beauty*: Humans generally desire beauty in the human body, and esteem the bodily signs of health and vigor. They adorn their bodies for pleasing display, and men generally prefer women whose bodies show signs of youth and nubility.
13. *Wealth*: Humans generally desire the economic goods necessary for a healthy and flourishing life.
14. *Speech*: Humans generally desire to communicate about themselves and their world, and children are naturally adapted to learn the language of their group or society.
15. *Practical habituation*: Humans are creatures of habit, and it is through this that they seek to manage their appetites and passions.
16. *Practical reasoning*: Humans seek to deliberate in a rational manner about what a good life is and to organize their actions to conform to their notion of a good life.
17. *Practical arts*: Humans generally desire craftsmanship.
18. *Aesthetic pleasure*: Humans desire and receive pleasure from their own artistic creations and the natural environments in which they live. Humans take pleasure in such activities as singing, dancing, playing musical instruments, painting, and decorating objects. They also take pleasure in the natural landscapes that resemble the environments in which they first evolved.
19. *Religious understanding*: Humans generally desire to understand the world by means of postulating the actions of supernatural powers.
20. *Intellectual understanding*: Humans generally desire to understand the world through the use of the intellect in ways quite apart from religious understanding.

Arnhart claims that these twenty categories of desire "are universally found in all human societies, that they have evolved by natural selection over four million years of human evolutionary history to become components of the species-specific nature of human beings, that they are based in the physiological mechanisms of the brain, and that they direct and limit the social variability of human beings as adapted to diverse ecological circumstances" (1998:36). As we can see, this list of categories overlaps significantly with the list of human universals discussed earlier. Although Arnhart probably considers his list exhaustive, I can think of at least two important categories of desire that he omits. Everywhere humans display a desire to minimize toil and to carry out burdensome activities by minimizing their expenditure of time and energy, a

basic principle that has been called the Law of Least Effort (Zipf, 1965). This seems to be an extremely important limiting factor in all human societies, and I would add it as a twenty-first category of desire. In addition, people have an extremely strong desire for group identity, especially as it is expressed in terms of cultural, ethnic, or national affiliations (van den Berghe, 1981a; Reynolds, Falger, and Vine, 1986). This can be added as a twenty-second category of desire. Many of these categories of desire will be explored at length in the chapters to come.

THE ANTISOCIOBIOLOGICAL REACTION

As soon as sociobiology proper got started with the publication of E.O. Wilson's book in 1975 it immediately engendered a storm of controversy. It was attacked as pushing a reactionary phiolosophy and as being just a new form of social Darwinism. At the annual meetings of the American Association for the Advancement of Science in Washington in 1978, E.O. Wilson was the target of some of the antisociobiological crowd. As he was speaking, someone ran up to him and dumped a pitcher of water on his head, and at the same time several people stood up in the audience denouncing sociobiology, saying that it should not be taught in universities (Pines, 1978). This last point has been uttered many times by others.

Some of the best-known critiques of sociobiology have been made by the Sociobiology Study Group of Science for the People (1976), Marshall Sahlins (1976a), Lewontin, Rose, and Kamin (1984), and Marvin Harris (1979). The Sociobiology Study Group of Science for the People is a group of academics, many of them political radicals, that includes such prominent scholars as the paleontologist Stephen Jay Gould and the geneticist Richard Lewontin, both Marxists in political outlook. This critique focused mostly on E.O. Wilson, the "founder" of sociobiology and its leading representative at that time. The critique's basic claim, repeated over and over, is that sociobiology is just another in a long line of biological determinisms whose main aim is to justify the status quo and legitimize present and past social arrangements. Wilson was accused of basically engaging in political advocacy shrouded in science. The critics charged that sociobiologists' claims for social behavior bear a remarkable similarity to modern market-industrial-entrepreneurial society, with its dog-eat-dog competition and search for profits, power, and status; that no evidence is offered by sociobiologists, beyond the arbitrary postulation of genes, for their claims for the biological basis of behavior; that there is no evidence whatsoever for a genetic basis for social behavior; that sociobiology grossly overemphasizes the role of adaptation in human behavior; and that sociobiology is really a deeply conservative political ideology. They also argued against sociobiological claims for a biological basis to warfare, holding that warfare in band and tribal societies is seldom lethal to more than a few individuals. Warfare in such societies, they said, generally takes the form of minor skirmishes, and the wounding of one individual may be sufficient to call a halt to the "war." As we shall see, all of these claim are false, most of them badly so.

Marshall Sahlins's (1976a) critique, published only a short time later, consisted of two main parts. First, he tried to demolish kin selection theory. He argued that the ethnographic record shows that human kinship systems are actually organized in ways that run completely counter to the predictions of kin selection theory. Kinship, he said, is organized culturally - by social definitions of who is or is not close kin - rather than by actual genetic relatedness. Culture can make a genetically distant relative a close kinsman and vice versa. Second, Sahlins claimed that sociobiology is an intellectual product that has been deeply shaped by the capitalist society of which it is a part. Sociobiologists take the competitive, highly acquisitive nature of modern capitalist society, which is a distinctive and unique type of society, and project it onto all of human society and all of human nature. Thus, sociobiology suffers from a serious ideological bias. Evolutionary biology has degenerated into ethnocentrism. Sociobiologists apply "the model of capitalist society to the animal kingdom", then reapply "this bourgeoisified animal kingdom to the interpretation of human society" (Sahlins, 1976a:101). Sahlins quoted from a letter from Marx to Engels: "It is remarkable how Darwin recognizes among beasts and plants his English society with its division of labor, competition, opening up of new markets, 'inventions,' and the Malthusian 'struggle for existence.'" However, what

Sahlins failed to recognize is that *Marx did not conclude from this that Darwin was therefore wrong.* Marx actually had great admiration for Darwin and generally accepted his theory of evolution by natural selection. He thought Darwin was right despite the fact that his ideas may have been stimulated by the nature of the society in which he lived.

Sahlins's critique, as we shall see, misses the mark widely. The theory of kin selection is not nearly as easily demolished as he imagines, and his notion that sociobiology has simply projected the character of capitalist society onto nature and then reprojected this assumed nature back onto society is exceedingly simplistic and rings hollow. What then of the critique by Lewontin, Rose, and Kamin (1984)? Their critique has much the same strident political tone as the Science for the People critique. Sociobiology is charged with being a biological determinism bent to the service of an ideology that attempts to legitimize inequality. Remarkably, the authors claim that there is no evidence for the actions of genes on behavior! It would seem that these authors would like nothing more than to ban sociobiology from universities, and if they could get away with it they probably would. One of the most obvious things about this critique is that it is Lewontin, Rose, and Kamin themselves who are pushing a political ideology, not the sociobiologists. There is very little evidence that sociobiologists are motivated by anything except the desire to attain scientific truth, but the evidence is overwhelming that Lewontin, Rose, and Kamin are politically motivated. They are simply beside themselves with fury over what they see as any form of biological determinism. As Vincent Falger (1984:131) has noted, "the radical critics, who make great fuss of undesirable political use of sociobiology, themselves are the main and not least dangerous ideological abusers of sociobiology."

Marvin Harris's critique of sociobiology is greatly overblown and, I shall argue, quite unnecessary, but it at least has the merit of being a scientific critique largely devoid of political motivation or invective. Harris's objections to sociobiology can be more or less summed up in the following way. First, Harris does not deny that there is such a thing as human nature; it exists and social scientists should try to describe it. However, he claims that even if sociobiological claims about human nature are correct, this would at most allow the sociobiologists to provide an understanding of the similarities between societies and could shed no light on their many important differences. This is exactly what I thought myself a quarter-century ago when I first encountered sociobiology, but the work of many sociobiologists since that time has convinced me that this notion is wrongheaded. As noted earlier, much of what is in the human biogram consists of facultative traits that cause people to respond in different adaptive ways to different environments. Second, Harris believes that the sociobiologists are committed to expanding the size and scope of human nature, whereas cultural materialists attempt to reduce the content of human nature to the smallest possible inventory of items. Harris claims that, in fact, human nature consists of only a few basic traits. These traits, which Harris calls biopsychological constants, are that people need to eat and prefer nutritious and high-protein diets with adequate calories, prefer to carry out tasks by minimizing their expenditure of time and energy, are highly sexed and generally prefer heterosexual sex, and need love and affection and will strive to increase the amount of those things that others will give them. Harris analyzes 13 characteristics of human nature identified by Wilson (1977) and attempts to show that these traits are not under any genetic control. Harris (1979:134) comments that the

> attempt by sociobiologists to add what are at best dubious and hypothetical genes to the human behavioral repertoire leads to the misrepresentation of human nature based on an erroneous construal of the course of human evolution. . . . Even if the dubious hypothetical genetic predispositions of sociobiological human nature actually do exist, knowledge of their existence can lead only to an understanding of the outer "envelope" (to use a metaphor proposed by Wilson . . .) within which cultural evolution has thus far been constrained. It could not lead to an understanding of the differences and similarities within sociocultural evolution.

Harris agrees that humans have a nature, but, apart from the biopsychological constants just mentioned, this nature is basically limited to their extraordinary capacity for culture and learning made possible by their large brains and their linguistic abilities. As I will argue in the next chapter, the evidence is rapidly mounting against the notion that the human brain is just some sort of general, all-purpose mechanism which makes humans solely dependent on learning for adaptation, and that it is in fact a complex composite of special-purpose mechanisms.

In his book *Sociobiology: Sense or Nonsense?* (1985) Michael Ruse, a philosopher of biology, has summarized many of the criticisms and defended sociobiology against most of them. Ruse argues that the political charges are highly unfair and wrongheaded. For all the talk of differences, Ruse says, it is noteworthy that the sociobiologists are really emphasizing the unity of mankind with their search for universal human nature. Ruse also denies that sociobiology is tainted with the ideology of Western capitalism. The critics are reading things into sociobiology that are not there, and are, sad to say, hysterical and paranoid. Sociobiology does not require anyone to endorse any aspect of Western ideology. Many sociobiologists are women who are active feminists. No one is saying that because evolution has worked in a particular way that it is therefore good. No sociobiologist is trying to say that some groups are inferior to others, or that men are superior to women. Regarding gender, sociobiologists would simply say that the sexes differ, not that one is better than the other. And sociobiologists do not say that people have to stay in the roles for which they were selected in biological evolution. Moreover, sociobiology does not paint a picture of humans as nothing but selfish individuals. It also stresses reciprocal altruism, cooperation, and people's conceptions of fairness. Human nature makes people cooperative just as it makes them competitive.[3]

Sociologists have probably been more antisociobiological than any other social scientists. I have long observed very strong sentiment against it among sociologists everywhere. In the Sanderson and Ellis (1992) study, sociobiology was shown to have a pitiful representation among sociologists, with only about 2.5 percent of sociologists identifying with it in any way. In the same study the national sample of sociologists was asked 12 questions dealing with the degree to which they thought biological factors were important determinants of human behavior. The 12 questions dealt with the following dimensions of human behavior: sexual orientation, delinquency and minor crime, serious crime, alcoholism, academic achievement, lifetime earnings, race differences in academic achievement, attitudes toward racial and ethnic minorities, marital stability, sex differences in occupational interests, sex differences in aggressive crimes, and sex differences in child nurturance. The results showed very little support for biological factors. The sociologists were most apt to see sexual orientation, alcoholism, and academic achievement as having biological determinants, but they only saw biology playing a minor role (about 27% of variance explained). Across all 12 dimensions the respondents saw biological factors contributing only about 14 percent of the total variance, thus giving social and cultural factors an overwhelming 86 percent of the explained variance. In a more recent study of sociological theorists, Jane Lord and I (Lord and Sanderson, 1999) discovered extremely weak support for sociobiology. We found that only about one percent of theorists identified with it as a primary perspective, and that only another one percent claimed it as a secondary perspective.

Considerable evidence suggests that psychologists and anthropologists are a good deal more sympathetic to sociobiology. Leonard Lieberman (1989) has surveyed cultural anthropologists' degree of support for certain key sociobiological ideas. He asked them whether they thought that certain key notions were useful for research. Here is what he found:

- kin selection: 35% agree, 43% disagree
- reciprocal altruism: 29% agree, 52% disagree
- genetic basis of altruism: 18% agree, 58% disagree
- male-female reproductive strategy: 44% agree, 30% disagree
- gene-culture coevolution: 46% agree, 26% disagree
- average: 34% agree, 42% disagree.

These cultural anthropologists were almost as supportive of sociobiology as negative toward it; sociologists would probably show nowhere near this kind of support if they were asked these same questions.

As for psychologists, they are much more prominently represented than sociologists as authors of books and articles written from a sociobiological perspective. As noted earlier, sociobiologically oriented psychologists call what they do *evolutionary psychology*. Evolutionary psychologists founded the major scientific society for sociobiology in North America, the Human Behavior and Evolution Society, and they are currently its most numerous representatives and leading figures. Two textbooks in evolutionary psychology have already appeared (Buss, 1999; Gaulin and McBurney, 2000), as has a handbook (Crawford

and Krebs, 1998). Why are anthropologists and psychologists more supportive of evolutionarily oriented social science? Part of the answer may be that they are much better trained in human biology than sociologists. But perhaps the real question is why sociologists are so hostile? The answer, I think, has little to do with science and mostly to do with politics and ideology. Most people who go into sociology want to change the world, and that is their motivation for becoming sociologists. Such people are ideologically convinced in advance that human behavior has little to do with biology. They believe this, and they dearly want to believe it, because they (incorrectly) see the acknowledgement of biological factors as indicating that behavior is resistant to fundamental change. So sociologists dislike sociobiology because they are severely threatened by it politically, and it must be said that sociology has become an increasingly politicalized discipline over the past two or three decades.[4]

The importance of ideology in the rejection of biological arguments has been demonstrated decisively by Carl Degler in his *In Search of Human Nature: The Decline and Revival of Darwinism in American Social Thought* (1991). Degler shows that in the late nineteenth and early twentieth centuries Darwinism was highly regarded by social scientists, and biology was considered a major underpinning of human behavior. By the 1920s the tide had begun to turn away from Darwinism and biology, and by the 1930s a cultural determinist or environmentalist position had triumphed and biology was pushed aside. And, Degler says, the reasons for this triumph were ideological: Social scientists wanted a more just and egalitarian society, and arguments for biological causation were perceived as inimical to this. It was not empirical research that was the basis for the triumph, because the data were highly equivocal.

Critics of sociobiology often assert that it is dangerous because it can be misused. It is impossible to deny this, but any social theory can be misused, and many have been. No social theory has been more misused and abused than Marxism (cf. Courtois et al., 1999), which is the preferred stance of many critics of sociobiology. And Skinnerian behaviorism, one of the best-known versions of the SSSM, has enormous potential for mischief, as the novel and film *A Clockwork Orange* showed. In a similar vein, Kevin MacDonald (1998b) has argued, in a most compelling fashion, that the real political danger comes from radical environmentalist rather than sociobiological theories of society. In his words (1998b:40):

> A theory that there is no human nature would imply that humans could easily be programmed to accept all manner of exploitation, including slavery. From a radical environmentalist perspective, it should not matter how societies are constructed, since people should be able to learn to accept any type of social structure. Women could easily be programmed to accept rape, and ethnic groups could be programmed to accept their own domination by other ethnic groups. The view that radical environmentalism is not socially pernicious also ignores the fact that the communist government of the Soviet Union murdered millions of its citizens and later engaged in officially sponsored anti-Semitism while committed to an ideology of radical environmentalism.

We should not refuse to construct social theories just because there is the possibility they will be misused. Actually, I would claim that, if the sociobiologists are right, then failure to acknowledge what they are saying is a sure way to guarantee that we will never be able to change society in ways that we want. We can only change society properly if we recognize the kinds of biological constraints on human behavior that likely exist. If we do not recognize them and they are important, then we will surely fail in our change efforts. It will not do any good to put our heads in the sand and deny the importance of biology just because we are afraid of the possible consequences. To change something one must have a good understanding of what that something is like and what would be involved in trying to change it.

There is another threat perceived by sociologists, though, that we need to recognize. Most sociologists believe that their claim to importance is to show that social and cultural forces shape everything. They seem to feel that without this they have nothing to distinguish themselves and make themselves important. We might call this sociologists' "Durkheimian mandate": social facts can only be explained in terms of other social facts. Stressing the importance of biology, they think, undermines this, and robs their discipline of its unique importance. Thus sociologists feel threatened disciplinarily (Udry, 1995). As Pierre van den Berghe (1990:180) has noted, "Antireductionism is a territorial display of sociologists, especially against their nearest intellectual rivals, the psychologists." However, sociologists are going to have to change their stripes on this matter or be at serious disciplinary risk in the near future (Udry, 1995; Ellis, 1996). The

evidence in favor of sociobiological arguments is growing by leaps and bounds, and most of it is unknown to sociologists because they do not bother to read the books and journals containing this information. If sociologists continue to deny the importance of biology, within 20 years or so, by which time the evidence for biology will have become much more massive, they are going to look increasingly foolish both within the academy and to the larger educated public. They will risk becoming seriously marginalized, if not destroyed, as a discipline.

My grand conclusion is that sociobiology has a great deal to teach us. It is an extremely provocative approach that has produced some extraordinarily important results. But it has limits, too. Sociobiological approaches work best in the areas of sex, gender, kinship, and family life, for reasons that are obvious. It can contribute to other areas of human behavior, but it can go only so far. It can contribute little, for example, to the study of macrodynamics and long-term historical change, except by way of showing how biology constrains the kinds of changes that are likely or unlikely to occur (see Sanderson, 1999:403-28). Social and cultural factors, themselves built up over the generations and millennia as the result of biological influences, also matter greatly and must be given their due as frameworks constraining human action. Sociobiologists, though, have never denied this. Any good social scientist should take a *biosocial* perspective, one that shows the complex interaction of biology and society in human behavior. The main problem with sociobiology is simply that it is an incomplete approach. Although it is an excellent foundation on which to build a general theory of human society, by itself it is not enough. Much more is happening in social life than the efforts of individuals to maximize their inclusive fitness. Sociobiology needs to be combined in a very selective way with those other social theories that have worked best. The achievement of that objective is my next aim in this book.

NOTES

1. Some of the most important general works in sociobiology are Barash (1977), Daly and Wilson (1978), Dawkins (1976), R. Alexander (1979), Symons (1979), Lopreato (1984, 1989), Trivers (1985), Ruse (1985), Barkow (1989), Barkow, Cosmides, and Tooby (1992), Badcock (1991), Maxwell (1991), Tooby and Cosmides (1989a, 1990), Nielsen (1994), Crippen (1994), Crawford and Krebs (1998), Lopreato and Crippen (1999), Buss (1999), Low (2000), Cronk, Chagnon, and Irons (2000), and Gaulin and McBurney (2000). A collection by Betzig (1997) reprints many classic articles. Ullica Segerstråle (2000) has written an excellent history of sociobiology and the debates and controversies that have centered around it. Her long work is packed with juicy tidbits of inside information often obtained in interviews with the principals. The authors of some of these works prefer the label *evolutionary psychology* rather than sociobiology, a distinction that will be explained later in this chapter. The works by Crawford and Krebs, Buss, and Gaulin and McBurney, for example, are intended as handbooks or textbooks in evolutionary psychology, which is regarded by these authors as a paradigm within the discipline of psychology. It should be noted that some evolutionary psychologists are not affiliated with the discipline of psychology; such scholars are most likely professional anthropologists. Cronk, Chagnon, and Irons (2000), cited above, prefer the name human behavioral ecology, but it is very similar to sociobiology.
2. A more complete understanding of sex differences in reproductive and mating strategies requires an understanding of the neo-Darwinian concepts of sexual selection and parental investment. These concepts are discussed in later chapters as they become more directly pertinent.
3. Concerning the alleged politics of sociobiology, Pierre van den Berghe (1981; quoted in Segerstråle, 1992:201) has said the following: "Actually, a review of the politics of leading sociobiologists would lend more credence to the contention that sociobiology is a Communist conspiracy: J.B.S. Haldane, who is generally credited for having first hit on the notion of kin selection – a theoretical cornerstone of sociobiology – was a leading member of the British Communist Party; so was John Maynard Smith. E.O. Wilson and most other leading sociobiologists are left-of-center liberals or social democrats. 'Racist' Trivers is even married to a Jamaican and is heavily involved in radical black politics." Van den Berghe, the leading sociobiologist among sociologists, has long been a foe of racism and social inequality, and I suspect that if the members of the Human Behavior and Evolution Society and the European Sociobiological Society, the two leading associations of sociobiologists in the Western world, were polled, most of them would be shown to be left-of-center politically, some of them strongly so. In reading the works of sociobiologists for a quarter of a century my impression has always been that their writings are

remarkably free of any political content whatsoever, suggesting that their overwhelming aim is scientific understanding, not political action of any kind.

In a closely related vein, Ullica Segerstråle (1992) tells the following story. In May of 1976 the Sociobiology Study Group of Science for the People held a meeting at which they tried to persuade Noam Chomsky, a well-known political radical, to write a statement strongly denouncing sociobiology. The group's members discovered, however, much to their chagrin and embarrassment, that Chomsky was actually in favor of the view that there is such a thing as a constant human nature. Moreover, Chomsky thought that the postulation of human nature would actually be helpful to the radical cause in fighting for a better society. As Segerstråle points out, under such circumstances it can hardly be surprising that Chomsky was unwilling to write a critique of sociobiology. Peter Singer (1999), a philosopher with Marxist sympathies, takes the same position as Chomsky – that only by understanding human nature can leftists hope to build the kind of society they desire.

4. As noted earlier, this is what has motivated the critiques of Science for the People, Lewontin, Rose, and Kamin, and to some extent Sahlins. The prominent paleontologist Stephen Jay Gould, who has written many widely read books on Darwinism and evolutionary theory in general, has also been a major ideological opponent of sociobiology (see Alcock, 1998 and Segerstråle, 2000). Gould is a prominent member of Science for the People and was actively involved in writing their ideologically driven critique of sociobiology in 1976. Obviously one does not have to be a sociologist to be ideologically opposed to sociobiology. Segerstråle's (2000) history of sociobiology discusses the ideological opposition to it at great length, although she takes the stance, which I strongly question, that even the ideological opponents of sociobiology were motivated to criticize it more on the basis of their view of good science than their political views.

Chapter 10

Reproductive Behavior

The natural man has only two primal passions, to get and to beget.

Sir William Osler

For the woman, the man is a means: the end is always the child.

Friedrich Nietzsche

Literature is mostly about having sex and not much about having children. Life is the other way around.

David Lodge

This chapter is devoted to showing how biomaterialist, ecomaterialist, and even polimaterialist theories are all necessary to make sense out of some important dimensions of human reproductive behavior. Human reproductive strategies are highly facultative, and thus are the product of complex interactions between biological predispositions and a range of environmental conditions.

SOCIAL STATUS AND REPRODUCTIVE SUCCESS

Lee Ellis (1995) has conducted an exhaustive review of research (hundreds of studies) on the relationship between dominance and reproductive success in a wide range of animal species. Reproductive success for males is measured among animals in terms of number of copulations, number of copulations with estrous females, or number of offspring sired. Usually it is the first of these.

Ellis's main findings may be summarized as follows: Ignoring both sex and species, about 75 percent of studies using primary indicators of reproductive success report a positive relationship between dominance and reproductive success, and about 80 percent of the studies report a positive relationship when secondary indicators of reproductive success are used. For males only, the findings are stronger. For nonprimate males, 89 percent of studies using primary indicators and 96 percent of studies using secondary indicators report a positive relationship. For nonprimate females, the corresponding numbers are 74 percent and 88 percent. For primate males, the numbers are 69 percent and 96 percent, and for primate females the numbers are 63 percent and 69 percent.

Geary (1998) summarizes research which shows that in one group of wild savannah baboons a single male sired 81 percent of the offspring born during a four-year span. In the years preceeding and following his reign, he fathered only 20 percent of the offspring. Research on three groups of wild long-tailed macaques shows that the dominant male fathered between 52 and 92 percent of the offspring, but the low-ranking males collectively sired only between 2 and 9 percent of the offspring.

Ellis goes on to elaborate a theory as to why some studies found no relationship between dominance and reproductive success. He argues that most of the variance in the relationship results from differences in resource availability. When resources are extremely plentiful, or when they are very difficult to monopolize, there should be little relationship between dominance and reproductive success. It is when resources become scarce, or when they are highly concentrated or clumped, that dominance has an

important relationship to reproductive success. Under these conditions, the dominant males tend to exert more control over resources and can use this control to attract mates.

There is abundant evidence that social status and reproductive success are closely linked in humans as well. Most human societies are polygynous, and it is most often the dominant males who have the most wives. This is an extremely common pattern in all types of societies all over the world and throughout history. As Pérusse (1993) observes, social status and reproductive success has been observed for such widely diverse societies as the Yomut Turkmen, rural Trinidadians, the Ifalukese of the Western Pacific, sixteenth-century Portuguese, the Kipsigis of East Africa, nineteenth-century Englishmen, and Swedish and German peasants. Indeed, Bobbi Low (2000) points out that there are at least 100 carefully studied societies in which high male status and reproductive success are closely associated. There is every reason to think that this is a true human universal or at least a near universal. (See Pérusse, 1993, and Low, 2000, for lists of studies.)

Laura Betzig (1986) notes that men compete for women in all societies, that women are the chief source of conflict between men in many societies, and that it is the dominant males who gain access to more women. The famous Shinbone of the Yanomamö had 43 children. Shinbone's father had 14 children, 143 grandchildren, 335 great-grandchildren, and 401 great-great-grandchildren. According to Betzig, "the evidence is overwhelming that rich and powerful men do enjoy the greatest degree of polygyny cross culturally" (1986:34). Betzig closely examined 18 highly stratified societies that she calls "despotic societies." In all of these societies rulers were highly polygynous, many having large harems of nubile women. In Dahomey there was a close parallel between the social hierarchy and the reproductive hierarchy. In ancient Israel, men of royal status had many wives and concubines. King Solomon was reported to have had 700 wives and 300 concubines. Among the Zande of Africa the king had at least 500 wives, and even chiefs had anywhere from 30 to 100 wives. Among the Inca of Peru, there was a direct correlation between rank in the political hierarchy and the number of wives a man had. "Principal persons" got 50 women, leaders of vassal nations were allowed 30, heads of provinces of a hundred thousand people received 20, leaders of a thousand people got 15 women, administrators of five hundred people got 12 women, and so on down to the "poor Indian," who took anyone who might be left. It is in these highly stratified preindustrial societies that polygyny is greatest. Using a sample of 99 preindustrial societies, Betzig found a correlation of .719 (Pearson r) between the degree of despotism and the degree of polygyny. The more power men have, the greater the number of wives they can command, and thus the greater their reproductive success. In societies where the power differentials between men are small, the most dominant men usually have only three or four wives.

It is commonly argued that in modern industrial societies there has been something of a reversal of the relationship between status and reproductive success. Higher-status individuals in modern societies seem to have fewer children, on average, than those of lower status. How can we explain this apparent violation of the general principle that social status leads to reproductive success? Some, such as Vining (1986) suggest that in modern societies people gain increased interest in creature comforts, and that children interfere with these enjoyments. This kind of argument fits Lopreato's Modified Maximization Principle. Daniel Pérusse (1993) distributed questionnaires to 3,000 students at two French-speaking universities in Montréal, Canada, nearly half of which were completed and returned. He found that, although higher-status males in his sample did not have greater reproductive success than lower-status males, the higher-status males did have much more sex, or what Pérusse calls "potential conceptions." More specifically, Pérusse found that measures of occupational prestige and on-the-job power together explained about 60 percent of the variance in the number of potential conceptions. Pérusse concludes that his data support the general biomaterialist argument linking social status and reproductive success, because higher-status men in industrial societies would be having more reproductive success than lower-status men were it not for modern contraception and modern socially imposed monogamy.

Van den Berghe and Whitmeyer (1990) have challenged the notion that the relationship between social status and reproductive success is reversed in industrial societies. They claim that this has not occurred in Japan; that a German census for 1981 showed that the fertility of wives increased with increases in their husbands' income; and that in the United States in both 1960 and 1970 the childlessness of wives

decreased as their husbands' income increased. They also refer to data (Simon, 1974) from post-demographic transition Sweden, West Germany, and New Zealand showing that socioeconomic status and fertility are positively related in those countries. They also note that, when they remarry, higher-status men are more likely than lower-status men to wed younger, more fertile women, and thus to produce more offspring by second or third wives. Moreover, a Forbes 400 study (Essock-Vitale, 1984) showed that the U.S. super rich have an average fertility of 3.1 compared to a rate of 2.7 for the American population as a whole.

Van den Berghe and Whitmeyer suggest that three different reproductive strategies can be found in industrial societies. In order to understand their analysis, we first need to understand the distinction between r and K selection in animal species (MacArthur and Wilson, 1967; Pianka, 1970; E. Wilson, 1975). r-selected species tend to be small animals that leave many offspring but provide little or no parental care. K-selected species, by contrast, are larger animals that leave fewer offspring but that provide much more parental care. Within the same species organisms may vary along the r-K spectrum depending on environmental conditions, being more r in some environments (typically less predictable environments) and more K in other environments (usually more predictable environments). The r-K distinction shows that there is a tradeoff between mating effort and parental effort. The higher the mating effort, the lower the parental effort, and vice versa. Humans are a highly K-selected species, but they have been shown to behave in more r-like ways under certain conditions.

According to van den Berghe and Whitmeyer, in modern industrial societies the stable working class and the middle and upper-middle classes tend to follow an extreme K strategy. Here people limit themselves to two or three children in whom they invest heavily. There is a quality-quantity tradeoff in favor of quality. Parental investment involves high-intensity care and the investment of economic and educational resources in order to equip offspring for success in a highly competitive environment (cf. Kaplan, 1996). A second strategy is employed by the upper classes, whose members can have both quantity and quality. This is a less extreme K strategy. Finally, the lower classes, especially stigmatized racial or ethnic minorities, adopt a more r strategy. Here fertility is higher and parental investment is lower; quantity is preferred over quality as a strategy of reproductive success.

Whatever may turn out to be the relationship between social status and reproductive success in modern industrial societies, the key arguments of a Darwinian biomaterialist perspective are not overturned. Even if high-status men are not leaving more offspring, they appear to be having more sex with more partners and thus having more "potential offspring." Given such behavior, they would have had more offspring in the ancestral environment.

INFANTICIDE

Sarah Blaffer Hrdy (1979) has shown that infanticide is widespread among animal species. Usually it is adult males who kill infants, but females are sometimes involved. Infanticide by adult males occurs among all higher primates (Old and New World monkeys and apes). Hrdy indicates five reasons for infanticide, four of which suggest that infanticide is an evolved adaptation:

- *Exploitation*: The killers may directly benefit from an infant's death; they might eat it, for example, or use it as a "buffer" in fights with others.
- *Resource competition*: Killing an infant may give the killer or his or her descendants access to more resources.
- *Parental manipulation*: A parent might kill an infant because it is defective in some way, because inferior ecological conditions might make its survival difficult, or because there are older offspring whose well-being would be harmed if resources are devoted to the new infant.
- *Sexual selection*: Adult males may kill offspring if they are not the killer's descendants and if the killing of these offspring would enhance the killer's opportunities to breed. For example, if a male kills a female's offspring sired by another male this will bring her into estrus again and allow him to mate and breed with her.

Infanticide is also a relatively common form of human behavior. It has been found in all types of societies, including modern industrial societies, as well as throughout history (Daly and Wilson, 1988; Hrdy, 1999). At first glance, infanticide seems utterly to contradict the notion of kin selection. However, as Daly and Wilson (1988) point out, infanticide can be a rational act from the point of view of inclusive fitness maximization if letting a child die helps promote the life or well-being of an older child or children in whom a great deal of time, energy, and resources have already been invested. In other words, parental care should not be indiscriminate. It would make little sense in terms of promoting inclusive fitness to invest in children whose chances of survival are low, and who may take away resources from children with a better chance of survival.

Daly and Wilson point to three basic reasons why parents might not want to invest strongly in a particular child: doubts about paternity; indications that the child is of dubious quality, for example is diseased or deformed; and poor circumstances for rearing, such as food scarcities, lack of social support, or the demands of an older child. Using a sample of 60 preliterate societies (39 of which reported the occurrence of infanticide), Daly and Wilson found extremely strong support for their claim. Out of 112 cases of infanticide, 97 resulted from one of their three basic reasons for expecting infanticide. They found only four instances out of 112 in which infanticide could be said to be damaging to the parent's inclusive fitness. In modern industrial societies, where infanticide still occurs occasionally, the risk to an infant is greatest in the case of a teenage mother, and declines as mothers move into their 20s and 30s. Mothers suffering from difficult economic circumstances are also much more likely to commit infanticide, and infanticide is more common among single than among married mothers.

Daly and Wilson note that the data on infanticide are perfectly consistent with the reality of maternal bonding (cf. Hrdy, 1999). They delineate three stages of bonding: initially the mother assesses the quality of the child and her own circumstances; then she begins to establish an individualized love for the child; finally, there is a gradual deepening of maternal love over the course of years. Thus child-specific parental love is variable, and highly dependent on the extent to which circumstances are favorable. Parental feeling seems to be such that the likelihood of infanticide declines progressively with the age of a child. This reflects the child's growing value to the parent, and we might say that parents have evolved to love children more the older they are because of older children's greater reproductive value. Daly and Wilson give recent data for Canada showing support for the effects of age. They point out that, even though there is often much conflict between parents and teenage children, these children are at very low risk for parental homicide. They are much more likely to be killed by nonrelatives than by relatives. When parents kill older children it is usually the result of such things as depression or insanity.

Kin selection theory predicts that stepchildren should be at greater risk of abuse and homicide, and this is exactly what the data show. In fact, Daly and Wilson show that stepparenthood remains the single most powerful risk factor for child abuse that has yet been identified. They present data showing that children living with substitute parents were about 100 times as likely to be the victims of fatal abuse as children living with their natural parents (data are for the United States in 1976). A famous childhood story in Western society is that of Cinderella and her evil stepmother and stepsisters, and Daly and Wilson show that similar stories and cultural themes are found throughout the world.

Daly and Wilson also note that most infanticide is female-selective. This has often been interpreted as the result of male domination (which generates a preference for males), or, by Harris (1974, 1977; Divale and Harris, 1976) and other cultural materialists (Divale, 1972), as a population-regulation mechanism, the idea being that the rate of reproduction is determined by the number of females, rather than the number of males, in a group. However, as Daniel Bates and Susan Lees (1979) have pointed out, there is precious little evidence to support the population-regulation argument, and it is even difficult to imagine how people would be able to track their environment in such a sophisticated way so as to achieve the desired result. An alternative interpretation is that people will normally want to invest more in sons because their reproductive potential is much greater than that of daughters. This leads to the notion that sex-selective infanticide should vary by social class. Mildred Dickemann (1979a) has studied sex-selective infanticide in three highly stratified societies: British India in the nineteenth century, China in the nineteenth century, and medieval and early modern Europe. In all of these societies there was extensive

hypergyny (i.e., women's marrying men of higher social status). Because of this, and also because of the practice of polygyny, in the middle and upper classes males had greater reproductive value and so female infanticide was practiced extensively, actually reaching 100 percent in some instances. By contrast, among the lower classes females had greater reproductive value because they were in demand by the men of higher social classes, and thus male infanticide was more common (although it failed to reach the levels of female infanticide among the middle and upper classes). Dickemann's research thus suggests that sex-selective infanticide springs from an evolutionary psychology that is derived from the attempt to maximize inclusive fitness: Parents invest in the sex with greater reproductive potential.

PUBERTAL TIMING AS A FACULTATIVE TRAIT

In a fascinating article, Draper and Harpending (1982) have suggested that in societies in which males generally invest heavily in their offspring, females tend to delay sexual bonding and to be very selective in regard to mate choice. On the other hand, in societies, or segments of societies, in which males provide little parental effort, females may adopt a reproductive strategy that emphasizes an early start to reproduction combined with little or no concern for mates. The authors point out that in the United States, adolescent girls from father-absent homes "show 'precocious' sexual interest in boys, a denigrating attitude toward males and masculinity, and little interest in maintaining sexual ties to one male" (1982:263). These behavior patterns are widespread among lower-class American blacks, but are also found to some extent among lower-class whites.

Draper and Belsky (1990) point to studies showing that girls from divorced homes reach puberty and begin sex earlier than girls from intact homes. These findings, plus those of Draper and Harpending mentioned above, suggest to them that girls from father-absent homes are following a more r-like strategy than other girls. They begin reproduction early and emphasize quantity over quality. Reproductive strategies are facultative traits which allow the organism to assess the environment and respond accordingly, as Belsky, Steinberg, and Draper (1991:653) explain:

> What evolutionary mechanism . . . would underlie an accelerated reproductive schedule in the context of *unfavorable environments* such as poverty or social and familial instability, as we propose? We point again to the maxim that organisms have been selected to reproduce themselves and to attend to important environmental cues in the process. In the absence of indications that delayed maturation and reproduction can have benefits, early sexual activity and high fertility have much to recommend them. This strategy may be associated with higher offspring mortality, but from the point of view of fitness, individuals living in such adverse circumstances who *delay* reproducing may well be selected against (i.e., leave few or no offspring). In such an environment, a man who invests disproportionately in one woman and in children (who may not be his own) will leave relatively few of his own offspring behind. Likewise, a young woman who waits for the right man to help rear her children may lose valuable reproductive opportunities at a time when her health and physical capability are at their peak and when her mother and senior female kin are young enough to be effective surrogates.

Recent studies of the effects of early family environment on reproductive strategies have been carried out by, *inter alia,* Kim, Smith, and Palermiti (1997), Ellis et al. (1999), and Ellis and Garber (2000). Kim, Smith, and Palermiti (1997) studied 380 secondary school students (197 females and 183 males) in two communities in southern Italy. They found that, for women, earlier menarche was correlated with more family stress throughout childhood, more unhappiness in parental marital relations throughout childhood, more conflict with mothers, more rejection from fathers, less emotional closeness to mothers, and more independence from parents in late childhood. For men, earlier spermarche was correlated with more parental marital conflict in early childhood, less emotional closeness to fathers during childhood, and more aggressiveness and unruliness in late childhood. However, most of the correlations obtained by the authors were relatively low, and it is therefore hard to say how theoretically significant (as opposed to statistically significant) the findings are.

Ellis et al. (1999) used a data set from a longitudinal study of 281 girls in the United States to analyze the effects of a variety of environmental variables on pubertal timing. These variables mainly involved family stressors and the quality of family relationships. The authors found that the single most important factor related to pubertal timing was what they called "father-daughter affectionate-positivity." The more affectionate and positive the relationship between fathers and daughters, the later daughters reached puberty, and father-daughter positivity predicted pubertal timing much better than mother-daughter positivity. However, in contrast to Belsky, Steinberg, and Draper (1991) and other studies, Ellis et al. found that negative relationships within the family were not correlated with pubertal timing.

Ellis and Garber (2000) studied the effects of the presence of stepfathers or mothers' boyfriends on girls' pubertal timing in a sample of 87 American girls. They found a correlation of $r = -.37$ between a girl's age and the entry of an unrelated father figure into her household. Thus, the younger a girl was when this father figure appeared, the earlier her pubertal timing. More importantly, Ellis and Garber found a strong relationship ($r = .67$) between the level of dyadic stress (i.e., stress between a mother and her romantic partner) in stepfather/boyfriend families and a girl's maturational timing, which indicates that girls reached puberty earlier when this stress was greater. In the biologically intact families in the sample, there was little relationship between dyadic stress and pubertal timing ($r = .15$). In contrast to the Ellis et al. (1999) study, this study does provide support for the importance of family stressors.

These studies and similar ones clearly suggest that the timing of sexual maturation (and thus potential reproduction) is influenced by key environmental circumstances, especially those involving the family environment. Reproductive strategies, perhaps more than another other human trait, are highly facultative in nature (cf. Chisholm, 1999:149-202).

FERTILITY DECLINE IN THE MODERN WORLD

A well-known feature of life in less-developed countries is the presence of a large number of children per family. Compared to couples in the developed world, couples in less-developed countries produce a good many more offspring. Yet in recent decades there has been a substantial and, to some at least, a surprising reduction in fertility rates. Between 1960 and 1990, total fertility throughout the world fell from 5.56 to 4.29 children per ever married woman, a 23 percent reduction. Efforts to explain this reduction, as well as efforts to explain fertility reduction in the countries of the original demographic transition of the nineteenth and twentieth centuries, have varied widely.

One of the most widely endorsed theories holds that people adjust their fertility levels to the economic value of children's labor. A major proponent of this ecomaterialist view is Marvin Harris (1989; Harris and Ross, 1987), who draws on research by Mamdani (1972), Benjamin White (1973, 1982), Nag, White, and Peet (1978), and Nag (1983). Harris notes that in societies or regions where the economy is still based on agriculture very young children typically perform such tasks as gathering firewood, carrying water for cooking and washing, grinding and pounding grains, taking food to adults in the fields, sweeping floors, and running errands. Older children are involved in cooking meals, working full time in the fields, hunting, herding, fishing, and making pots, containers, mats, and nets. Under these conditions, it is rational for people to keep their fertility high to maximize the economic benefits their children provide for them. The shift from high fertility to low fertility results, Harris claims, mainly from the changing economic value of children's labor with industrialization. As children cease to be economic assets - become, in fact, very costly to rear - it is much more rational for couples to limit their numbers. The same type of argument has also been pressed by a number of demographers. John Caldwell (1976), for example, holds that the key issue is the direction of wealth flows between parents and children. High-fertility societies - primitive and traditional societies - are those in which wealth flows are primarily from child to parent, whereas low-fertility societies - industrial societies - are those in which wealth flows go primarily from parent to child. Ester Boserup (1986) regards occupational change as the crucial factor leading to fertility decline in countries with high-fertility regimes. Boserup presents fertility rates for people in four different occupations in 28 less-developed countries. Those working in agriculture had the highest rate, 8.38. The lowest rate

was obtained by workers in professional, managerial, technical, and clerical jobs, 6.74. People working in sales and service and in manual work had fertility rates that were in between, 7.39 and 7.63 respectively. Note, however, that the fertility differences between occupational groups presented by Boserup are relatively small, and that the fertility of the lowest group is still nearly seven children, much higher than is found in the industrialized countries. This suggests that something is amiss with the ecomaterialist argument.

Several demographers have challenged the view that in preindustrial societies the net flow of benefits is from child to parent (Turke, 1989; Low, 1991, 1993a; Low and Clarke, 1992; Kaplan, 1994). Low has claimed that "children's labor is never sufficient to result in a net economic gain to parents" (1993a:184), and Turke (1989:76) goes so far as to argue that in all societies "the net flow of services and resources will usually be from older to younger generations." Both Turke and Low and Clarke conclude that traditional demographic arguments have had it backwards: People do not use reproductive resources to acquire economic benefits, but rather use economic resources to acquire reproductive benefits. It is the promotion of inclusive fitness rather than the net economic value of children that governs fertility behavior. Hillard Kaplan (1994) has tested the wealth flows argument by using data from three tribal societies in South America, all of which practiced a mixture of hunting and gathering and horticulture. In all three societies fertility was very high – an average of 8.15 for the societies combined – but children in each society were producing far fewer calories than they were consuming. Kaplan's conclusion is that the flow of benefits was overwhelmingly from parent to child rather than the reverse, even taking into consideration other kinds of work that children performed. "The data from all three groups," he says, "show that even though children were very costly to raise, fertility was high" (1994:763).

A second theory of fertility decline is the biomaterialist argument of Carey and Lopreato (1995). Carey and Lopreato argue that the main determinant of the level of fertility is the level of mortality. According to them, humans have evolved a "two-surviving-children psychology" in which they gear their total fertility to the frequency with which offspring survive to adulthood. Where infant and child mortality are high, fertility will be high in order to replace offspring expected to die before they themselves become reproductive. Likewise, where infant and child mortality are low, and thus where most infants survive to reproductive age, fertility is adjusted downward. If two children born are both likely to survive, why have more? Carey and Lopreato note that Darwin himself "argued that, despite the tendency of populations to outpace the growth of their resources, a countertendency toward population stability is a characteristic of all species. The theory of natural selection suggests that, given the real or potential Malthusian scarcity and the associated struggle for existence, the fertility of individuals displays a vigorous tendency to track mortality – a tendency toward a replacement-level reproductive strategy" (1995:616). Carey and Lopreato go on to remark that the original demographic transition itself provides highly suggestive evidence for their interpretation. As mortality levels dropped fertility dropped even though people were living longer and had more years in which to reproduce. The authors argue that the maximization of inclusive fitness is not a matter of the sheer production of offspring, but rather the production of offspring who are likely to survive and reproduce in the next generation. As they say, "parents who invest their limited resources in fewer rather than more children may also have the greater genetic success" (1995:625). I am skeptical of Carey and Lopreato's argument that people have evolved to aim for only two surviving children but accept their more general point that fertility should track mortality and that the maximization of inclusive fitness often means having fewer (but higher quality) children. In fact, demographers have long thought that fertility levels should logically be related to levels of infant mortality; numerous studies have been carried out to test this argument, but highly disparate results have been obtained (Matthiessen and McCann, 1978; van de Walle, 1986; Cantrelle, Ferry, and Mondot, 1978). Moderate to high correlations have been found for some countries at some times, but for other countries and at other times low correlations have been reported, and in several instances negative correlations have even been found. On the basis of these studies it is difficult to know what to conclude, but I shall readdress this point shortly.

A third major interpretation of fertility decline, a type of polimaterialist argument, emphasizes female empowerment (Dyson and Moore, 1983; Malhotra, Vanneman, and Kishor, 1995; Handwerker, 1993; Murthi, Guio, and Dreze, 1995; Jejeebhoy, 1995; Penn, 1999). In an early study along these lines,

Dyson and Moore (1983) showed that fertility was notably higher in the northern states of India than in the southern states and related this difference to kinship patterns affecting the autonomy of women; greater female autonomy was associated with lower fertility. Malhotra, Vanneman, and Kishor (1995), also studying India, found that where gender discrimination and gender bias in the marriage system were at lower levels, fertility was also lower; the effect of these gender variables remained substantial even when controls for economic development, social stratification, and region were introduced. Murthi, Guio, and Dreze (1995), in yet another study of Indian fertility differentials, focused in particular on the role of female education. They found that female literacy and female labor force participation both exerted negative and statistically significant effects on total fertility. Because the authors presented some of their raw data, I was able to reanalyze them and found that the effect of female literacy was much more important than the effect of female labor force participation. The former correlated at r = -.864 with total fertility, whereas the latter correlated much more weakly, at r = -.166. Handwerker (1993), in a study of Antigua in the West Indies, found that female empowerment in the form of increased education and expanded employment opportunities was significantly correlated with lower fertility. Jejeebhoy (1995) examined 59 studies that explored the relationship between female education and fertility in a large number of countries. Most of these studies showed that greater female education is closely linked with lower fertility. In societies at very low levels of development small amounts of education do not seem to produce a decline in fertility (or in some cases actually are associated with an increase in fertility), but when greater amounts of education are obtained fertility then starts a continual decline. At higher levels of economic development, even small increments in educational attainment lead to fertility decline. Jejeebhoy stresses, as have others, that education exerts its effect on fertility by way of giving women greater autonomy and empowerment with respect to such things as knowledge, decision-making, physical mobility, feelings and loyalties, and economic behavior.

Sanderson and Dubrow (2000) have performed a series of quantitative analyses designed to provide further empirical testing of each of these theories and to compare their relative explanatory power. They used a sample of 121 nations and data collected largely by the World Bank for the years 1960 and 1990. Gross national product per capita, percentage of the labor force in agriculture, percentage urban, female empowerment, and infant and child mortality were in nearly all cases moderately to strongly correlated with fertility in both 1960 and 1990. The number of physicians in the population was highly correlated with fertility, but it washed out when the infant mortality rate was controlled, so it was dropped from further analyses. Several sets of multiple regression analyses were then carried out. In the first, the five independent variables were regressed on 1960 fertility levels. Collectively, these variables explained approximately 60 percent of the variance in 1960 fertility (adjusted R^2), but infant mortality, female empowerment, and percentage urban were clearly the best predictors of fertility, and in that order. As infant mortality fell, fertility fell, and as females became more empowered and more of the population lived in urban areas fertility fell as well. Percentage of the labor force in agriculture largely washed out when the other independent variables were controlled, and thus was an extremely weak predictor. Gross national product was inversely correlated with fertility, as expected, but it underwent a sharp sign reversal when the other variables were factored out. It therefore cannot be regarded as an important predictive variable one way or the other. This analysis was repeated but substituting child mortality (ages 1-4) for infant mortality. Matthiessen and McCann (1978) have suggested that if parents are reducing their fertility in response to lower mortality, then what is really important is the rate of child rather than infant survivorship (cf. van de Walle, 1986:210). However, Sanderson and Dubrow's results showed that child mortality was a much poorer predictor of fertility than was infant mortality. In this analysis female empowerment was the best predictor, and child mortality ranked even below percent urban, which was a relatively good predictor.

The third analysis repeated the first analysis but used 1990 levels of all of the variables. Here again infant mortality was the best predictor with female empowerment second. Gross national product, literacy, and percentage urban were very weak predictors, and percentage of the labor force in agriculture was only marginally better. The five independent variables together explained approximately 80 percent of the variance in fertility, with infant mortality and female empowerment explaining the vast majority of this. The next analysis repeated the third analysis but substituted child mortality for infant mortality. Actually, this

analysis used the mortality rate for children aged 0-5 (because that was the only measure available in the data set), and thus was a combination of infant and child mortality. These results therefore are not strictly comparable to those in the second analysis. In any event, 0-5 mortality was easily the best predictor of 1990 fertility, with female empowerment again the next best. Percentage urban essentially washed out, and the predictive ability of gross national product and percentage of the labor force in agriculture was quite modest. The total variance explained was 80 percent, the vast majority of which was being explained by 0-5 mortality and female empowerment.

It could be argued that, if declining infant mortality leads to lower fertility it can only do so if people are given time to assess its effects over the medium to long run (this is similar to the argument that child mortality should be a stronger determinant of fertility than infant mortality). To test this possibility, Sanderson and Dubrow repeated the third analysis but substituted the 1982 infant mortality rate for the 1990 rate. Here it was found that the predictive ability of infant mortality dropped substantially and that female empowerment became the best predictor. The predictive ability of 1982 infant mortality was not only less than that of female empowerment, but it was only a slightly better predictor than the percentage of the labor force in agriculture. It thus appears that, whatever effect infant mortality has on fertility, this effect is felt quickly rather than after a lag in time.

The final analysis examined changes in fertility between 1960 and 1990 and how they related to changes during the same period of levels of the five independent variables. Together changes in the independent variables explained 55 percent of the variance in fertility change. Infant mortality change was by far the best predictor of fertility change (st. beta coeff. = .551), and change in female empowerment was the only other variable with much predictive ability (st. beta coeff. = -.278).

A second set of analyses was carried out to see if these results could be replicated for the original demographic transition between 1880 and 1940. The sample was a group of 27 countries in North America and Europe (Japan was also included) that are now highly developed or at least have achieved substantial levels of industrialization. Of the two most important predictive variables in the first study, infant mortality and female empowerment, it was only possible to examine the effects of infant mortality. As far as I know, no data exist that would measure female empowerment for these rather distant time periods. However, it is unlikely that there was much female empowerment occurring at these times (especially in 1880 and 1910). Married women were a minuscule proportion of the labor force in all of the now-industrialized societies, and they were much less involved in the educational world than is the case today. Therefore it is probably safe to assume that female empowerment could not have been an important determinant of fertility decline during the original demographic transition, at least on the level that female empowerment has reached today. Sanderson and Dubrow did look, however, at the effects of two other independent variables, percent of the labor force in agriculture and percent of the labor force in manufacturing. These were designed to measure the economic value of children's labor. Thus, the analyses for the original demographic transition test the ecomaterialist theory against the biomaterialist theory.

The results for 1880 show that infant mortality was the best predictor of fertility, with percent of the labor force in agriculture a close second and percent of the labor force in manufacturing a distant third. However, the results were weak and all three variables together explained very little of the variance in fertility. The reason for such weak results is probably the data themselves. Older data are often more unreliable than more recent data, and in this case several values had to be estimated for some countries. This interpretation is reinforced by the fact that the data for 1910 and 1940 showed much stronger results. Infant mortality was far and away the most important predictor of fertility rates in 1910. For 1910, infant mortality and percentage of the labor force in manufacturing together explained 72 percent of the variance, and infant mortality explained most of this. The results were not as strong for 1940, but they were still good. All three variables explained 22 percent of the variance, and infant mortality was clearly the leading predictive variable.

One of the most striking findings of the Sanderson and Dubrow study was that both infant and child mortality were much more strongly related (zero order) to fertility than has been found in most previous studies. The average correlation between infant mortality and fertility for 1960 and 1990 was a

whopping .808, and the correlation between 1960-1990 infant mortality change and 1960-1990 fertility change was .724. For the 1880-1940 data the average correlation was .588. The average correlation between child mortality and fertility for 1960 and 1990 was .732. No previous study has achieved average correlations anywhere near these numbers. It is not clear why Sanderson and Dubrow's results were so much stronger; one possibility is the higher level of aggregation of their data compared to previous studies.

Sanderson and Dubrow's results are broadly consistent in one way with those from an earlier study by Lopreato and Yu (1988), who carried out a multiple regression analysis on data from 63 contemporary nation-states. Their principal finding was that the number of physicians in the population was the best predictor of fertility, and they regarded their physicians variable as a proxy for the rate of infant and/or child survivorship. They included infant mortality in their regression analysis, but it was not a strong predictor. This is surprising in view of the fact that the infant mortality rate is a direct rather than an indirect indicator of infant survivorship. Sanderson and Dubrow found that both the number of physicians and infant mortality were highly correlated (zero order) with fertility, but, as they expected, the number of physicians washed out when infant mortality was controlled. In this respect their results differ from Lopreato and Yu's. Nevertheless, Lopreato and Yu interpret their findings in the same way as Sanderson and Dubrow: The survival rate of infants, which is determined in large part by the level of health care in a society, is a major determinant of the level of fertility. Also in a similar way to Sanderson and Dubrow's study, Lopreato and Yu found that two female empowerment variables, percentage of women in the labor force and percentage of women who are literate, were good predictors of fertility.

Sanderson and Dubrow's results for the 1960-1990 period thus support both the biomaterialist argument that emphasizes infant mortality decline and the polimaterialist argument that emphasizes female empowerment, but the biomaterialist argument receives stronger support because infant mortality was the best predictor in four of the six analyses, and also because it was clearly best in predicting actual fertility *change*. By contrast, their results show weak support for the ecomaterialist argument that fertility is being adjusted primarily to the economic value of children's labor. Three of the standardized beta coefficients for percentage of the labor force in agriculture were not much above zero, and the other coefficients were weak. In terms of percentage of the population living in urban areas, this was a good predictor for 1960 but not for 1990 or for changes during the 1960-1990 period. In fact, the beta coefficient with respect to 1960-1990 change carried the wrong sign. Finally, it cannot be a simple increase in material wealth or the standard of living that is determining fertility change. Three of the beta coefficients for gross national product were barely above zero and the other three, though substantially larger, were in the wrong direction. The analyses of the original demographic transition, the 1880-1940 period, show even more support for the biomaterialist argument. Here infant mortality was easily the best predictor of fertility in all three analyses, and in 1910 it explained well over half of the variance. However, there was slightly more support here for the role of the changing economic value of children's labor, and so this may have been somewhat more important for the original demographic transition than for the 1960-1990 period.

Although Sanderson and Dubrow found that fertility does not decline simply because societies develop economically and become wealthier, the transformation of society from a rural and agricultural base to an urban and industrial base is no doubt an important part of the story (even if not in a way that was measured by any of their independent variables). Hillard Kaplan (1996) suggests that modernization has produced a situation in which people reduce the number of offspring they have but invest much more in each: They trade quantity (an r strategy) for quality (a K strategy) (cf. van den Berghe and Whitmeyer, 1990). He proposes that it is the development of skills-based competitive labor markets that is the crucial factor. Such markets have the effect of increasing the importance of parental investment in offspring well-being. In having fewer children but investing more in each, parents are not necessarily maximizing their fitness in the classic sense, even in the long run. However, at the very least they are maximizing the "cultural fitness" of their offspring (and grandoffspring, etc.). Kaplan's argument is highly consistent with the common observation that in modern societies better-educated and wealthier parents tend to have fewer offspring than couples with less education and lower income. Alan Rogers (1995) has argued that the concept of fitness should be expanded to include not only the total number of offspring produced, but also the economic well-being of each offspring. As he puts it, "In a world with heritable wealth, wealth has value

over and above its effect on the number of one's offspring. By continuing to earn, a rich person can increase the wealth of descendants several generations removed. Thus, the marginal effect of wealth on fitness may remain positive even among the very wealthy" (1995:94) (cf. Borgerhoff Mulder, 1998).

What is missing in these arguments is any reference to reduced infant mortality. In order for parents to have only two or three children and invest heavily in each child the rate of infant and child survivorship must be very high, as it is in modern industrial societies. As Wiley and Carlin (1999) have persuasively argued, mother-child attachment will be at its highest in those societies that have both low fertility and low infant mortality, i.e., in post-demographic transition societies. "Exclusive attachment," they say, "may . . . be an outcome of low mortality, as parents are relatively assured that their investment will pay off" (1999:153). Strong mother-child attachment (and perhaps even father-child attachment) would seem to be a prerequisite for intensive parental investment.

The importance of female empowerment in the process of fertility decline would seem to fit well into this picture. It has been argued that female empowerment helps reduce fertility because women often regard having a large number of children as a serious burden since it is they who take on most of the duties of child care in all societies; as women are empowered they will be in a better position to act on their desire to have fewer children (Murthi, Guio, and Dreze, 1995; Jejeebhoy, 1995; Penn, 1999).

Unfortunately, the various arguments sketched above only work if we can show that the causal arrows are actually pointing from lower infant mortality to lower fertility, as well as from enhanced female empowerment to lower fertility. With respect to the infant mortality-fertility relationship, a number of demographers have suggested that the causal arrows could be running in the opposite direction (Chowdhury, Khan, and Chen, 1978; Knodel and van de Walle, 1986). In this argument, high fertility leads to high mortality because more children stress family resources and place greater burdens on mothers, both of which produce a poorer level of infant and child care. When fertility declines (for whatever reason) it generates circumstances favorable for a decline in infant mortality. As women have fewer children they are able to provide a higher standard of care to each and thus increase the rate of infant survivorship. A number of demographers have tried to specify the temporal order between mortality change and fertility change. However, the results are highly inconclusive (e.g., Matthiessen and McCann,1978; van de Walle, 1986; Chowdhury, Khan, and Chen, 1978; Ben-Porath, 1978; Knodel, 1986). For some countries at some times it has been found that infant mortality declined first, but in other countries at other times it was fertility that declined first.

In new research as yet unpublished, I have tried to disentange the causal relationships between infant mortality decline, female empowerment, and reduced fertility. To test the causal relationship between infant mortality decline and fertility decline I performed a series of panel regression analyses. First, I regressed 1960 levels of all of the independent variables on 1990 levels of fertility, while simultaneously controlling for 1960 levels of fertility. The results showed strong support for the argument that the direction of causation is from infant mortality to fertility. All of the beta coefficients except those for infant mortality and 1960 fertility were small and statistically nonsignificant. The beta for infant mortality was the largest (.586), and it was substantially larger than that for 1960 fertility (.305). Then I made 1990 levels of infant mortality the dependent variable and regressed 1960 levels of the independent variables on it, while at the same time controlling for 1960 levels of infant mortality. Infant mortality in 1960 was by far the best predictor of infant mortality in 1990 (beta = .832), whereas 1960 levels of fertility did not predict infant mortality in 1990 (more accurately, did not predict changes in infant mortality between 1960 and 1990) (beta =
-.103). These two panel analyses, then, show that infant mortality decline is causal to fertility decline rather than the other way around.

I also carried out several panel analyses for the 1880-1940 period. Unfortunately, these produced mixed and inconclusive results. Infant mortality decline did appear to be an important cause of fertility decline between 1880 and 1910 but not for the 1910-1940 period. However, I was able to eliminate the reverse hypothesis for both periods: that fertility decline was the cause of infant mortality decline.

What about the female empowerment-fertility relationship? Two panel analyses were carried out to test for the direction of causation. In the first, 1960 levels of the independent variables (infant mortality

excluded) were regressed on 1990 fertility levels while controlling for 1960 fertility levels. The results showed that female empowerment had no causal influence on fertility decline between 1960 and 1990. The beta for female empowerment in 1960 was -.053, whereas that for fertility in 1960 was .411. In the second analysis, 1960 levels of the independent variables (again, infant mortality excluded) were regressed on 1990 levels of female empowerment while controlling for 1960 levels of female empowerment. This analysis was designed to determine whether the causal relationship was running in the opposite direction, i.e., whether it was fertility decline that was causing an enhancement of female empowerment. The results showed that this was indeed the case. The beta for fertility in 1960 was -.387, whereas that for female empowerment in 1960 – which should have been the best predictive variable – was only .185. The results of these last two panel analyses thus suggest that fertility does not decline because women become more empowered, but rather that when fertility does decline this opens up opportunities for female empowerment by making it easier for women to pursue more education and to enter the labor force.

This means that we are left with the biomaterialist argument as the only viable explanation of declining fertility. Female empowerment is a result rather than a cause of fertility decline, and, as noted earlier, the changing economic value of children's labor seems to play no significant role in the process of fertility decline. Such is the value of comparative theory testing using rigorous quantitative data.

ADDITIONAL EVIDENCE

Cronk (1989, 1991, 1999, 2000) discusses a number of cases of female-biased parental investment and shows how these can be explained through a combination of biomaterialist and ecomaterialist theories. Sieff (1990) has contributed an important article along similar lines. Hrdy (1999) provides excellent discussions of a variety of reproductive phenomena, especially differential parental investment in sons or daughters and biased sex ratios. Low (1993a) and Low, Clarke, and Lockridge (1992) illustrate a variety of applications of a biomaterialist perspective to demographic phenomena. Harris and Ross (1987) offer an extensive ecomaterialist analysis of demographic phenomena in the main types of human societies. Sometimes their analyses are complementary with biomaterialist analyses, but, as was seen above, often they conflict with them and must give way to them. Blurton Jones (1986) has carried out a classic study of birth spacing among !Kung women. The women he studied had birth-spacing intervals of approximately four years, and he has shown that this was optimal in terms of producing the most surviving offspring.