Chapter 1
From Nature Up

Homage to Aesop

Admirers of particular animals sometimes see in their favorites the human condition writ small. Eighteenth-century naturalists studied the animal creation for testimony to the wisdom and beneficence of its author. Their twentieth-century successors are more likely to draw conclusions about a less exalted subject. For contemporary students of animal behavior, the ladder of evidence does not reach all the way from nature up to nature's god. Yet the observations of animal activity are seen as offering general morals: the Uganda kob and the well-trained pigeon reveal human aspirations in different dress. Cautionary fables are there for those who have the wit to read them.

Sociobiology is popularly regarded as a program launched in 1975 with the publication of Wilson's book. So conceived, it stands in a long tradition of attempts to discern the elements of human nature in the behavior of nonhuman animals and thus to justify the ways of man to man. Advocates of the program pride themselves on having transcended the naive attempts to derive grand conclusions about human nature from detailed observation of a single species or from scattered investigations into selected types of behavior in a motley of species. Not for them the casual assumption that explanations that work today for pigeons will work tomorrow for people. Not for them the collection of scraps of natural history to provide suggestive analogy. Sociobiology has put system and science in the place of parochial vision and animal anecdote. Contemporary evolutionary theory has supplied a new philosophers' stone, one that enables Wilson and his followers to turn ethological dross into sociobiological gold.

Wilson explicitly links his "new synthesis" to the faltering but meritorious efforts of his predecessors. After commending popular books by Konrad Lorenz, Robert Ardrey, Desmond Morris, Lionel Tiger, and Robin Fox for their "great style and vigor," he continues,

> Their efforts were salutary in calling attention to man's status as a biological species adapted to particular environments. . . . But

> their particular handling of the problem tended to be inefficient and misleading. They selected one plausible hypothesis or another based on a review of a small sample of animal species, then advocated the explanation to the limit. (Wilson 1975a, 551; see also 28–29, 287)

Those who are fortunate enough to draw on refinements of evolutionary theory that have made possible a science of human social behavior can afford to sympathize with their pioneering predecessors who were not so lucky.

The central message of one program in human sociobiology, a program that derives from some of Wilson's writings (notably 1975a, 1978), is that the integration of evolutionary insights with careful observations of animal behavior yields a particular theory of human nature. Much of the controversy about sociobiology has concerned the credentials of this program. The harsh response of the Sociobiology Study Group of Science for the People was intended to devastate the program. Yet the message rings on in the pages of such committed early Wilsonians as David Barash (1977, 1979) and Pierre van den Berghe (1979). Whoever is not for the program is against Darwin.

Despite its appropriation of the term "sociobiology," Wilson's new synthesis does not appeal to all those who want to call themselves "sociobiologists." There are sociobiologists and sociobiologists and sociobiologists. Some are card-carrying followers of the program begun in *Sociobiology* and *On Human Nature.* Others, most prominently Wilson himself, have gone on to new heights. Still others, writers like Richard Alexander and Napoleon Chagnon, maintain that there are important implications of evolutionary theory for the study of human nature and human society, while diverging in important ways from the analysis favored by Wilson and his followers. In addition to these, there remains a large group of scientists, probably a majority of those interested in the behavior of nonhuman animals, for whom the controversies that swirl about sociobiology provoke profound discomfort. For people in this group, the evolution of animal behavior is interesting in its own right. They have no wish to play Aesop, and they distrust the idea that the theory of evolution offers any direct insight into human nature. They worry that lurid advertisements for programs like Wilson's will cause sound biology to be viewed as politically dubious. Some would even prefer to use another label to describe their work (Hinde 1982, 151–153).

It will be useful to have a term for the enterprises that promise important insights into human nature. *Pop sociobiology*, as I shall call it, consists in appealing to recent ideas about the evolution of animal behavior in order to advance grand claims about human nature and human social institutions. I use the term as an abbreviation for "popular sociobiology"; the name seems appropriate because the work that falls under this rubric not only is what is commonly thought of as sociobiology but is deliberately designed to command popular attention. Pop sociobiology is practiced by such people as Wilson, Alexander, Robert Trivers, Richard Dawkins, Barash, van den Berghe, and Chagnon. Some of these people (the first four, for example) also engage in biological investigations into the evolution of nonhuman behavior. One of the tasks of the chapters that follow will be to differentiate their additions to pop sociobiology from their contributions to the biology of nonhuman behavior.

Pop sociobiology should be distinguished from both the subdiscipline of evolutionary theory that studies the behavior of nonhuman animals and a possible future discipline that might employ ideas from evolutionary theory in investigating human social behavior. Pop sociobiology is a particular historical movement—more exactly, a cluster of related historical movements—a collection of ideas, arguments, and conclusions that have emerged in recent years. I shall postpone to the very end of this book the question whether it is possible to develop a genuine science of human behavior that draws on the insights of evolutionary biology. For the moment I am only concerned to note that the pop sociobiologists should not be granted a monopoly. There is no a priori reason to believe that any serious biological study of human behavior must go the way of pop sociobiology. For the present, however, pop sociobiology dominates this area of inquiry.

There are three major rival programs within pop sociobiology. Although there are important affinities among them, each deserves separate treatment. First, and most widely known among general readers, is the *early Wilson* program, whose central texts are *Sociobiology* and *On Human Nature*. Second is the program inaugurated by Wilson and Lumsden in *Genes, Mind, and Culture*. Finally, perhaps the most influential sociobiological program among practicing social scientists is the enterprise recommended by Richard Alexander. By considering the proposals of each of these three approaches, I hope to offer a clear diagnosis of the state of pop sociobiology.

Like any group of scientists, sociobiologists have their differences. But the variation among students of the social behavior of animals is not simply a routine range of disagreements about specific points. The primary division is between the advocates of pop sociobiology and those who explicitly distrust any grand theorizing about human nature (see Maynard Smith 1982b, 3). The caution of the latter often strikes more ambitious sociobiologists and their supporters as a failure of nerve. Thus Michael Ruse chides Maynard Smith for failing to see that "analogies work two ways"—if Maynard Smith is content to apply methods originally introduced for studying human decision making to the behavior of nonhumans, then, Ruse contends, he ought to be prepared to apply his conclusions about nonhuman behavior to humans as well (Ruse 1979, 147). The anthropologist Chagnon is also puzzled by Maynard Smith's reluctance to "apply" sociobiology to humans (1982, 292). For him, for Ruse, for Wilson, and for a host of other writers, sociobiology is conceived as a general doctrine, which flows ineluctably from evolutionary theory and which yields profound consequences for our understanding of human behavior. There is no separating any such thing as "pop sociobiology." There is only sociobiology, practised with more or less consistency and intellectual fortitude.

I believe that this conception is radically incorrect and that it has proved seriously misleading. Insofar as there is a subject, "sociobiology," which flows from evolutionary theory, it is not a general doctrine. Insofar as there is a general doctrine that challenges us with important claims about people and their institutions, it does not flow from evolutionary theory. If we are to avoid losing blooming babies with unwholesome bath water, it will be necessary to make some important distinctions and to offer a better picture of the varieties of sociobiology. Because of its importance in the political controversies and because it offers a way into many of the central issues, I shall start by trying to characterize the early Wilson program.

Wilson's Ladder

No one who picks up a copy of *On Human Nature* can avoid the advertisement. The cover of the hardbound edition announces that the book "begins a new phase in the most important intellectual controversy of this generation: Is human behavior controlled by the species' biological heritage? Does this heritage limit human destiny?" Once inside, the vision blurs. It seems that conclusions about human limitations are supposed to follow from premises about the evolution of behavior, but the structure of the argument is elusive. Sometimes a brief sentence makes the intent plain: "Polygyny and sexual differences in temperament can be predicted by a straightforward deduction from the general theory of evolution" (1978, 138). Yet how does the "straightforward deduction" go?

Unsympathetic critics can easily devise versions of their own, demolish the arguments they have constructed, and move on to wage new intellectual battles elsewhere. Wilson evidently envisages a ladder that will enable him to ascend from studies of nature up to controversial claims about human nature. Let us briefly consider how he justifies his remarks about "polygyny and sexual differences in temperament." Wilson devotes several pages to discussing the differences between male and female behavior that we might expect to find, given that human behavior is a product of evolution. He suggests that evolution under natural selection would favor the differences that are found in most human societies. Putting evolutionary expectations together with claims about the widespread occurrence of the differences, Wilson arrives at the conclusion that there are genetic constraints on gender roles.

When we try to reconstruct the argument, we have to fill in some lacunae. There is a version of Wilson's ladder that will apparently accommodate our example, and it has figured prominently in discussions of Wilson's ideas. It runs as follows.

Wilson's Ladder (Naive Version)

1. Evolutionary theory yields results to the effect that certain forms of behavior maximize fitness.
2. Because these forms of behavior are found in many groups of animals, we are entitled to conclude that they have been fashioned under natural selection.
3. Because natural selection acts on genes, we may conclude that there are genes for the forms of behavior in question.
4. Because there are genes for these forms of behavior, the forms of behavior cannot be altered by manipulating the environment.

When the argument is stated so baldly, almost anybody will disavow it. There are passages in Wilson's writings—even in his most recent books—in which he seems to come close to embracing the ideas of the naive version. In many of his remarks, however, especially on occasions on which he is taking care to guard himself against criticism, Wilson will have no truck with anything so crude. His articles are full of weary attempts to dissociate himself from some of the doctrines that the naive version ascribes to him. I shall take these remarks seriously, in the hope of seeing if a more refined version is available.

To discover how Wilson's ladder is really constructed, I suggest that we start with the naive version and consider how to modify it in the light of Wilson's disclaimers. This strategy is forced on us because of Wilson's preference for "pungency and simplicity of style" (1978 jacket blurb) over logical explicitness. Protestations about the ways in which he has been misunderstood provide the best clues we have to the character of the intended argument.

What is wrong with the naive version of the ladder? Plenty. Its conception of the deliverances of evolutionary theory is suspect, its assumption that optimal behavior signals a history of selection deserves scrutiny, and its talk of genes "for" forms of behavior will make any practicing geneticist wince. But the most glaring error comes at the end. Enshrined among the commonplaces of contemporary biology is a principle that every beginning student learns—the characteristics of an organism are the result of the interplay between the genes of the organism and the environment in which it develops. We do not live by our genes alone. Wilson's critics have forced him again and again to announce his devotion to this commonplace. His detractors read the announcements as a smokescreen. There must be a secret denial behind the public acceptance, they allege, because if Wilson really meant what he says he means, then his version of pop sociobiology would become trivial. Without a commitment to "genetic determinism" pop sociobiology may be full of sound and fury, but it signifies nothing.

Our attempt to find a better version of Wilson's ladder can profitably begin with this vexed issue. Unless we can dissolve some of the myths that surround the idea of "genetic determinism," then it is likely that we shall bequeath "the most important intellectual controversy of our generation" to generations without end.

The Iron Hand Meets the Empty Mind

The organization of organisms is deceptively simple. Animal bodies are composed of cells. The nuclei of the cells provide a home for the chromosomes. Genes are segments of chromosomes. If we select a particular animal we can, in principle, identify the collection of genes that distinguishes it from any other animal (with the possible exception of siblings formed from the same fertilized egg). This complement of genes, the animal's *genotype*, will be found in almost all the cells that together make up the animal. The exceptions (barring mutations) will be the sex cells—the sperm or ova—produced by the animal. Typically, these contain only half of the animal's chromosomal material, and thus about half of the animal's genes.

Reproduction transmits the parental genes to a new organism. Animals that reproduce sexually pass on half their genes to their offspring. Asexual organisms do better at making faithful copies of themselves. Barring mutation, their offspring are perfect replicas of the parent. (Whether this should count as success for the single parent is a delicate question in contemporary evolutionary theory. See Williams 1975; Maynard Smith 1978; Stanley 1980.)

Memories of Mendel dominate popular ideas of the process of reproduction and foster the belief in genetic determination of readily observable traits. Mendel believed that there was a one-to-one correlation between genes and observable characteristics. Using notions that were not available to him, we can explicate his ideas as follows. The chromosomes of a sexually reproducing animal typically pair up just before the division in which the gametes are formed. Each chromosome has a mate (the chromosome *homologous* with it) with which it pairs, and, if the process of division goes smoothly, one member from each pair goes to a gamete. (For the moment I shall ignore complications.) Let a *locus* be any region of chromosomal material at which exactly one gene occurs. The different genes that can occur at a particular locus are called *alleles.* If we look at corresponding loci on homologous chromosomes, we find a pair of alleles. One way to formulate Mendel's idea is to say that the combination of alleles present at a pair of corresponding loci determines the form of the characteristic that is governed by that pair of loci. So, to revert to a classic example, Mendel envisaged a locus for seed color in garden peas. Depending on the alleles present at that locus and the corresponding locus, the color of the seeds set by the plant would be yellow or green.

Contemporary geneticists know more than Mendel did, and they recognize that simple kinds of connections between genes and observable traits are extremely rare. The totality of the characteristics of an organism, the organism's *phenotype,* is the product of a complicated interaction between the genotype of the organism and the environment(s) in which it develops. Occasionally we can isolate traits that are dependent on only one locus and for which environmental effects are negligible. (Terminological note: Here and hereafter I use "locus" to refer to a pair of corresponding regions on homologous chromosomes; this agrees with the usual practice of geneticists.) Typically, however, many genes combine to affect the characteristics we observe, and their action can be perturbed by changes in the environment. Eye color in the fruit fly is controlled by an array of genes scattered across all the chromosomes. Raise your fly at an unusual temperature and you will find that the eye color manifested is like that found in flies with unusual genotypes. Grow plants with identical genotypes in different soils, and you will discover a wide range of variation in height, vigor, and quality of flowers and fruits.

Mendel's successors have made clear to us why we should find so complicated a story. Genes are segments of chromosomes, and chromosomes are composed of DNA. *Structural* genes are chunks of DNA that direct the formation of proteins. *Regulatory* genes are stretches of DNA that control the times and rates at which the structural genes operate. As an embryo develops, there is a sequence of cell divisions—itself ultimately directed by the reactions that occur in the individual cells—and a series of chemical combinations in each cell. Depending on the internal state of a cell, certain genes will be "switched on." The products of the genes, the proteins, will react with one another and with other molecules within the cell. New genes may become active, previously productive genes may go into retirement. As the cell's internal state changes, its relations with other cells may alter, through cellular motion, through changes of shape, or through cell division. In the process new contacts may be made with other cells. Molecules may be transported across cell membranes, yielding a novel chemical state within the cell. Further genes may become active. Finally, as the result of a long and complex series of exquisitely timed reactions, we have an organism with certain observable features. If we now focus on one feature, asking ourselves how that feature has been affected by the genes and the environment, it is easy to see that there are many possible ways for the end product to be altered. Typically there will be a host of gene products that have to be available at just the right times. In environments where the developing organism is stimulated in certain ways, it will not be able to obtain the molecules it needs to continue the ordinary sequence of cellular divisions, motions, and interactions. (This is especially obvious in very dramatic cases, as when a developing mammal is deprived of food and water; but such dramatic cases are only the tip of the iceberg.) Given certain sorts of gene mutations, or simply an unusual combination of genes, particular molecules may be unavailable at the stages at which they are needed. So we can point to a host of genes and a host of environmental factors, and claim that, had any one of them been appropriately different, the final result would have been changed. The shoe might have been lost for want of any of a large number of nails.

Developing organisms are buffered against catastrophe, and our appreciation of the fine timing with which reactions occur in the growth of an organism should be tempered by recognition that backup systems are often available if matters go awry. Even if the normal causal sequence breaks down, the organism may still contrive to reach its usual end state by following an alternative route. Nevertheless, the moral of the last paragraph stands. The organism comes to be as it is because of a complex interaction. If some of its properties are stable under relatively large changes in environment or genetic constitution, that is often because, under different circumstances, different complex sequences of reactions would generate the same trait. The availability of alternative routes in no way detracts from the actual causal efficacy of a host of genes and environmental factors. Consider an analogy. Colonel Custard died because Major Mango shot him. Had Mango missed, the colonel would have drunk the poisoned martini on the table before him, prepared by Private Prune. Mango's counsel would be ill advised to plead that the accused's actions were not causally responsible for the death on the grounds that Custard would have been killed anyway. Custard went out with a bang, not a gurgle.

We do not literally pass our phenotypic characteristics on to our progeny. The idea of eyes and noses—or, more pertinently, talents and dispositions—being handed down across the generations is a myth. Out of the recognition that it is a myth comes the "most important intellectual controversy of our generation." We give our children particular protein makers. What exactly does the gift entail?

The ideas that I have been reviewing are commonplace. None of those who participated in the fierce debate about the merits of Wilson's early pop sociobiology questions the picture of development as involving complex interaction between genes and environment. Yet it is a convenient tactic to portray one's opponents as denying the commonplace. Tactical convenience breeds caricatures, and the true debate is never joined.

As we have seen, those inspired by Wilson's early pop sociobiology view themselves as identifying the limits that human genes place on human behavior and on the development of human social systems. (Reminder: Early Wilsonians are not the only pop sociobiologists, and their self-image should not be attributed to those, such as Richard Alexander, who have different ideas.) Their claims frequently give offence, for they appear to foreclose the possibility of the kinds of society hoped for by those who suffer most from present social arrangements. Even in the mollifying terms of *On Human Nature*, we are told that there may be "unmeasurable costs" involved in trying to implement certain ideas of social justice. Critics are quick to react. The conclusions that quicken the conservative pulse, they assert, are obtained at the cost of denying the commonplace. Wilson and his followers have fallen into a well-known trap in theorizing about human nature. Some critics even believe that the lapse is no accident but reflects the way in which dominant ideology shapes scientific research (Lewontin, Rose, and Kamin 1984).

Pop sociobiology, in its early Wilsonian form, revives a familiar flop, the tawdry drama of genetic determinism. So, at least, claim the critics, whose favorite metaphor is the iron hand of the genes. Everybody acknowledges that our genotypes set limits to the ways in which we can behave; all human genotypes are such that, whatever environmental manipulations we make, humans will never be able to fly simply by flapping their arms. Wilson is charged with confusing this innocuous idea with a stronger genetic constraint and thus supposing that there are genes that direct—or determine—specific pieces of behavior, no matter what the environment.

Gould makes the criticism with characteristic lucidity in an influential review of *Sociobiology*:

> Wilson's primary aim, as I read him, is to suggest that Darwinian theory might reformulate the human sciences just as it previously transformed so many other biological disciplines. But Darwinian processes cannot operate without genes to select. Unless the "interesting" properties of human behavior are under specific genetic control, sociology need fear no invasion of its turf. By interesting, I refer to the subjects sociologists and anthropologists fight about most often—aggression, social stratification, and differences in behavior between men and women. If genes only specify that we are large enough to live in a world of gravitational forces, need to rest our bodies by sleeping, and do not photosynthesize, then the realm of genetic determinism will be relatively uninspiring. (1977, 253)

Wilson is aware of the elementary fact that phenotypes are the product of an interaction between genes and environment, and Gould is aware that Wilson is so aware. Gould quotes Wilson's remark that "the genes have given away most of their sovereignty" (1975a, 550). Moreover, Wilson's subsequent writings abound with explicit disavowals of the view that genes determine human behavior and with metaphors intended to convey his ideas about the relation between genes and behavior. Yet critics galore follow Gould in contending that Wilson cannot be serious (see, for example, Lewontin, Rose, and Kamin 1984). Their assessment rests on the kind of argument Gould provides in the passage I have quoted.

I am not concerned with fathoming Wilson's exact intentions. The crucial issue is not whether Wilson believes in the position Gould ascribes to him, whether he really holds the view that he professes when pressed by his critics, or whether he oscillates between the two according to the phases of the moon. I am interested in trying to find the best argument behind the early Wilsonian version of pop sociobiology. Thus we can abandon speculative psychology in favor of attention to matters of logic. Does Gould's reasoning show that, on pain of trivializing his enterprise, Wilson is logically committed to the theses about genetic determination that he disavows?

No. The connections in this area are much more elusive than Gould's remarks suggest. Even if we cannot suppose that biology is the key to all human behavior, the recognition that genes are causally relevant to the development of behavior might make massive changes in some social scientific circles. Scientific revolutions are sometimes born of awareness that certain extra variables need to be considered. More important, there is a non sequitur. True enough, showing that genes control our inability to photosynthesize does not a revolution make. It does not follow that revealing how the genes limit our range of possible forms of behavior in the area of sexual relations would be equally boring. The Wilson program does not depend on genetic determinism for its excitement.

I shall develop the point in detail in the next section. For the moment, let us consider a simple way in which Wilson could offer provocative conclusions without embracing the doctrines Gould attributes to him. It is possible to argue that male propensities for parental care are not genetically determined: there are some environments in which males grow up to be loving and conscientious parents. Similarly, it is possible to deny that females are genetically bound to reject promiscuity: there are environments in which females develop dispositions to great sexual freedom. There is no inconsistency in now claiming that our genes preclude the combination. If the environments that dispose males to parental care do not overlap the environments that prompt females to promiscuity, then, without any crude commitment to genetic determinism (of the kind that Gould envisages), pop sociobiologists can still maintain that they advance revolutionary conclusions. Although the example is an artificial one, it is not entirely divorced from the sociobiological literature. At the end of a scholarly article on monogamy in mammals, Devra Kleiman suggests that the ideals of some feminists—increased male parental care, increased female sexual freedom—may be unattainable. The reason? They are "biologically inconsistent" (Kleiman 1977, 62).

The foes of sociobiology invent a myth, the myth of the iron hand of the genes. The myth cannot be regarded as integral to Wilson's version of pop sociobiology. The champions of pop sociobiology avail themselves of similar tactics, however. The blistering attack of the Sociobiology Study Group provoked Wilson to a quick defense, and in quick defense he concocted his own myth. The critics, he claimed, believe in the "infinite malleability" of human beings. "They postulate that human beings need only decide on the kind of society they wish, and then find ways to bring it into being" (Caplan 1978, 292).

So Wilson and his followers appropriate for themselves the sensible position that phenotypes—including behavioral characteristics—result from an interaction between genes and environment (see, for example, Barash 1977, 39–43). Opponents are assigned the myth of the blank mind, and in recent theoretical developments much attention has been lavished on the problem of showing that blank minds would be eliminated in the course of evolution (Lumsden and Wilson 1981).

What initially appears as a furious debate quickly dissolves into a tempest in a teapot. Pop sociobiologists and their opponents agree that genes and environment together determine phenotype, and that is the end of the matter. This conclusion ought to be disquieting. How have intelligent people managed to convince themselves that they have deep differences? I think that the illusion of a particular type of disagreement can easily be replaced by an illusion of agreement. For nearly a decade the iron hand of the gene has wrestled with the blank mind. Nobody believes in the iron hand of the gene, and nobody believes in the blank mind. Everybody honors the picture of the inheritance of genes and the complicated development of phenotypes that I have outlined in this section. Yet there is still an important divergence of opinion, a debate not about "genetic determinism" or "cultural determinism," a debate not readily captured in a single formula. To understand the early Wilson version of pop sociobiology and the position of its critics, we have to move beyond the public postures.

Fixed Proteins and Protean Organisms

Proteus, the legendary sea god, could assume any form he chose and consequently enjoyed great advantages in achieving his goals. The apparent threat of Wilson's pop sociobiology lies in its denial that we can mimic Proteus. Because of the genes that we have inherited from our ancestors, we are not sufficiently flexible to attain our social ends. However we vary the environment, we cannot create Utopia.

The threat can be made precise by borrowing one of the fundamental ideas of quantitative genetics, the notion of a *norm of reaction.* Suppose that we are interested in some property that admits of degrees—the height of a plant, for example. It would be folly to suppose that plants with a particular genotype always have a particular height. The composition of the soil in which they grow is plainly relevant to the heights they eventually achieve. We know enough about the requirements of plants to provide a convenient representation of the effects of the genotype. We can draw a graph that plots the height of a plant with the given genotype against the critical environmental variables. Our graph displays the norm of reaction of the given genotype. Suppose, for the sake of simplicity, that the only crucial factor is the amount of water the plant receives each day. Then our graph will reveal the height of the plant for different values of the "watering index," the number of liters that the plant receives per day. Obviously it would be more realistic to consider a number of other environmental variables—the acidity of the soil, the nitrogen and phosphorus content, the amount of sunlight, and so forth. Taking such factors into account would deprive us of the possibility of giving a simple two-dimensional representation of the dependence, but it is still possible to imagine a higher-dimensional generalization of the same basic idea.

Let us take a similar approach to the observable characteristics of any organism. Suppose that the genotype of the organism is fixed. The considerations of the last section make it clear that that, by itself, does not determine a unique phenotype that the organism will inevitably manifest. However, we can ask for the way in which the phenotype will vary across all possible environments (or, perhaps, all possible environments in which the organism can survive). By analogy with the notion of a norm of reaction, we can associate with the genotype a function that, for any possible environment, assigns the phenotype the organism will manifest in that environment. We expect that different genotypes will be associated with different functions and that we shall be able to compare the effects of different genotypes by looking at the functions associated with them.

In its simplest form the disagreement that lurks behind the rhetoric about genetic and cultural determinism is a disagreement about the forms of the functions when the genotypes we consider are human genotypes and the phenotypic properties in whose variation we are interested are the kinds of properties that anthropologists squabble about. To a first approximation, Wilson and his followers believe that the values of the functions vary relatively little and that they do so only when the environment is quite drastically altered. The critics maintain that the values of the functions are quite responsive to changes in environmental variables. Each side may justly claim to have absorbed the commonplace story about genes and development. There is still a genuine difference, deriving from alternative articulations of the story.

The norm of reaction of a genotype is a function that assigns a phenotypic value to each appropriate argument. An appropriate argument is some combination of the critical environmental variables—the amount of water added, the acidity of the soil, the amount of sunlight, and so forth. For plant geneticists, norms of reaction reduce (in the literal sense of "reduce") much more complex and unmanageable mappings. In principle, we could consider the function that takes any possible plant environment onto the height of the plant, but nothing would be gained by doing so. We organize the set of possible environments by picking out the critical variables and focusing our attention on the changes that result from varying just these factors. When we compare two plants with different genotypes, we do so by looking at the two functions that assign heights to different combinations of the critical environmental variables. Plant genetics has no need to differentiate environments in which kindly gardeners sing lullabies to their budding shoots. At least, not yet.

Human behavior is another matter. In this domain we may speculate about environmental variables that may be relevant, but it would be rash to assume that we already know how to identify all the critical factors. So the task of investigating human behavioral flexibility must be approached by way of the "unreduced" mapping that takes a possible environment onto some measure of the behavior in which we are interested. The disputes that underlie the sterile exchange about "genetic determinism" often concern the merits of attempts to argue that a particular reduction of the environmental variables effectively represents this complex mapping. The parties on one side claim that we can organize the vast collection of possible environments by concentrating solely on certain environmental variables; any possible variation in phenotype is supposed to be available by modifying some of the selected environmental variables. Their opponents contend that the simplification overlooks possibilities of phenotypic variation that would only be revealed by altering different features of the environment.

To appreciate the character of the disputes, let us begin with a hypothetical example. Suppose that it is alleged that women are by nature more disposed to spend time in child rearing than men. What could this claim mean?

Consider any female genotype. With respect to this genotype, there is a mapping that assigns to each possible environment a measure of the willingness to spend time in child rearing. (For the sake of simplicity let us assume that actual time spent in child rearing is an appropriate measure. This is obviously implausible, but our present concern is to understand what a claim about human nature might *mean*, not to assess its truth.) By averaging out the assigned values for different female genotypes, we can construct a composite mapping that represents the dependence of the disposition to spend time in child rearing on possible environments for some kind of "average woman." The details of the construction are as follows. Fix any environment. Take a particular female genotype and find the value of the measure of the willingness to spend time in child rearing for that genotype in that environment. Repeat the process for all other female genotypes in the same environment. Average the values obtained. This average value is now the value for the composite mapping in that environment. Repeat the procedure for each possible environment.

Now one obvious interpretation of our hypothetical claim is the suggestion that the value of the mapping for the "average female" is always greater than the value for the "average male." On this strong construal the claim would be analyzed as follows:

> (A) For any possible environment, the value of the child-rearing propensity is greater for the "average woman" than it is for the "average man."

There is a simple way to present (A). Let us say that a state is *precluded* for a given genotype (or collection of genotypes) if there is no possible environment in which that genotype (or collection of genotypes) attains that state. Then (A) is simply the claim that, for the composite mappings drawn from male and female human genotypes, the state in which males have a propensity to spend time in child rearing that is greater than or equal to that of females is a precluded state.

(A) is not the only construal of our hypothetical claim, however, even if we interpret it as a proposal about average males and average females. A weaker suggestion is that we can indeed achieve a state in which males have an equal enthusiasm for child rearing, but that this can only be done at considerable cost. (Perhaps it can only be attained in situations in which all parents are extremely reluctant to care for their children. A possible example is the sorry state of the Ik, studied by Colin Turnbull [1972].) The only possible environments in which the state is attained are highly undesirable. Here is an analysis of the weaker proposal:

> (B) There is a collection of desirable properties (desiderata) such that any possible environment in which the value of the child-rearing propensity is equal for the "average male" and the "average female" is an environment in which at least one of the desiderata is absent.

In other words, (B) tells us that the state in which equal propensities to rear children are accompanied by all our cherished human institutions is precluded.

I think that statements like (A) and (B) make explicit what most pop sociobiologists have in mind when they talk about limits set by human nature. It should be obvious that claims like these are neither unexciting nor committed to the simplistic version of "genetic determinism" often ascribed to Wilson and his followers. It should be equally obvious that there are many alternative analyses that might be offered. Our envisaged composite functions could be compared in other ways. We could weight genotypes or weight environments. We could also resist the idea of contrasting "average female" and "average male" values in favor of a direct comparison between individuals. I shall not explore these alternatives, not only because I doubt that they represent the intentions of any flesh-and-blood pop sociobiologist, but also because the considerations relevant to discussing (A) and (B) seem to me to be equally pertinent to other possible construals.

My confidence that (A) and (B) represent what prominent pop sociobiologists have in mind is based on what they say. Describing the aims of his version of sociobiology, Barash writes, "the process of evolution, operating on human beings, has produced a creature for whom certain behaviors just don't go at all, whereas others go very well indeed" (1979, 11). Wilson is more canny, opting for (B) rather than the more provocative (A): "There is a cost, which no one yet can measure, awaiting the society that moves either from juridical equality of opportunity between the sexes to a statistical equality of their performance in the professions, or back [*sic*] toward deliberate sexual discrimination" (1978, 147). Talk of composite mappings, possible environments, and precluded states may seem artificial in contrast with the plain idiom in which Barash and Wilson announce their conclusions. Yet the unnatural idiom serves its purpose in enabling us to see clearly how the pop sociobiological view is compatible with conventional wisdom about gene-environment interaction.

My reformulation also reveals what Herculean labors await those who hope to arrive at conclusions about the limits of human nature. Comparison of actual behavior is not enough. We cannot compare the overall behavior of two functions by looking at their values for a single argument or for arguments within a small interval: if we only consider values between 0 and 1, x is greater than x^2; this does not show us that the value of the former function is always greater than that of the latter. Yet, as Wilson's critics have repeatedly pointed out, there is a long and dismal history of drawing grand conclusions from just such comparisons (see Lewontin 1976; Gould 1981). A quick look at actual behavior and at behavioral differences among groups has all too frequently served to buttress hypotheses about the fixity of human institutions and the impossibility of eradicating inequalities among races or classes. Plant breeders who inferred the qualities of rival strains from consideration of relative vigor in a single environment, or from casual inspection of a collection of environments, would have a pronounced tendency to go rapidly out of business. By contrast, their imitators in the behavioral sciences usually seem to thrive.

Ironically, it is the immensity of our ignorance about the environmental influences on human behavior that enables behavioral scientists to practice methods that would doom their plant-breeding cousins. In the human case we lack the lore that enables a cautious plant breeder to arrive at a justified assessment of the relative merits of particular strains. We have no representation of the collection of possible environments that will reduce it to a space of manageable dimensions. We know, in a dim and unsystematic way, that features of child rearing and of cultural history can make profound differences in the behavior of individual human beings. What eludes us is the detail, the behavioral counterpart of the adjustments of pH or the nitrogen content.

If Wilson's ladder is to enable us to climb from nature to self-knowledge, then it must surmount the old problem of the casual comparison. We shall have to achieve some clear view of the kinds of changes in environment that would be critical for changes in various kinds of human behavior. We shall have to be given evidence that the forms of behavior and social institutions alleged to be stable—or, perhaps, modifiable only at great cost—really do remain constant when the crucial variables vary. Wilson's version of pop sociobiology has so far emerged as an *intelligible* program, and we have been able to understand its conclusions without assuming them to depend on denying the commonplace. However, to understand it is one thing. To see if it is plausible is quite another.

The One and the Many

The time has come to make a distinction that I have so far cheerfully blurred. Wilson and his followers are interested in deriving conclusions about human nature, about limits on the behavior of individuals or, perhaps, limits on their dispositions to behave in various ways. Yet this is not their only concern. Pop sociobiologists are devoted to certain human institutions: home, family, and maternal care, to name but three. They hope to show that such institutions will be permanent features of our social condition, that they are grounded in behavioral characteristics that are, in their turn, extremely stable.

Just as the passage from the genotype to the phenotype is fraught with complications, so too there is no easy bridge between the behavior (or behavioral propensities) of individuals and the character of the society to which they belong. The first point to recognize is that the aspirations and attitudes of individuals are typically shaped by the institutions of prior generations. (This is a leading theme of Bock 1980; Wilson attempts to come to terms with it in his recent work on gene-culture coevolution.) Second, societal institutions and societal attitudes need not mirror the aspirations of individuals. Nations may be aggressive despite the fact that a majority of their citizens are peace loving—witness the Germany of the 1930s. There may be institutions promoting inequalities of race, sex, and class, even though individuals would prefer to treat one another as equals. To emphasize these points is not to invoke some mysterious "Force of Culture" that is responsible both for shaping the ideas of developing individuals and for distorting the societal expression of those ideas. It is simply to recall the obvious facts that human social environments reflect human history and that, when groups of people interact, the arrangements they reach may be wildly at odds with their individual preferences.

Thomas Schelling has provided some beautiful illustrations of the latter point (Schelling 1978). Some of them involve everyday occurrences, small irritants in our social lives. Most people have found themselves on an expressway clogged with traffic because the drivers ahead have slowed down to peek at an accident on the opposite side. We would willingly forgo the chance to take a look if everyone else would do likewise. The other people in the traffic jam share this preference. However, when we arrive at the scene, there is no further cost to ourselves in indulging our curiosity, and so we make our own small contribution to delaying those who are behind us.

There are other examples that concern matters of importance. Consider the assorting of individuals according to race or class. Suppose that members of Group I have a range of tolerance for living in the same area as members of Group II. The most tolerant people in Group I are happy with a situation in which they are outnumbered 2:1. The least tolerant will be unhappy if they have any Group II neighbors at all. The median members of Group I are happy if the ratio is 1:1. Group II people have exactly the same distribution of attitudes toward Group I people. Now suppose that there are many more people in Group I than in Group II. How will people distribute themselves into neighborhoods? (We are suppressing all kinds of complications that arise in realistic situations—ability to afford particular kinds of housing, and so forth.) Much depends on the initial conditions and the details of the dynamics of movement. However, it is relatively easy to reach a situation in which all members of Group I live together and all members of Group II live together. (See Schelling 1978, 157ff., for details.) This result is quite compatible with the assumption that most members of both groups are either very happy or only mildly unhappy with a situation in which the ratio is 1:1.

We can now begin to see that there are two distinct ways in which goals of social justice might be protected against the pop sociobiological charge that they fall afoul of human nature. The more obvious is to suggest that our present social structure and our cultural history are important determinants of the forms of behavior and the attitudes that individual human beings develop. Implementing this strategy would lead to the type of debate envisaged in the last section. Pop sociobiologists would insist that the forms of behavior are relatively resistant to modification through adjustment of our social environments. Their opponents would claim that appropriate social changes can alter the forms of behavior and the underlying propensities.

Less obvious is the strategy of denying that our present social arrangements accurately reflect our individual preferences and propensities. It is possible to concede that those individual attitudes are relatively invulnerable to change through modification of the social environment, but still to deny that our institutions are unalterable. Just as genotypes do not determine phenotypes, so too, individual propensities to behavior do not determine the character of a society.

To revert to our example, the clustering of people by race need not reflect any individual racial prejudice. It is possible that most of the people in a society would prefer to live in racially mixed neighborhoods, but that accidents of initial distribution and initial movements should produce a collection of racially homogeneous neighborhoods. The social institutions that arise from a collection of individual propensities may be crucially shaped by an accident of history. The social system begins to go in a particular direction, and once it does, it has its own momentum. (In appreciating this point, we should not lose sight of the other important fact, that individual propensities are themselves shaped by social arrangements. Thus the dynamics is even more complicated than our simple example indicates.)

As in our study of gene-environment interactions, we can give a more precise analysis of the type of relationship envisaged here. Suppose for the sake of the present discussion that the propensities of individuals are not altered by changes in the social environment. We want to understand the societal implications of the individual propensities of the people who make up the society. We regard these people as interacting with one another to refashion the institutions of the society into which they are born. So we associate with a particular set of people having particular characteristics a mapping that assigns to each possible social context the social situation that would result from the response of those people to that context.

The idea behind the view that our present social arrangements reflect accidents of history can now be stated quite simply. Given certain initial social contexts, a collection of people with common behavioral propensities will develop certain social institutions; once in place, these social institutions will belong to the initial social contexts of subsequent generations and will be stable. There are alternative social contexts, however, in which alternative social institutions develop and are equally stable. Consider a simple example (suggested to me by Elliott Sober): we want to use the same language to communicate with one another; it is an accident of history that we use English and not Chinese.

The accidents of history put us into one social tradition—one sequence of social contexts—rather than another, and we can only remove ourselves from this tradition by conscious social engineering. By engaging in social engineering we reach an alternative stable set of social institutions. Since our common behavioral attitudes are compatible with both social traditions, we are not compelled to regard our present social arrangements as the only ones that are possible for us. This, I suggest, is the major claim of those of Wilson's critics who have emphasized the role of history and culture (Sahlins 1976; Bock 1980). That claim is intelligible, it is by no means obviously false, and it is not committed to the existence of dubious entities (mysterious "Forces of Culture").

Thoughtful people should not wed themselves to either one of the two strategies I have envisaged as responses to Wilson's early pop sociobiology. They should insist both that changes in social environment can effect changes in behavioral propensities and that the social environment itself is the product not only of individual attitudes but of the prior social arrangements that have been developed. The most extreme versions of pop sociobiology contend that human genotypes preclude certain forms of behavior and that our actual behavioral dispositions preclude certain social arrangements. We ought to be suspicious about both types of claims.

In recognizing the need for an extra step in defending some sociobiological conclusions, the need to justify projecting the structure of society from the behavioral inclinations of individuals, I have touched on a problem that has moved Wilson to go beyond his early program. (Wilson notes that this move was inspired by the challenges of Bock [1980], Harris [1979], and Sahlins [1976]. See Lumsden and

Wilson 1983a, 44.) The theory of gene-culture coevolution, developed by Lumsden and Wilson, is an attempt to respond to the kind of difficulty that I have been describing. Lumsden and Wilson hope to show how the institutions of a society are determined by the behavioral propensities of the members of the society. In chapter 10 we shall consider the extent to which their hopes are well-founded.

For the time being, however, I want to set to one side the problem of the one and the many in favor of the more straightforward approach of Wilson's early program. Even before we ask whether the flaws Wilson now perceives in that program are indeed corrected by his most recent efforts (see Lumsden and Wilson 1983a, 47–50), we should consider whether there are other defects that ought to be addressed. Even if our goal is simply to understand the genetic limits on the behavior of individual human beings, can we reasonably hope to attain it in the way that Wilson suggests?

Short Cuts and Blind Alleys

My aim has been to clarify the kinds of conclusions at which pop sociobiology is directed. I have been trying to find the points against which the top end of Wilson's ladder is supposed to rest. However, once we have identified the target area, it is natural to wonder about the route. Why should a search for conclusions about the genetic limits on human behavior begin with evolutionary theory and with the behavior of nonhumans? Why not take a simple short cut and investigate ourselves, as we are, here and now?

There is, of course, a science of the genetics of human behavior, and it has some notable discoveries to its credit. The most convincing results are instances in which pathological conditions have behavioral effects, as in the case of various metabolic disorders and defects in color vision (see Ehrman and Parsons 1981, 281–285, 288–291). These hardly provide a basis for the grand conclusions after which pop sociobiologists hanker. Because the types of behavior most susceptible to rigorous genetic analysis are not those that pop sociobiology finds most interesting, the writings of pop sociobiologists do not brim over with technical reports from human behavioral genetics—although Wilson and his followers are not too proud to advertise any promising suggestions when the moment seems right (see Wilson 1978, 47). As in the early days of classical genetics, when the fruit fly was the geneticist's best friend and when mutants were gifts of the population cage, not artifacts of X-ray bombardment, the kinds of genetic systems that are best understood in human beings are those in which variation produces markedly deleterious effects. Human geneticists can look sadly on as their colleagues employ the impressive arsenal of classical and molecular techniques developed in this century. Their own hands are tied.

Orwellian fantasies aside, it is considered poor form to subject people to the kinds of procedures that are used in rigorous genetic analysis of nonhuman animals. Breeding pure lines, rearing us in controlled environments, irradiating us to induce mutations, inserting genetic markers, and like tactics are clearly out of the question. Moreover, even if we were to be grossly insensitive to the ethical considerations, the long wait from birth to reproduction makes us poor subjects for classical genetic analysis. The hope of human behavioral genetics is that, without interference, it will be possible to trace the features of the genetic components in human behavior from the unsystematic collection of human genotypes and environments that are actually given to us. One prominent and familiar method is to investigate cases in which monozygotic twins (twins who originate from the same fertilized egg and who thus usually have the same genotype) are reared in different environments. Yet, while geneticists may yearn for a world in which monozygotic twins are born in profusion and in which they are reared completely apart in radically different environments, that world is not ours. In consequence, the task of achieving justified views about the genetics of human behavior is difficult and painstaking.

If we are not prepared to wait for the slow and cautious accumulation of conclusions by classical methods, for the patient survey of interactions between genes and environments as they haphazardly occur, for the development of biochemical techniques and of the tools of molecular biology, if we want a grand theory of human nature and we want it now, what can we do? There is no short and direct route to constructing the function that, for a fixed genotype, maps possible environments onto a behavioral phenotype. With respect to interesting human characteristics—such as the notorious example of human intelligence—there are well-known short cuts that end in blind alleys (see Block and Dworkin 1976a). The ambitious student of human behavior needs something new.

Frustrated by the cautious plodding of behavioral genetics, pop sociobiologists, like Ardrey, Lorenz, and Morris before them, turn to our evolutionary history. We have the genes we do because we inherited them from our ancestors. Perhaps we can learn something about them by investigating animal behavior, by understanding how it is adapted, by appreciating the selective forces that have been at work in evolution. But how exactly can we learn? For Ardrey and Morris it is simply a matter of seeing suggestive analogies. Wilson and his followers are more systematic. They try to build a ladder from nature up.

The central issue in the sociobiology controversy is whether there is a firm ladder that will take pop sociobiologists where they want to go. The need for the ladder is clear. We have managed to identify the intended terminus. It is now time to return to the origin.