Chapter Six

Narrative and Interpretation in the Sociobiology *Controversy*

When E. O. Wilson's *Sociobiology: The New Synthesis* was published in 1975, it was given wide publicity, in newspapers and general-interest magazines, as well as in the popular science press and the journals in biology and the social sciences. Most reviews by behavioral biologists welcomed it, viewing it as a classic survey of the biological literature that drew attention to important and rapidly developing work that had not been brought together before because it fell between several established specialties. But there were also immediate criticisms by researchers in comparative psychology, genetics, and anthropology, and by groups of scientists in the United States and in Britain who were concerned about the political implications of the book. The arguments have continued since then. The debate over *Sociobiology* offers the chance to study a controversy like that among *Cnemidophorus* researchers in chapter 4, but a controversy that goes beyond one group of researchers and enters the public forum. It also offers the chance to study the ways popularizations use narrative to make claims for the relevance and authority of the methods of various disciplines, and the ways popularizations can dismiss rival disciplines. Wilson's book is unusual in its size, in its format, in its place in the author's career, and in its audience—what other recent scientific book is both a synthesis for specialists and a polemic addressed to a wide academic audience? But the dynamics of the controversy are not unusual (we could compare it to many other controversies in the public realm), and it can help us understand how the discourses of various biological disciplines relate to each other, and how the discourse of biology relates to political discourse.

The literature on this book over the past ten years is rather daunting; I am drawing on half a dozen books and collections of papers devoted to it and about twenty or so reviews in journals, and I am aware that I am overlooking a great deal. There have been a number of methodological studies by philosophers, some good historical and sociological studies, and even some studies of studies.[1] Rather than rehash the arguments presented in these texts, I would like to look at the texts themselves. I am going to argue that part of the power of Wilson's book is in its narrative structure, and that the controversy that followed can be seen in terms of the interpretation and ironic reinterpretation of that narrative and other competing narratives.

What I've missed so far, in the mountain of reviews by supporters and critics of sociobiology, is an explanation of just why Wilson's book is so persuasive, why I, a nonbiologist whose ideas about society are quite different from Wilson's, and who read the book after years of reading only its critics, could read it from cover to cover, suspending for the duration my disbelief. To attempt an explanation of this rhetorical power, I am going to argue that Wilson in *Sociobiology* incorporates and transforms the conventional narrative of natural history texts, with their sense of an immediate encounter with nature, by stripping them of narrative elements and then reconstructing the fragments into a grand narrative of evolutionary adaptation. And I will treat the criticisms of the book in the same way, not as arguments that make their points more or less conclusively, but as texts that reconstruct *Sociobiology*, putting it in a new context, transforming its narratives, and accounting for it. I am going to use the texts sociologically to examine one instance of the processes through which the authority of science is established and is applied in public controversy.

When I say *Sociobiology* is a narrative I do not mean to imply that Wilson is doing something unscientific. (See chapter 4, notes 4 and 7 for a parallel problem.) I must emphasize this because both Wilson and his critics criticize the telling of stories as a resource in scientific rhetoric. Wilson's critics say he tells "Just-So Stories" of adaptation. And Wilson is at pains to separate himself from such popular sociobiological writers as Lionel Tiger and Robin Fox, whose works advocate hypotheses by "selecting and arranging . . . evidence in the

1. The most useful studies for my approach were by David Hull ("Scientific Bandwagon or Travelling Medicine Show?"), Gerald Holton ("The New Synthesis?"), and Ullica Segerstrale ("Colleagues in Conflict"). Segerstrale's article is particularly helpful in giving details of the complicated context in the discipline. The philosophical studies by Ruse, Burian, and Dunbar were also helpful. As I note later, Joe Crocker has a good analysis of the political critique of sociobiology, and W. R. Albury has a good analysis of sociobiologists' responses to the political critique, though I think he fails to apply a similar analysis to the critics. In "Sociobiology and Ideology: The Unfinished Trajectory" Martin Barker makes some comments on his earlier analysis in *The New Racism* that are relevant to some of the issues I discuss later in the chapter and in my "Conclusion."

most persuasive manner possible" so that "verbal skill . . . becomes a significant factor" (p. 28).[2] The verbal skill of many of the participants in the controversy, their selecting and arranging, is precisely what interests me. What makes Wilson's book different from those of popular writers like Tiger, Fox, Desmond Morris, and Robert Ardrey—and it is different—is not his method of "postulational-deductive model building" or "strong inference" (the terms he uses to describe his method) but his way of making his model seem to correspond with perceived reality. He does this by inserting a narrative of natural history, which we associate with reality, within a narrative of evolution, which we associate with model-building.

The Narrative of Natural History

Wilson is ambivalent about the power of natural history. He seems to use the term in a favorable sense when he begins his last chapter by asking us to look at man in "the free spirit of natural history." But through most of the book he uses the phrase to describe disciplines like sociology, psycholinguistics, or studies of mammalian behavior that he thinks have not yet developed out of the messy adolescent phase of inquiry to become mature sciences. Natural history is the opposite pole from developed theory; he warns that "natural history is sometimes so diverting, to the point of making one forget the main thrust of the theory" (p. 32). And it is indeed diverting: even such critics as the anthropologist S. L. Washburn and the sociologist Bruce Eckland admit their fascination with the details of animal behavior collected in the book.

For my purposes, natural history is neither a stage of disciplinary history, mature or immature, nor the subject matter of animals and plants, but a kind of text. Natural history gives a written account of actions of particular animals at a particular place or time, recorded by particular observers, as in this passage from Darwin's *Journal* of the voyage of the Beagle:

> I took the boat and rowed some distance up this creek. It was very narrow, winding, and deep; on each side a wall thirty or forty feet high, formed by trees intertwined with creepers, gave to the canal a singularly gloomy appearance. I saw there a very extraordinary

2. All references in parentheses without further details are to pages in Wilson's *Sociobiology*. Other works cited by short title in the text or notes are listed in the Reference List, section 5, "Texts Discussed: Chapter 6."

> bird, called the Scissor-beak (*Rhynchops nigra*). It has short legs, web feet, extremely long-pointed wings, and is about the size of a tern. The beak is flattened laterally—that is in a plane at right angles to that of the spoonbill or the duck. It is as flat and elastic as an ivory paper-cutter, and the lower mandible, differently from every other bird, is an inch and a half longer than the upper.[3]

The passage suggests some of the textual signals we can use to define the natural history narratives in *Sociobiology*.

- The use of the past tense, the tense used in English for particular moments in the past.
- The presence of apparently gratuitous details of time and place.
- The treatment of animals as individual, sometimes anthropomorphized, characters.
- The attention to the observer's perspective and response, and especially to what seems remarkable or strange.

The bits of natural history in *Sociobiology* that have these features are nearly all quotations from other observers, so one might think they were irrelevant to Wilson's own methods. But Wilson is unusually generous with such quotations, letting them have their say, and they play an important part in the texture of his narrative. Earlier natural historians comparing and classifying the forms of animals brought back specimens and had them stuffed and collected in museums. The quotations are Wilson's way of bringing back specimens of behavior.

The Past Tense

In natural history, things happen. Such events are indicated in natural history texts such as the passage just quoted from Darwin's *Journal*, by the use of the past tense. In contrast, the present tense usually indicates, in scientific texts, the general nature of the phenomenon being described, asserting that it is true at all times. The effect of the shift in tense can be seen in a quotation Wilson uses (p. 135):

> According to Schaller (1972), "Wildebeest sometimes stampede toward a river from as much as 1 km away. The long column of animals hits the river at a run, and if the embankment is steep and the water deep the lead animals are slowed down while those behind continue to press forward until the river turns into a lowing, churning mass of

3. Charles Darwin, *Journal . . . During the Voyage . . . of H.M.S. 'Beagle'* (London: John Murray, 1901), p. 146.

> animals some of which are trampled and drowned. One such herd I observed at Seronera left seven dead behind; several hundred may drown in such circumstances."

The first sentence states a general fact, with an indefinite article ("*a* river") and a measurement of distance but not a location. The second sentence tells an exciting story, but in general form; we are to imagine this happening now and then, here and there, constituting one item in the behavioral repertoire of wildebeest. The first part of the third sentence shifts to past tense, to tell about *one* herd, which *I* observed, at a particular place ("Seronera") with a specific number of deaths as a result. Finally, after the semicolon, the event is generalized, again in present tense.

Such shifts to past tense narratives occur throughout *Sociobiology,* in accounts of birds mobbing (p. 47), gazelles stotting (p. 124), wasps fighting (p. 284), wild dogs adopting cubs (p. 125), or chimpanzees using tools (p. 173).

> *Use of leaves for body wiping.* The Gombe stream chimpanzees commonly used leaves to wipe their body free of feces, blood, urine, semen, and various forms of sticky foreign material such as overripe bananas. "A 3-year old, dangling above a visiting scientist, Professor R. A. Hinde, wiped her foot vigorously with leaves after stamping on his hair" (Van Lawick-Goodall, 1968a).

That this is in the past tense marks it as a statement about one particular group of chimpanzees. When the general statement is followed by a quoted passage we would expect a particular incident supporting the general statement; the practical joke comes when we see it is also an embarrassing anecdote involving a particular (and eminent) victim, whose hair is apparently classed with overripe bananas as "sticky foreign material."

For a natural historian, even the Darwin of the Beagle *Journal,* the anecdotes are the point, and the scientific generalizations are framed within the narrative. Wilson, on the other hand, frames these narrative accounts in the present tense of scientific generalization. But the past tense particulars have their own authority, even in passages that seem quite theoretical. Compare the effect of these two sentences from the same paragraph, the first describing a mathematical model and the second describing an observation (p. 326):

> As Trivers has pointed out, there may come a time when the investments of both partners are so great that natural selection will favor desertion by either partner even if the investment of one is proportionately less.

> Rowley (1965) described a parallel episode in the Australian superb blue wren *Malorus cyaneus*. Two neighboring pairs happened to fledge their young simultaneously and could not tell them apart, so that all were fed indiscriminately as in a creche. One pair then deserted in order to start another brood. The remaining couple continued to care for all the young, even though they had been cheated.

The second passage describes something that was observed only once, but it is something that actually happened. Wilson does not offer it as any sort of proof, but it is persuasive nonetheless.

Gratuitous Details of Setting

Early natural history accounts were provided by travellers and explorers, so it is not surprising that a strong sense of place remains. For instance, in the Darwin passage, the fact that this bird was seen by a creek bordered with forest is ecologically relevant, but the references to a particular creek, to the height of the walls of vegetation, and to the gloomy appearance all go beyond a description of a habitat. Wilson gives gratuitous details of setting in many quoted and paraphrased passages; one example stands out because he uses it twice to show the scaling of dominance behavior (p. 444).

> When black iguanas (*Ctenosaura pectinata*) occur in less disturbed habitats, so that individuals are able to spread out, each solitary male defends a well-defined territory. Evans (1951) found a population in Mexico which was compressed into the rock wall of a cemetery. During the day the lizards went out into the adjacent cultivated fields to feed. At the rock wall retreat there was not enough space to permit multiple territories, even though the food supply in the fields was able to support a sizable population. As a result the males were organized into a two-layered dominance hierarchy. The leading male was truly a tyrant. He regularly patrolled his domain, opening his jaws to threaten any rival who hesitated to retreat into a crevice. Each subordinate possessed a small space which he defended against all but the tyrant.

The fields, the rock wall, Mexico, and the daytime are all relevant details of setting, but the fact that it is a *cemetery* wall is not ecologically significant. Still, it sticks in our minds, as it must have stuck in Wilson's, and surely it colors the highly anthropomorphic story that follows. Similarly, the lovely two-page drawings by Sarah Landry that illustrate Wilson's book often give a sense of a specific, named place, as well as providing general information about the conditions under which the animals live, what they eat, or what other species compete with them. Wilson's comments on frog calls give a powerful sense of setting in a style reminiscent of that of nineteenth-century naturalists: "The wailing of thousands of spadefoot toads (*Scaphiopus*) in a Florida roadside ditch, in the pitch-black darkness of a hot summer night, brings to mind the lower levels of the Inferno" (p. 443).

Animals as Characters

Animals described in biology are typical of their species, often distinguished only by the observable characteristics of sex and size. Animals in natural history are individuals like characters in novels, and they may even have names. Wilson notes that it is methodologically important that the primate ethologist can pick out individuals without marking them artificially, as must be done with the insects that Wilson studies. With chimpanzees and gorillas, "It is easy for observers to recognize individuals at a glance and even to guess their parentage with a high degree of accurancy" (p. 517). Gorillas are also recognized as individuals by other gorillas, and in one passage this recognition seems indistinguishable from that of the observer (p. 538):

> Fossey has stressed the importance of the personal idiosyncrasies of the dominant males, who control the movements of the group. One of the groups was under the control of Whinny, a silver-backed male given his name because of his inability to vocalize properly. When Whinny died, the leadership passed to the group's second silverback, Uncle Bert, who clamped down on the group's activities "like a gouty headmaster." Where the group had previously accepted Fossey's presence calmly, under Uncle Bert's command they changed to breast beating, whacking at foliage, hiding, and other signs of alarm. Soon they retreated into a more remote area higher up Mount Visoke.

The gorillas do recognize as different those individuals who have silver backs, but it is the observer who gives the names and the characterizations. The two kinds of individualization merge. Several other accounts of animals give names (pp. 214, 512), including that of the macaque Imo (p. 170), who is famous as the inventor of potato washing (and who is especially famous after she was discussed on David Attenborough's popular BBC series *Life on Earth*). She even gets an entry in Wilson's index, though she might be offended to find herself identified there as a chimp.

As the paraphrase of the Fossey account shows, anthropomorphization is common in natural history accounts. When it occurs in the eighteenth- or nineteenth-century texts that Wilson collects (pp. 281, 370, 542), one might imagine it to be an antiquated device. A particularly lovely passage by Guthrie-Smith on the sham death throes of pied stilts (one which is quoted admiringly by one of the reviewers) was written in 1925 (p. 123). But anthropomorphism also occurs in a number of the most dramatic passages from contemporary scientific texts that Wilson quotes (pp. 214, 473), as well as in his accounts of his own studies of ants, and in his coinages of new terms (p. 413).[4]

In discussing popularization, I noted D. R. Crocker's argument that anthropomorphization is not a bias added in popular texts to make them interesting, but is an unavoidable part of the scientific work of ethology that is more or less successfully concealed in the more scientific publications. One can see the human shaping of the animals' narrative in a passage Wilson quotes from an academic work by Alison Jolly (p. 278):

> "On August 16 and August 24, 1963, and in a more leisurely fashion, on March 23, 1964, a whole troop of *L. catta* barred the *Propithecus'* way, while the *Propithecus* returned their teasing. Again, the animals leaped towards each other, stared, feinted approach, but never came into contact. All the game lay in leaps and counterleaps, the *Propithecus* trying to pass through the *L. catta* troop, the *L. catta* attempting to keep in front of them, facing the other direction. Since there are about twenty *L. catta* to five *Propithecus,* the *L. catta* had an advantage; if one animal does not outguess the *Propithecus'* next move, another can do so."

Jolly's attribution to the lemurs of leisureliness, teasing, game-playing, and guessing remind us that human observers define behavior in human terms; it is the human analogy that enables us to see a series of actions as a behavior. The analogy is an old one; Wilson, with

4. Wilson responds to criticism of his anthropomorphic terminology in his *BioScience* article, "Academic Vigilantism."

his encyclopedic knowledge of eighteenth- and nineteenth-century writings on insects, reminds us of the tradition of moral fables around the ants and the bees. Ethel Tobach's highly critical review of *Sociobiology* says that Wilson concludes that "it is the social insects who have evolved the most peaceful and perfect of social organizations." And she comments, "The adage is old and worn." Perhaps it is only another way of observing the same thing to say that Wilson, in comparing the insects to humans, is not only drawing on the scientific tradition of his teacher, the entomologist William Wheeler, but is drawing on a much older literary tradition.[5]

The Perspective of the Observer

If animals are made into characters in the natural history narratives, so are their human observers, the travellers and autobiographers: Darwin rowing up the creek and being affected by the gloom of the forest wall, or the tiny figure of a painter that Thomas Cole paints in the lower left-hand corner of his huge canvas of the Oxbow in the Connecticut River. The reader is aware of observers in many of the natural history passages Wilson quotes; for instance, in the passage from Fossey that I have already quoted, the focus shifts from the observer watching the gorillas to the gorilla watching the observer. In another quoted passage, Alison Jolly recreates the observer's gradual construction of a scene (p. 530):

> "Your first impression of an *L. catta* troop is a series of tails dangling straight down among the branches like enormous fuzzy striped caterpillars. Later, with difficulty, you put together the patches of light and shade into a set of curved gray backs, of black and white spotted faces, of amber eyes. By this time, if the troop does not know you, they are already clicking to each other, and first one and then a chorus begin to mob you with high, outraged barks. The troop is quite willing to click and bark for an hour at a time in the yapping soprano of twenty ill-bred little terriers."

In the illustration of this scene drawn by Sarah Landry, the observer is made strikingly present by the gaze of the largest lemur; the caption says that "one male faces the observer with a threat stare" (p. 532). In

5. Tobach, "Multiple Review" (1976). One literary example of this tradition of moral or political fables is the charming story of the bees and the kingfisher that Hector St. Jean Crevecoeur presents as if it were a natural history observation in *Letters from an American Farmer* (1783; rpt. New York: Dutton).

another of Sarah Landry's drawings, showing a pride of lions, the caption tells us that one of the animals "stares at an unidentified object past the observer" (p. 506). If this were a photograph the object outside the frame would be unidentified, but here in a reconstruction it is, of course, imaginary. The caption, by treating the unseen object as real, emphasizes how the delimitation of the frame in these drawings makes them stand as representative of a larger world around them. In making this connection between particular observations and general statements about the world, the drawings and the natural history texts on which they are based follow the metonymic strategy some literary critics attribute to the realistic novel.[6]

There are a number of other textual means of making the reader aware of the observer, besides the pictorial devices of perspective and framing. The passage quoted from Jolly gives a sense of the observer by describing the development of her perception in time, as a narrative. Wilson admires other observers, who might be unseen in their own scientific reports of their observations, for the strenuousness and persistence of their work (p. 31), or for their exposure to danger (p. 495). Some of Wilson's illustrations present symbolically the problems of observation; for instance, one figure includes a human head to represent the observer of animal communication, and another includes two human heads, with angles of vision drawn from their eyes, to show the consequences of two observers having different definitions of populations.

We also become aware of the observer when he or she states a response, especially when they respond to the scene as something strange or extraordinary. Whereas biology texts focus on the representative and make all creatures ordinary by finding a place for them in biological description, natural history texts—like their BBC descendants today—focus on the remarkable or impressive. When Darwin comments in the passage I've quoted on "a very extraordinary bird," we are seeing Darwin as well as the bird. The natural history strain in Wilson's book is apparent in the striking pictures that show frogs, ants, or birds that are strange in appearance or behavior, and in his frequent expressions of awe at the wonders of nature:

6. I have analyzed the illustrations in *Sociobiology* in more detail in another paper, "Every Picture Tells a Story." For an influential analysis of metonymy and other rhetorical figures in the discourse of history, see Hayden White, *Metahistory* and (for a very concentrated presentation) "The Fictions of Factual Representation" in *Tropics of Discourse*.

> The males belonging to species on this list [of birds that mate after communal displays] are among the most colorful of the bird world. The brilliant red cock of the rock, for example, is easily the most spectacular cotingid, and the birds of paradise are justly considered the most beautiful of all birds. (P. 332, see also pp. 46, 220, 331, 332, 375, 423, 529)

The explanation of such colors by reference to sexual selection is characteristic of what I am calling biological texts; the response to one of these birds as beautiful is characteristic of what I am calling natural history texts. Though the bits of natural history are not representative of Wilson's usual style, and the natural history passages quoted make up only a small part of the text, they are the basis of its authority with the popular reader, because they connect all the model building to the immediate experience of nature. The quoted bits of natural history are like once-scattered specimens of behavior, all brought under one textual roof, not in the form of the emphatically unreal stuffed animals of museum dioramas, but in the form of stories.

Arrays of Information

If *Sociobiology* were just a massive anthology of natural history, it would not have aroused controversy. No observation, however awesome, horrific, or bizarre, is controversial outside some theoretical context. What makes *Sociobiology* dangerous or promising (depending on one's view) is that, like the museums Louis Agassiz or Richard Owen envisaged in the mid-nineteenth century, it projects a vision of the world, "an epitome of creation," as Agassiz's biographer called it. It seems appropriate to quote Agassiz's plan, since the Museum of Comparative Zoology that he founded now employs Wilson (as well as two of Wilson's most prominent critics).

> The casual observer . . . should walk through exhibition rooms not simply crowded with objects to delight and interest him, but so arranged that the selection of every specimen should have reference to its part and place in nature; while the whole should be so combined as to explain, so far as known, the faunal and systematic relations of animals in the actual world, and in the geological forma-

tions; or, in other words, their succession in time, and their distribution in space.[7]

What makes Wilson's book different from those of popular sociobiologists, and also different from Richard Dawkins' presentation of sociobiological ideas in *The Selfish Gene,* is the way it builds up an immense array of representations of life like the halls of a museum. This is not just to say that *Sociobiology* is a big book, bigger than those of critics or popularizers of sociobiology, though bigness is part of it. Wilson does not merely collect a large number of the narratives I have described; he arranges them so that they keep their narrative force, their immediacy, while they are stripped of all their particularity, their excess details of sequence, time, place, and perspective; he transforms narratives into information. The narrative of nature becomes the narrative of science only by passing though this stage of stripping and arranging.

As I have noted, natural history texts seek out the singular, whereas biology texts seek out the typical. Wilson is careful to emphasize that a single observation, however careful, means nothing until it can be combined with others. For instance, he criticizes one ethologist, saying "Idiosyncratic actions of individuals do not constitute roles; only regularly repeated categories fulfill the criterion" (p. 299). And he comments at one point that "one anecdote does not prove the existence of a behavior" (p. 46)—even though, further down the same page, he makes skillful use of such an anecdote about parental care in monkeys.[8]

The text can make anecdotes into behavior by combining many observations through comparisons, classifications, or models. When Wilson compares three stages of "aggressive displays" in a figure of a monkey and a bird (figure 8-3, p. 180), he must leave out the developmental and behavioral narratives implied in telling when and how these two very different species make these displays. As the compara-

7. Elizabeth Cary Agassiz, *Louis Agassiz: His Life and Correspondence* (Boston: Houghton Mifflin, 1886), vol. 2, p. 556. That the arrangement of a museum still projects a view of the world is apparent in the vociferousness of the recent controversy in the letters to *Nature* about the reorganization along cladistic lines of displays in the main hall at the Museum of Natural History in London.

8. The sociobiologist Robin Dunbar questions this argument about the transformation of anecdotes into information. He comments: "Isn't it that their function here is to bring alive abstract relationships that have been deduced either from some theoretical consideration or larger body of data? They are not *isolated* examples but *selected* examples."

tive psychologist Frank Beach points out in criticizing such comparisons, a great deal of detail is lost in them, or never found in the first place.[9] What is gained is an analytical category, "graded signals," that can be used to analyze communication in a number of species. The first comparison like this that Wilson presents in the book is apparently intended to be provocative (it is quoted at the beginning of the blurb on the dustjacket): "When the same parameters and quantitative theory are used to analyze both termite colonies and troops of rhesus macaques, we will have a unified science of sociobiology" (p. 4). He carries out this apparently outrageous comparison and then comments, "This comparison may seem facile, but it is out of deliberate oversimplification that the beginnings of a general theory are made" (p. 5). When he later compares the behavior of three species of jays, his methods would be allowed by any zoologist, but when he compares herds of dolphins and ungulates (p. 475), or dinosaurs and elephants as large animals of the plains (p. 446), or castes in ants and vervet monkeys (p. 299), he is making comparisons that some comparative psychologists would consider unreasonable, because the species are so widely separated, and because he is looking more specifically than they would at just a few traits.

Classification is a further step in the stripping away of narrative. If the first thing noticed by the general reader leafing through *Sociobiology* is the pictures, the next thing will be the massive and daunting tables. These tables are not just lists; each supports an argument made in the text, for instance on density-dependent controls (pp. 88–89), chemical communication systems (p. 331), territorial behavior (p. 263), or dominance (p. 292). Each table brings together a number of narratives. For instance, Table 12-1, on "Examples of territorial behavior in which the primary function has been reasonably well established" (pp. 263–64), draws on narratives of the animals encountering other animals, of each animal's life history, of the observer recording these encounters, and of the sociobiologist correlating behavior with functions. The events of these narratives are left out when they are presented in a table, and only return when the information is questioned. When John Krebs criticizes the selection of articles to support Table 12-1, commenting that some of the studies are more reliable than others, he makes the reader reconstruct how each of the functions that were "reasonably well established" were actually established. In the case of another table, when Beach says that infanticide among langurs may be "simply an infrequent, aberrant, and extrane-

9. Beach, "Sociobiology and Interspecific Comparisons," pp. 116–35.

ously induced event" he raises questions about observation and about life histories, calling up all the particulars about who saw what, where, that Table 15-2 (p. 322) cuts out when it lists "infanticide of loser's offspring and insemination by the winning suitor" as one form of sexual selection.[10]

The goal of such classification is not just order; classification is supposed to lead to rules. The tables in the last third of the book attempt to find regular patterns in each order of social animal in the distribution of social behavior with respect to ecology. Sometimes this effort is successful, as in a table based on Jarman's work on ungulates that shows that the social systems "can be transformed with minor distortion into a single axis or sociocline" (p. 479). But for the mammals in general, he finds, "It is difficult, if not impossible, to put this information into one grand evolutionary scheme" (p. 456). For primates he is particularly cautious; he presents two different tables (based on those of Crook and Gartlan and of Eisenberg et al.), arguing that despite what he sees as logical flaws in their arrangements, they have value as analytical tools (see my Epilogue). Ultimately, Wilson wants to make behavioral biology as systematic and quantitative as physics or molecular biology—that is, he wants to remove entirely the narrative elements of particular places, times, and actors. His own ergonomic models of castes (p. 307) are an example of how the natural world can be explained in nonnarrative terms. The various activities of the ants—foraging, fighting, building a nest, laying eggs, caring for the young—become ratios of the weights of castes. Even the individual actors disappear, to be represented by their collective masses. Such models raise one of the key problems for evolutionary narratives, the problem of defining the actor that is the subject of evolution.

The Narratives of Adaptation

If such graphs were the only product of sociobiology, the book might anger some biologists with its methods and criticism, but it would hardly cause a stir outside the discipline. The public controversies concern the larger narratives these tables and graphs serve. Agassiz's synoptic room in his museum would show the glory of God as the creator; Wilson's tables and graphs point toward an equally grand, if rather different narrative, the Darwinian narrative of adaptation. This narrative requires the creation of a subject, and of a species-centered

10. Krebs, "Multiple Review"; Beach, "Sociobiology and Interspecific Comparisons," p. 119.

narrative that holds each of the "Just-So Stories" together, and of a grand evolutionary narrative that structures the whole book, a Great Chain of Behaving. My argument is that *Sociobiology* fills out these very abstract narrative structures with the actors of natural history.

One of the controversial issues in *Sociobiology* is just what the subject of this narrative is. Some critics have accused Wilson of supporting an economy based on competition by emphasizing the individual. But his book is part of a line of thought that seems to eliminate the individual, taking the population as the crucial actor and seeing this population in terms of gene frequencies. In Wilson's definition, "Natural selection is the process whereby certain genes gain representation in the following generations superior to that of other genes located at the same chromosome positions" (p. 3). Wilson's discussions of inclusive fitness, altruism, and group selection have been frequently analyzed elsewhere. What is important to my analysis is the way this construction of a subject both undoes and uses the natural history narrative, with its focus on the individual animal as a character. Wilson quotes Samuel Butler's aphorism, so often cited in biology in the last fifteen years, that "the chicken is only an egg's way of making another egg." The wittiness, the paradox, of this aphorism is in the way it juxtaposes the more familiar narratives in which creatures must be the subjects with the scientific narrative that has genes and populations doing things.

Looking at this view in textual terms, and leaving aside the methodological and philosophical difficulties with such a gene-centered analysis that are emphasized in critiques of sociobiology by geneticists, we can see several possible solutions to the problem of constructing a narrative that apparently lacks a subject. Richard Dawkins worked out a way of telling the story with genes as anthropomorphized characters in *The Selfish Gene*. Another way of telling the story is found in the game-theory work of John Maynard Smith, Geoffrey Parker, and others, who model the organism as if it were a rational strategist. Darwin found a subject by metaphorizing Nature as a careful breeder, in his comparisons of artificial selection with natural selection. Wilson's solution is similar to that of Darwin, but it is perhaps characteristic of the differences between Victorian English culture and American culture today that Wilson compares nature, not to a gentleman farmer, but to an engineer. In discussing animal communication, he says, "If the theory of natural selection is really correct, an evolving species can be metaphorized as a communications engineer who tries to assemble as perfect a transmission device as the materials at hand permit" (p. 240). Later he refers to the "engineering rules" for the evolution of pheromones (p. 370). This language puts Wilson in a long line of writers who treat animals as automata. At the lowest level of organismic response the organism "is like a cheaply-constructed servomechanism" (p. 151). But it must be admitted that he can imagine some very complex automata; he explains the responses of organisms to environmental change in terms of "an immensely complicated multiple tracking system" (p. 145).[11]

This metaphor is worked out for humans in one of the most controversial passages of reductivism, at the end of the book (p. 575).

> The transition from purely phenomenological to fundamental theory in sociology must await a full, neuronal explanation of the human brain. Only when the machinery can be torn down on paper at the level of the cell and put together again will the properties of emotion and ethical judgment become clear. Simulations can then be employed to estimate the full range of behavioral responses and the precision of their homeostatic controls. Stress will be evaluated in terms of neuronal perturbations and their relaxation times. Cognition will be translated into circuitry.

Even Wilson agrees with criticism of this prediction for the future of sociology, but he still argues that the study of human societies will eventually have to go as far as "systems analyses of neuronal populations."[12] When some critics lump Wilson with various behavioral psychologists who actually have quite different views of the causes of behavior, it is perhaps because he shares with them this metaphor of social engineering.

Just-So Stories

Though the metaphor of the engineer will serve to structure opening and closing passages, reminding us of a larger project, it still leaves the problem, in almost every paragraph, of how to tell a story without

11. Robin Dunbar (pers. comm.) emphasizes here that the genes are not themselves actors in this narrative. "The genes are the *currency* of exchange, the individuals are the actors, though it is generally only as *populations* of individuals that Wilson sees them as interesting." He says the difference between Wilson and some other biologists in this emphasis is "due to his being an ant person rather than a mammal person." For an analysis of the "actors" and the "currency" of some accounts of the evolution of society, see Latour and Strum, "Human Social Origins." Ullica Segerstrale comments on the differences between Wilson and some British sociobiologists over such issues as group selection.

12. Wilson, "Multiple Review," p. 717.

a subject. For instance, the section on "The ecology of parental care" begins with an extraordinarily complex and abstract set of narrative chains, of which this is just one (p. 337):

> Expressed in the language of population biology, [the theory] postulates a web of causation leading from a limited set of primary environmental adaptations through alterations in the demographic parameters to the evolution of parental care as a set of enabling devices. The reader can gain the essential idea by studying Figure 16-2. The proposition states that when species adapt to stable, predictable environments, *K* selection tends to prevail over *r* selection, with the following series of demographic consequences that favor the evolution of parental care: the animal will tend to live longer, grow larger, and reproduce at intervals instead of all at once (iteroparity). Further, if the habitat is structured, say, a coral reef as opposed to the open sea, the animal will tend to occupy a home range or territory, or at least return to particular places for feeding and refuge (philopatry). Each of these modifications is best served by the production of a relatively small number of offspring whose survivorship is improved by special attention during their early development.

Note that the "actors" here (that is, the subjects of the action) are *K* selection, demographic consequences, and the production of a small number of offspring. "The animal" mentioned in the middle of the passage is just a counter in the demographics. This bit of narrative is one of the four represented in Figure 16-2 (p. 338), which shows the basic form of the adaptive narrative in four arrows converging from the corners to "Parental Care." Each arrow represents a narrative that begins with an environmental factor, moves through various demographic consequences, and arrives at a change in behavior. The definition that Wilson gives at the beginning of the book describes this same narrative, only in reverse order: "Social evolution is the outcome of the genetic response of populations to ecological pressure within the constraints imposed by phylogenetic criteria" (p. 32).

The empty slot in this abstract narrative of adaptation is filled by bits of natural history. If the reader looks up from the bewilderingly abstract narratives on parental care, like the one I have quoted, he or she sees, on the same page, a very striking picture of a scorpion carrying her young, tiny white miniatures, on her tail. Those who persist past the "language of population biology" to the end of the section come to a charming natural history anecdote about lions teaching their young cubs to hunt (p. 341). These infusions of natural history make the narrative of adaptation come alive. Wilson is careful not to make the animal the conscious subject of evolution—which would make the whole story comic—but his text juxtaposes the two kinds of narrative, and the two scales of the time of evolution and the time of the development of the individual.

The narrative of adaptation has several variants. One can start, not with given environmental factors, as in the parental care stories, but with "phylogenetic inertia," as in the stories of the evolution of social parasitism in ants (p. 364), or of hymenoptera (p. 415), or fly mating (p. 227), or social primates (p. 516). At the level of abstraction of these evolutionary stories, it is sometimes unnecessary or impossible to tell what should come first. A number of the stories allow for multiple paths to the same end, for instance, in the explanations of the relation between environmental unpredictability and species distribution (p. 29), or the evolution of a solitary condition in previously social species (p. 36), or the evolution of sexual dimorphism (p. 334). One would not expect this explanatory flexibility and apparent open-mindedness after reading critics of Wilson's "Just So Stories." But the critics could point out that all the alternative narratives, however different in their implications, follow the same basic story of the adaptiveness of social behavior; in that sense, all the alternatives considered are sociobiological, and explanations from rival disciplinary approaches cannot fit in.

The Great Chain of Behaving

The "Just So Stories" link members of a population syntagmatically from one stage of evolution to the next in almost every section or paragraph of the book. The larger structure of the book, including the order of many of its paragraphs, is given by another narrative of adaptation that links species paradigmatically in a hierarchy. For instance, Wilson ends the important chapter on altruism by asserting that, on the basis of the evidence he has given, "a single strong thread does indeed run from the conduct of termite colonies and turkey brotherhoods to man" (p. 129). It is hard for him, in describing this thread, not to treat the more social species as somehow better, so he uses words like "pinnacles," "haut monde," and "most advanced." But the irony of his grand narrative is that as social evolution is progressing, it is also declining, so that in his terms the most social of all animals are the most primitive (p. 379), the colonial invertebrates. And, not surprisingly, this entomologist finds the ants more social than any mammals. This sort of reasoning parallels that which has traditionally led social philosophers to praise the selflessness of the social insects.

Such a chain of behavior, following the taxonomic chart fom colonial invertebrates up to man, organizes the chapters in the third part of the book. Similarly, many of the sections within chapters in the second part are organized along taxonomic lines, always with the lower orders first and the primates last, for instance in the surveys of play (pp. 165–66), of traditions (pp. 168–72), or of ritualization (pp. 226–28). Of course the chain of behaving is not phylogenetic, and does not imply relations of homology, with humans inheriting a tendency to build cities from the corals. But there is a powerful narrative thrust to these surveys that leads some readers to see Wilson as presenting insect social behavior as the ancestor of human social behavior, instead of as presenting the two kinds of behavior as parallel responses to different environmental challenges.

The controversy about this chain of behaving concerns its end point, with humans. Defenders of Wilson, and sometimes Wilson himself, remind us that man is central in only one chapter of twenty-seven—the last. But links to human behavior are drawn throughout the book, sometimes playfully, often very cautiously, but enough to keep the direction of the narrative clear.[13] A chain that moves up the taxonomic system can be seen structuring one of the most controversial paragraphs of the book, one which asserts that xenophobia can be found in geese, chickens, monkeys, and man.

> The relative calm of a stable dominance hierarchy conceals a potentially violent united front against strangers. The newcomer is a threat to the status of every animal in the group, and he is treated accordingly. Cooperative behavior reaches a peak among the insiders when repelling such an intruder. The sight of an alien bird, for example, energizes a flock of Canada geese, evoking the full panoply of threat displays accompanied by repeated mass approaches and retreats (Klopman, 1968). Chicken farmers are well aware of the

13. The links to man throughout the text include, for example, these passages:
- The defensive array of ungulates is paralleled to Clausewitz's rules of war (p. 45).
- Incest avoidance is linked to the inability of former students to become their teachers' colleagues on equal terms (p. 79).
- Mennonite communites provide an example of the lower limit of herd size (p. 135).
- The waggle dance of bees in compared to Wilson's communication with the reader (p. 177).
- A Harvard commencement is compared to ritualization in birds (p. 224).
- Hormone changes in aggression are illustrated with an example from hockey (p. 253).
- Human occupations are compared to animal roles (p. 313).
- Monkey alloparenting is compared to babysitting (p. 350).

> practical implications of xenophobia. A new bird introduced into an organized flock will, unless it is unusually vigorous, suffer attacks for days on end while being forced down to the lowest status. In many cases it will simply expire with little show of resistance. Southwick's experiment (1969), cited in Chapter 11, demonstrated that the appearance of a newcomer is the single most effective means of increasing aggressive behavior in a troop of rhesus monkeys, most of the hostility being directed against the stranger. Human behavior provides some of the best exemplification of the xenophobia principle. Outsiders are almost always a source of tension. If they pose a physical threat, especially to territorial integrity, they loom in our vision as an evil, monolithic force. Efforts are made to reduce them to subhuman status, so that they can be treated without conscience. They are the gooks, the wogs, the krauts, the commies—not like us, another species surely, a force remorselesly dedicated to our destruction, who must be met with equal ruthlessness if we are to survive. Even the gentle bushmen distinguish themselves as the !Kung—the human beings. At this level of "gut feeling," the mental processes of a human being and of a rhesus monkey may well be neurophysiologically homologous.

Parts of this passage are quoted both by Wilson's critics (Alper) and his popularizers (Silcock).[14] The narrative works in two ways. Here Wilson does explicitly say that the rhesus and human behaviors may be homologous, but he is also tracing an analogy (not a homology) from birds to monkeys to man. There is a reference to an experiment, but as with the abstract narrative of adaptation, the story has to be filled in with natural history—the traditional observations of farmers, or the supposed experience of the readers. This paragraph shows the rhetorical shift commented on by so many critics of the last chapter—the slots that in earlier chapters were filled in with observations of animals are filled in chapter 27 with references to common knowledge.

The Territory of the Sociobiologist

The narrative that caused the most antagonistic responses to Wilson's book—the narrative of the future growth of sociobiology itself—seems at first glance not to be related to the narrative of adaptation. This story is told in a way that seems counterproductive. One expects that a

14. Alper, "Ethical and Social Implications," p. 209; Silcock, "How Genetic Is Human Behavior," p. 17.

specialist who wants members of other disciplines to apply the principles of his own discipline will avoid threatening the potential readers he or she seeks to persuade. Niko Tinbergen provides an example of unthreatening rhetoric in a 1968 *Science* article that makes many of the same points Wilson makes in his opening and closing chapters, but apparently does so without arousing antagonism. "As an ethologist," Tinbergen says, "I am going to suggest how my science could assist its sister sciences in their attempts, already well on their way, to make a united, broad fronted, truly biological attack on the problems of behavior."[15] Wilson, on the other hand, uses very aggressive language in dealing with the disciplines nearest sociobiology, whose members he presumably wants to bring over to his vocabulary and methods. A sentence much quoted by his critics says that both ethology and comparative psychology "are destined to be cannibalized by neurophysiology and sensory physiology from one end and sociobiology and behavioral ecology from the other (see Figure 1-2)" (p. 6). Wilson has found the one word most likely to antagonize his readers, and he repeats it at the end of his book when he speaks of neurophysiology *cannibalizing* psychology. Not surprisingly, a number of ethologists and psychologists who reviewed the book sieze on this, the most quoted word in the book, when they criticize what they see as Wilson's misunderstanding of their disciplines.[16]

Whether this aggressive strategy is simply a mistake, or whether it relates to the complex hierarchy of disciplines Wilson lays out in one of his later articles ("Biology and the Social Sciences"), it is consistent with the rest of the book in its emphasis on territory. Several reviewers, sympathetic or unsympathetic, parallel Wilson's view of the sciences to his view of animal competition, referring to the "territoriality" shown in the controversy (not just by Wilson), or to Wilson's assumptions about the "natural selection of academic disciplines."[17] The parallel suggests that the terrain of science is fixed, and resources

15. Tinbergen, "On War and Peace"; Mulkay, Ashmore, and Pinch describe such a project in their unpublished paper on the rhetoric of health economists, "Colonising the Mind" (1986).

16. See, for instance, the comments of the psychologist Frank Beach and the ethologist R. A. Hinde.

17. The phrase is from a review by A. Hunter Dupree. George Barlow ("Multiple Review," p. 701) uses another biological metaphor when he says, "There is an ecology of scientific activity. Valid major ideas, like top-level carnivores, are few. Scientific findings are like primary producers: while numerous and often short-lived, they drive the system." See my paper, "Every Picture Tells a Story," for a comparison of Wilson's diagram of scientific disciplines to a pair of maps of blackbirds' territories.

can be gotten only by defeating a neighboring discipline and usurping its space. A generally favorable reviewer, the ethologist R. A. Hinde, scolds Wilson for his aggressiveness and proposes a less agonistic view of the scientific world: "Whilst Wilson's enthusiasm is infectious, he must not forget that other people are interested in other things."[18] The tone of the reviews suggests that Wilson's disciplinary imperialism had as much to do with the reception of the book as did any of its claims or implications.

Interpretation and Irony in Reviews of Sociobiology

I have argued that what makes *Sociobiology* persuasive is not the facts, not the arguments, but the narrative. I should like to look at some of the many reviews of the book and responses to it as part of a process of interpretation and ironic reinterpretation of this narrative, much as I looked at the published responses to Crews and Fitzgerald's article in chapter 4. There have been a number of different kinds of criticisms, but I shall draw most of my examples from various articles by members of the Sociobiology Study Group (SSG) of Science for the People, which present a case against what they call Wilson's "biological determinism," from Wilson's responses to this group, and from the ensuing exchanges. Other critics—including comparative psychologists, geneticists, anthropologists, and philosophers of science—may not be making the same arguments, but their texts often take similar forms. That is, they do not just disagree with Wilson's arguments, but represent his text so as to weaken his own claims and support theirs. Wilson, in response, uses the same sort of textual devices.

There is a large and still-growing literature on *Sociobiology*, but when one reads the articles one finds many of them are remarkably alike. W. R. Albury comments on this, but is surprised only by the similarity of the various responses to the SSG in defense of *Sociobiology*.

> We have found a high degree of coherence among those responding to the SSG's critique, with regard both to tactics (reversal and reduction) and to strategy (systematic exclusion of politics). It is, of course, possible that this coherence is an artefact of the particular sample of writings studied; but even if this should prove to be so, it

18. Hinde, "Multiple Review," p. 707.

> is still significant that such a diverse group of authors should exhibit a unity of this kind.[19]

My contention is that both sides use the same tactics and strategies. Even some Marxist critics practise the "systematic exclusion of politics" from an analysis of the construction of their *own* position. And the processes Albury analyzes as reversal and reduction are, in my terms, part of the reconstruction both of Wilson's and the SSG's texts. I see the process of reconstituting and interpreting Wilson's text in the way reviewers quote him (and he quotes them), the way they place the text in a genealogy (and the way he responds), and in the ways the two sides define the arguments and account for the existence of a controversy.

The focus on texts is itself an indication of a controversy. I have argued in chapter 4 that, although scientific texts are usually treated as transparent, so transparent that they can be summarized sufficiently with a claim and a citation, a controversy makes them opaque, focusing attention on the participants' words and textual strategies. Because, in realist discourse, two incommensurate views of reality cannot both be right, the problem for a realist must be in the formulation or presentation of these views. When the attention is focused on the presentation, it is annoying to those observers who want to find the purely scientific issues in the controversy; Nicholas Wade, writing a news article on the controversy in *Science* complains, "The chief bone of contention . . . thus dissolves into an arid analysis of Wilson's text."[20] I do not find such analysis arid, because I am arguing that the chief bone of contention *is* Wilson's text.

Quoting out of Context

Any review is a reconstruction of the text reviewed. When a review uses quotations, it offers them the way natural history narratives offer facts, as bits of the world that speak for themselves. But the purpose is seldom just to say what those words say; the mere fact of quotation indicates that the writer thinks these words are particularly apt or, more often, particularly and obviously vulnerable.

Wilson charges that he was quoted out of context in some reviews of *Sociobiology*, and his charge is supported by journalists who reported on the controversy for *Science* (Nicholas Wade) and for *New*

19. W. R. Albury, "Politics and Rhetoric in the Sociobiology Debate," *Social Studies of Science* 10 (1980): 532.

20. Wade, "Sociobiology," p. 327.

Scientist (Roger Lewin). But we should remember that all quotation, textually speaking, is out of context. When Wilson quotes a bit of natural history, he omits anything leading up to it and anything after it, perhaps omitting the original author's reasons for making the observation and interpretation of it. Wilson puts it in the context of his own argument, perhaps comparing this behavior, in a way the original author might not accept, to that of another animal in another text. Similarly, when I have quoted Wilson in this chapter, I have not tried to put the quoted passages in the order he put them, and I have often used only a phrase or a sentence from a much longer passage. More important, I have made his words say what I want to say—about the construction of narrative—rather than what he meant to say—about the evolution of social behavior. Other commentators make their own selections for their own reasons. When I read some of the criticisms, especially those of the SSG, I have the sense I am reading a sort of anthology of Wilson's work, for a number of different authors in a number of different articles draw on almost exactly the same quotations, and these quotations all come from just a few sections of the book, especially the first and last chapters and the chapter on altruism. But Wilson himself does such rearranging in his own summary of the book; a quotation he gives when he defends himself is transformed by his highlighting of it, and by his addition of italics to emphasize the point he now wants to make with these words.[21]

Both Wade and Lewin have compared a number of passages quoted in reviews to fuller original versions. I want to look in more detail at one such case to see just what it means when they say Wilson was quoted out of context. After C. H. Waddington's review of *Sociobiology* in the *New York Review of Books*, the *NYRB* published a letter from the SSG, a group consisting mostly of academics from the Boston area, and including some well-known colleagues of Wilson's. The signers disagreed with what they read as a favorable review, and criticized the book for promoting "biological determinism." One full paragraph from the letter reads:

> Another of Wilson's strategies involves a leap of faith from what might be to "what is." For example, as Wilson attempts to shift his arguments smoothly from nonhuman to human behavior, he encounters a factor which differentiates the two: cultural transmission. Of course, Wilson is not unaware of the problem. He presents (p. 550) Dobzhansky's "extreme orthodox view of environmentalism":

21. Wilson, "Multiple Review," p. 698.

> Culture is not inherited through genes; it is acquired by learning from other human beings. . . . In a sense human genes have surrendered their primacy in human evolution to an entirely new non-biological or superorganic agent, culture.
>
> But he ends the paragraph by saying "the very opposite could be true." And suddenly, in the next sentence, the opposite does become true as Wilson calls for "the necessity of anthropological genetics." In other words, we must study the process by which culture is inherited through genes. Thus, it is Wilson's own preference for genetic explanations which is used to persuade the reader to make this jump.[22]

The paragraph uses three bits from Wilson's text (the whole passage from *Sociobiology* is quoted in Appendix 5). The writers are quoting Wilson quoting Dobzhansky, and presumably they quote Wilson's characterization of Dobzhansky's position as the "extreme orthodox view" to show that Wilson is not in his camp. They link his comment that "the opposite could be true" to the quotation by noting that it is at the end of the same paragraph. They present his next sentence as a leap, and show what a leap it is by offering an interpretation of what Wilson means "in other words." The page number they give before their quotation reminds us that these words are in the book for anyone to check; such conventions reinforce the reader's sense that a quotation is a fragment of the original that speaks for itself.

Wilson responds to this charge in another letter to the *New York Review of Books:*

> Allen *et al.* try to make me appear to be the arch hereditarian by quoting my sentence "The very opposite could be true" after a quotation from Dobzhansky stating that "In a sense human genes have surrendered their primacy in human evolution to an entirely new non-biological or superorganic agent, culture." In fact, my sentence came fourteen lines of mostly technical information after the Dobzhansky quotation, and it really followed the sentence "It is not valid to point to the absence of a behavioral trait in one or a few societies as conclusive evidence that the trait is environmentally induced and has no genetic disposition in man." My meaning, which refers to a lesser technical point, was thus grossly distorted by this elision. A reading of the full paragraph will show that I am

22. Allen et al., "Against Sociobiology," p. 262.

> far closer to Dobzhansky in my overall view than to the opposite position which seems to be indicated by the mutilated version.[23]

If we look at the paragraph from Wilson quoted in the SSG's letter, we see that his last sentence does comment on a methodological statement, rather than on the statement from Dobzhansky, and that earlier he does in fact imply he is close to Dobzhansky "in his overall view." But he introduces this statement with a subordinate clause, "Although the genes have given away most of their sovereignty," that leads us to expect less than complete agreement with Dobzhansky. And a qualification follows: "they [the genes] maintain a certain amount of influence." He puts the Dobzhansky quotation in his own context, when he makes an apparently parallel statement right after it, so that Dobzhansky's admission that culture is dependent on the human genotype is expanded into the very different statement that genes may influence "the behavioral qualities that underlie variations between cultures." And what Wilson characterizes as "mostly technical information" might be seen as highly controversial support for a position different from the environmentalism of the Dobzhansky quotation, not as data too technical to be considered in the controversy. Also, as the SSG suggests in their ironic quotation of Wilson, his phrase that introduces the quotation, by referring to "the *extreme orthodox* view of environmentalism," implies in the context of scientific rhetoric that such a fixed position should be challenged. If we look at the whole paragraph, we see that while it is indeed supporting some form of environmentalism, in almost every sentence it implicitly questions this position.

If we take just a slightly larger context that the paragraph, looking at the three sentences before it, we find an even more complex interpretation to Wilson's statement that "the very opposite could be true." In these sentences Wilson explicitly states that we should not expect much variation between groups in the human genotype. And he cites, in support of this, a study by Richard Lewontin, his colleague at Harvard, one of his fiercest critics, and one of the authors of the SSG letter. Though Lewontin's work concerned genes specifying blood chemistry, Wilson does not try to argue that the genes he is interested in, those specifying behavior, would be any different. But the apparently gracious last sentence of the paragraph might also be read as a provocation, for it assimilates Lewontin's research to Wilson's argument about social behavior, whereas Lewontin might argue

23. Wilson, "For Sociobiology," p. 266.

that his work has nothing to do with behavior one way or the other, because social behavior is not genetically controlled. His claim, like Dobzhansky's, is supported but is given a quite different interpretation from what he might have intended.[24]

If we look back even further, to the beginning of the paragraph before that quoted by the SSG, we see that Wilson supports his apparent agreement with the environmentalists with some examples from history. Wilson sometimes refers to this passage, in responses to criticism, to show he has always acknowledged that short-term historical change cannot be accounted for genetically. But again the context in which he gives these statements affects our interpretation of them, for he introduces the paragraph by calling this "conventional wisdom." In the rhetoric of scientific texts, such a phrase usually signals an intention to disagree, as the phrase "orthodox view" does in the next paragraph. We would expect Wilson to go on to analyze the deficiencies of the "conventional wisdom."

My purpose here is not to decide if Wilson was or was not unfairly quoted, but to suggest that there is no context large enough to guarantee that a statement will have just one meaning, the intended meaning, that it will speak for itself. In other arguments in the course of the controversy, one side or the other says that the relevant context is to be found in passages in other, less controversial chapters of the book, or in other publications by Wilson, or in earlier publications to which he is responding, or in textbooks encapsulating the assumptions of the discipline, or in comparisons to other contemporary controversies, such as that over research on race and I.Q., or in past controversies such as those over Social Darwinism or immigration, or in the disciplinary history of comparative psychology or ethology. Different kinds of contexts are invoked at different points in the controversy in arguments over the interpretation of Wilson's statements on such issues as territory, sexism, the relations of the social sciences to biology, the relations of modern societies to early human or primate societies, or Wilson's use of anthropomorphic terminology.

For instance, Lawrence Miller argues that the explicit statement by Wilson that sexism is a bad thing should be read in what is presum-

24. Interestingly, nearly every major critic of *Sociobiology* who has done related work, including Lewontin, Levins, Washburn, Beach, and Rosenblatt, is cited prominently and favorably in it. Scientists often tell anecdotes about reviewers responding antagonistically to books that attack or ignore the reviewer's work. But perhaps, in this case, a reviewer is more likely to be antagonized by seeing his or her work appropriated as part of Wilson's argument. Segerstrale mentions this in her account of the relations between Wilson and Lewontin.

ably a larger context, the tendency of the whole book. "We agree with Wilson's *caveat:* sexism is not justifiable. But the thrust of Wilson's point, here and elsewhere, is that sexism is inevitable, even if undesirable, because it is genetically determined." Here the relevant context, the *thrust* of Wilson's text, is something separate from any quotable statement in it. Joseph Alper transforms Wilson's many statements on population "groups" into statements on "race" by noting that, although Wilson is careful not to use the word "race" and although "group" does not usually mean "race" in the discourse of evolutionary biology, another author, R. A. Goldsby, *has* given a definition of "race" such that one can say "race, as defined by Goldsby, is one type of Wilsonian group."[25] In this rather complicated process of translation, the context for determining the meaning of the author's statements is not in his text, or even in the disciplinary discourse of which it is a part, but is in another text that gives the real meaning of his terms, such that he can be seen to say something even if he deliberately does not say it.

Wilson's characteristic style may make it easier to pick out damning quotations than it is with some scientists; he tends to intersperse long stretches of cautious suggestion and qualification with a few brash overstatements. But we can see such selective quotations in many scientific controversies, and indeed we can see it on both sides of this controversy. Members of the Sociobiology Study Group make the same sort of claims that they were misinterpreted that Wilson makes about their reading of his text. Responding to an analysis of the controversy by Arthur Caplan, Lawrence Miller says, "the article represents a systematic misunderstanding of our critique of sociobiology." Responding to the *Science* article, Joseph Alper and his colleagues say "Wade distorts and, in effect, trivializes the whole matter." Steven Rose says, after the article in *New Scientist,* "Roger Lewin's account of the sociobiology controversy . . . suffers, it seems to me, from just those vices he accuses E. O. Wilson's critics of adopting—selective quotation to distort the nature of the charges being made."[26] My point is that Lewin does use selective quotation and paraphrase to put statements in a context other than that intended, but so does Rose in response to him, and so does another letter in response to Rose's

25. Miller, "Philosophy, Dichotomies, and Sociobiology," p. 322; Alper, "Ethical and Social Implications," p. 208.

26. Miller, "Philosophy, Dichotomies, and Sociobiology," p. 319; Alper et al., "The Implications of Sociobiology" (see also Alper, "Ethical and Social Implications," p. 206); Rose, "Sociobiology," p. 433.

letter (Hammerton), and so does Rose again in the response to Hammerton's letter ("Sociopolitics"). And so do I, in presenting this sequence. All participants in the controversy assume that there is a self-evident meaning to their words, and that the reader who wants to find what Wilson calls "my true meaning" or what Rose calls "what Wilson actually does say" need only go to the trouble of looking up the original text. I am arguing that the various contexts introduced by the participants themselves make it impossible to settle on one meaning on the basis of the text alone. A discourse community can settle on one interpretation of a text, and I think both sociobiologists on the one hand, and critics of sociobiology on the other have reached consensus about how to read *Sociobiology.* But such agreement comes at the end of a controversy, through the controversy, not at the beginning. The assertion that texts have perfectly straightforward, context-free meanings that can be found by application of rational rules is just another rhetorical tool, and it is a tool equally available to both sides.

In all these cases, the charge of misinterpretation is used to rebut criticism. References to misinterpretation can also serve as a polite cover for disagreement, one that does not directly challenge the competence of the scientist criticized. For instance, reviewers who essentially agree with Wilson focus on issues of interpretation when they want to point out some issue on which they differ from him. The explanation of the difference cannot be in nature, because nature can have only one correct interpretation, and it cannot be due to an error on Wilson's part, or one's own part, so it must be due to misreading. This is especially true in the generally favorable comments by other behavioral biologists in a collective review in *Animal Behaviour.* G. P. Baerends, after praising *Sociobiology,* accounts for his differences with Wilson as errors in Wilson's reading: "I find Wilson's discussion of Tinbergen's conflict hypothesis . . . based on insufficient and biased use of the available evidence." In his sympathetic review John Krebs comments, "I feel that in some places Wilson is not discriminating enough in his literature reviews. In summary tables such as 12-1 . . . he makes no distinction between short-term observational studies published in popular magazines and painstaking experimental work done over several years."

Jerry Hirsch's attack in this same collective review, though remarkably vicious and personal, uses the same device used by those who generally agree with Wilson, to end his comment on a note of apparent concern and respect for Wilson's abilities: "This has been done in the hopes of encouraging Wilson to read more carefully, to prune his bibliography of trendy sources (*Atlantic Monthly, Time, Scientific Ameri-*

can, etc.; and also unreliable claims in the primary journals)."[27] It is clear from Hirsch's comments that he and Wilson differ on a number of fundamental methodological and social issues, but for the purposes of a disinterested conclusion, these can all be reduced to the advice to "read more carefully." For these purposes, Hirsch takes the reading of the primary journals, and the weeding out of "unreliable claims," to be unproblematic. But in their exchange in *Animal Behaviour*, Wilson and Hirsch disagree about just what is claimed in an article by Dobzhansky. Hirsch reprints apparently inconclusive figures to show that the article can offer no support for Wilson's claim of experimentally demonstrated rapid speciation, whereas Wilson says, "the unevenness in the progress of selection stressed by Hirsch was explained by Dobzhansky and Pavlovsky as largely an artifact having no bearing on the main result." That they can differ so completely shows again how the same quoted words, and even the same figures, can have radically different meanings when placed in different contexts.

Tracing the Genealogy

One's interpretation of the book depends not only on the context in which one reads passages word by word, but also on the context in which one puts the book as a whole, and the selection of this context is another interpretive decision in which one can ironically invert the apparent narrative. We saw in chapter 4 that participants in that controversy within the core set gave sharply contrasting introductory reviews of the literature. Almost every reviewer of *Sociobiology* gives it a genealogy, a set of texts against which it is to be read. Two sorts of genealogies figure frequently in the controversy, and both genealogies are open to reinterpretation by critics or defenders of the book.[28]

J. R. Krebs's brief review opens with a passage that can be taken as typical of those in reviews by other ethologists. He locates the book in disciplinary terms (Krebs is an ethologist), as part of a personal tradition of major figures, in terms of national traditions (Krebs is British), and in contrast to earlier popularized treatments of sociobiological concepts.

> A biochemist recently asked me to define sociobiology. The only simple answer to the question was "The branch of biology covered

27. Baerends, "Multiple Review," p. 700; Krebs, "Multiple Review," p. 709; Hirsch, "Multiple Review," p. 709.

28. I discuss cartoons and visual suggestions of a genealogy in "Every Picture Tells a Story."

> by E. O. Wilson's book." There is no other definitive work on this eclectic field which combines behavioral ecology, population biology, and evolutionary theory. Although many of the earlier key developments in sociobiology were due to European workers such as Crook, W. D. Hamilton, Maynard Smith, and the followers of Lack and Tinbergen, the mainstream of European ethology sadly failed to follow up the lead, so that most of the more exciting recent developments have come from North America. R. A. Hinde's seminal work, *Animal Behaviour,* makes no mention of Hamilton and Maynard Smith, and only brief reference to the work of Lack and Crook. It goes without saying that Wilson's book is outstanding, both as an encyclopedic review of the literature and as a lucid, critical synthesis of theoretical concepts.

Krebs credits Wilson with defining a field, created by combining three separate fields in the "synthesis" referred to in Wilson's title, but it is a field that already has some distinguished members who worked under other disciplinary titles. He puts Wilson in a tradition with several British researchers, some of whose names appear at the start of most favorable reviews, and compares the book favorably with that of the best known British ethologist. When he says the quality of the book "goes without saying," he acknowledges, as do most of the favorable reviews, Wilson's considerable reputation from his earlier books. The last sentence of Krebs's review also helps place Wilson, by contrasting his work with that of the authors of earlier sociobiological bestsellers: "Those who accepted uncritically the views of the evolution of social behavior popularized by Lorenz, Ardrey, and Tiger should study E. O. Wilson to learn the proper version of the story."[29]

In the opening of the NYRB letter criticizing the book, the SSG give it a different genealogy, placing it in terms of past figures who held views that are said to be analogous, and in terms of its possible future social effects.

> Beginning with Darwin's theories of natural selection 125 years ago, new biological and genetic information has played a significant role in the development of social and political policy. From Herbert Spencer, who coined the phrase "survival of the fittest," to Konrad Lorenz, Robert Ardrey, and now E. O. Wilson, we have seen proclaimed the primacy of natural selection in determining the most important characteristics of human behavior. These theo-

29. Krebs, "Multiple Review," pp. 709, 710.

> ries have resulted in a deterministic view of human societies and human action. Another form of this "biological determinism" appears in the claim that genetic theory and data can explain the origin of certain social problems, e.g., the suggestion by eugenicists such as Davenport in the early twentieth century that a host of examples of "deviant" behavior—criminality, alcoholism, etc.—are genetically based; or the more recent claims for a genetic basis of racial differences in intelligence by Arthur Jensen, William Shockley, and others.[30]

Here Wilson figures, not as the first synthesizer of several scientific fields, but as the latest in a line of scientists who applied biology to social policy. The theories of these scientists have apparently led people to have a "deterministic view," and their theories are related to, but are apparently not the same thing as, those of another and more virulent line that leads to the two best known academic proponents of racial differences in intelligence. After a paragraph accounting for the persistence of these wrong ideas, the writers show how dangerous they are by noting the use of Social Darwinism by J. D. Rockerfeller, Sr., to justify his practices, and by tracing American racism and German Fascism to eugenics: "These theories provided an important basis for the enactment of sterilization laws and restrictive immigration laws by the United States between 1910 and 1930 and also for the eugenics policies which led to the establishment of gas chambers in Nazi Germany."

Although the political genealogy does not actually contradict the scientific genealogy, no reviewer places Wilson in both contexts. He must be either the product of scientific progress or the product of ideological reproduction. But it is possible to alter either the scientific or the political genealogy so that its significance is reversed. For instance, the familiar geographical terms of Krebs's scientific genealogy—the Europeans failing to follow up their lead and the Americans taking over—can be rearranged in terms less favorable to Wilson. The American anthropologist S. L. Washburn includes Wilson in the tradition of "European thinking," condemning it for "eugenics, racist theories" and other errors. But the the *NYRB* review by the British evolutionary biologist C. H. Waddington (the review that ostensibly inspired the SSG letter) criticizes Wilson by associating him with "certain algebraic theories about population growth recently developed by American authors" which "biologists on the other side of the Atlantic feel . . . are

30. Allen et al., "Against Sociobiology," pp. 259–60.

rather schematic, and never fully apply to the complicated situations that arise in the actual ecological situations of nature."[31] Since Wilson is an American who reads British and continental scientists, he can be placed intellectually on either side of the Atlantic, and his critics, British or American, prefer to put him on the other side.

The revision of the genealogy can also rearrange the line of scientific figures from which Wilson derives. So, for instance, a comparative psychologist like Ethel Tobach removes Wilson from the central scientific position Krebs gives him, relating him only to ethology and contrasting this tradition with the comparative psychology tradition of C. L. Morgan, T. C. Schneirla, and D. E. Lehrman.[32] Also, she introduces other figures who said what Wilson said before Wilson said it, so that Wilson's book is not new—the genealogy then becomes a priority account. Almost every figure in Krebs's genealogy is rescued from this association with Wilson by one or another reviewer who uses this model figure—whether it is Tinbergen, or Crook, or Maynard Smith, or Hamilton—to show how better scientists avoided Wilson's errors. Stephen Jay Gould elegantly opens an essay on sociobiology by praising Linnaeus. Although most critics associate Wilson with Konrad Lorenz to discredit Wilson (political critics often cite a racist essay Lorenz wrote in 1940), Mary Midgely uses Lorenz's *Behind the Mirror* as "the proper guidebook" that shows in contrast the weakness of Wilson's philosophical position ("Rival Fatalisms"). A key figure in these lists of names is J. B. S. Haldane; British sociobiologists claim him as a founder of mathematical evolutionary genetics, whereas Marxist scientists look back to him as a figure who could combine scientific work with political action.[33]

Wilson, of course, tries to disassociate himself from a political genealogy that leads back to J. D. Rockefeller and Hitler. He argues that sociobiology, by showing altruism to be adaptive, actually refutes

31. Washburn, "Animal Behavior and Social Anthropology," p. 63; Waddington, "Mindless Societies," p. 256.

32. Tobach, "Multiple Review." David Crews points out that Tobach and Wilson reenact the old debate between their mentors Schneirla and Wheeler.

33. See Gary Werskey, *The Visible College* (London: Allen Lane, 1978), on Haldane's politics, and Ronald Clark for a biography, *J.B.S.: The Life and Work of J.B.S. Haldane* (London: Hodder and Stoughton, 1968); it unfortunately skimps on scientific detail. Haldane is another remarkable popularizer whose style combines cautious science with brash pronouncements. One can imagine a selection of statements from his writings that could make him seem politically reactionary; in fact, one can imagine a selection that could make him seem to say practically anything. In the "Preface" to his collection of popular essays, *The Inequality of Man* (Harmondsworth: Penguin, 1937), he invents an amusing example of how he could be misinterpreted in the press.

Social Darwinism, with its emphasis on individual fitness. He specifically attacks the views of Shockley and of earlier eugenicists. He argues that the method of "strong inference" separates him from popularizers who use the "advocacy method." He seldom cites Lorenz except to attack his assumptions and models as primitive or simplistic, and he singles out Lorenz's popular and controversial book *On Aggression* for attack. Wilson rejects any genealogy that puts him in a line of social engineers; he often argues that to treat his descriptions of social behavior as prescriptions for social policy is to commit the "naturalistic fallacy," confusing what is with what ought to be.

But Wilson's most effective response to the political genealogy is to turn the accusations against him back against his accusers, to use their words ironically as they use his words. This tactic is analyzed well by Albury, but Albury does not note that the SSG uses the same sorts of ironic reversal. When Wilson quotes the reference in the *NYRB* letter to Nazi gas chambers, it is not, presumably, to give the charge wider circulation, but to show that his critics have gone over the top and proved themselves to be "political" rather than scientific. The review by Jerry Hirsch, to which I already referred, repeats the affiliation of Wilson with Shockley in this form: "He [Wilson] was once bothered enough about heritability . . . to send me his manuscript in advance and then, like William B. Shockley, to telephone long distance in an unsuccessful attempt to recruit my support." Wilson responds by using the tenuousness of this particular attempt at affiliation to undermine Hirsch's whole review: "To connect my name with that of William Shockley, a notorious professed racist, on the sole ground that we both talked about heritability on the telephone to Hirsch, is in my opinion a tactic that should be beneath the reviewer of a scholarly work. I can only interpret it as further evidence that in this particular review political criteria were used to judge science."[34]

This ironic turn is a potent response. Later criticisms respond to Wilson's attempts to separate himself from any political genealogy. Steven Rose rebuts the charge that Wilson is being persecuted like earlier scientific heroes: "Far from being much abused 'new Galileos,' as their advocates have claimed for them, the sociobiologists are mere Ptolemaic medieval schoolmen." Rose's response to Wilson's response is again ironic, echoing the text of his opponent—in this case, Wilson's supporters' protests of persecution—to give it a meaning exactly the opposite of its apparent meaning. The critics respond to Wilson's declarations of political liberalism, his denials of any attempt

34. Hirsch, "Multiple Review," p. 708; Wilson, "Multiple Review," p. 718.

to perpetuate the oppression of women or blacks, by insisting that they are tracing a line of ideas and effects, so that Wilson's own political statements are irrelevant to the effect of his ideas. The Sociobiology Study Group says, in one of its later articles, that "such determinism provides a direct justification for the status quo as 'natural,' although some determinists dissociate themselves from some of the consequences of their arguments." Still, the critics do search Wilson's writings for some explicit and incriminating political statement. One such statement is found by Rose, Lewontin, and Kamin in an interview in *Le Monde* in which Wilson, "identified himself with American neoconservative libertarianism."[35] That they would find this statement in such an out-of-the-way source, rather than in any of Wilson's many, many writings or his interviews in English, suggests both that Wilson avoids making any explicit political statement and that his critics, despite their larger critique of ideology that makes such self-declarations irrelevant, need him to make such statements. The critics have an easier time finding such statements in the less cautious prose of Richard Dawkins, and so they often link his book *The Selfish Gene* to *Sociobiology*, though the two texts are quite different, the two approaches have some important theoretical differences, and the authors do not refer to each other.

These genealogies change in the course of the controversy; Albury points out that the political genealogy was often softened for rhetorical reasons in the later versions. The supporters who offered the professional genealogy revise it somewhat in retrospect, and Wilson's later texts reinterpret his stance in *Sociobiology*. Those who criticize *Sociobiology* often attack it by using quotations from Wilson's later books, so that *Sociobiology* is reread in terms of *On Human Nature*. Rose, Lewontin, and Kamin say, "The development of the literature of sociobiology since 1975 . . . including Wilson's own *On Human Nature*, leave little doubt that the problem of human nature is at the center of sociobiological concerns." This assumes that the "literature of sociobiology" is whatever Wilson writes; if one looked for this literature in the writings of Maynard Smith, Parker, and other British sociobiologists, one might well doubt that "human nature" was the central problem. The critics of *Sociobiology* might want to criticize this work too; my point is that they do not need to deal with the huge literature of the field in the last ten years as long as they can define "the literature" as being synonymous with Wilson's later writings and

35. Rose, "It's Only Human Nature"; SSG, "Sociobiology," p. 280; Rose et al., *Not in Our Genes*, p. 264.

with the more extravagant sociological and anthropological claims based on them.[36]

Retrospectives by some of these British researchers, written after the controversy died down somewhat, have made a different sort of revision of Wilson's genealogy. Geoff Parker, Maynard Smith, and J. R. Krebs have stressed more the importance of the work leading up to Wilson's book, and have in one way or another distanced themselves from his methods of argument and his conclusions about human nature. For instance, Krebs, whose genealogy I have quoted, credited Wilson in his 1975 review with defining the field of sociobiology. But in his 1985 retrospective he needs to stress that the book merely coincided with the publication of major work by other people. "E. O. Wilson's book *Sociobiology*, published ten years ago, was by no means the start of sociobiology and behavioural ecology. But its publication coincided with, and perhaps helped to sustain, an almost explosive growth of interest in the subject." Whereas earlier Krebs had praised Wilson's work as "outstanding, both as an encyclopedic review of the literature and as a lucid, critical synthesis of theoretical concepts," his later review says, "It is an eclectic compendium of facts with a limited amount of theorizing." The retrospective is still favorable, as are all the recent retrospectives by evolutionary biologists that I have seen. But one can sense in it that the controversy over the book was not entirely appreciated by others who had long been working in fields that have come to be defined as sociobiology. For instance, in 1982, Patrick Bateson explicitly disassociated researchers in the King's College Sociobiology Group from the more controversial aspects of sociobiology: "Nobody who knew their work could accuse them of doing bad science. Furthermore, they would tolerate neither sloppy argument nor extravagant generalisations from studies of animals to humans."[37] To return to the metaphor I have used, in which texts are collected like items in a museum, the response of other sociobiologists to Wilson can be likened to the response of the curator of a natural history museum who arrives at work to find a living wooly mammoth in the great hall; it brings in the crowds, it's worth studying, and it certainly deserves respect, but every time it moves it messes up all the other cases.

36. Rose et al., *Not in Our Genes*, p. 243; see Gould, "Biological Potential," for a criticism of an article containing the most absurd of such claims.

37. Krebs, "Sociobiology Ten Years On," p. 40; Bateson, "Preface," p. x.

The Truth Lies Somewhere in Between

In quoting and paraphrasing, writers on both sides of the controversy create a version of the texts they read and criticize. In giving genealogies, writers create a version of the text's past and future affiliations. Writers also give a version of the controversy, a narrative that explains how there could be disagreements about matters of fact. Two devices particularly interest me in these versions: the creation of opposition, and the asymmetrical accounting for controversy. That is, various participants define the issues of the controversy in terms of two poles, and then place themselves between the two poles while their opponents are at one or the other of the extremes. And various participants explain why there is a controversy by giving a sociological explanation for their opponents' errors, while explaining their own position in scientific terms.

The creation of opposition has been a frequently used device, not only in the sociobiology controversy, but in earlier controversies on similar issues. In the kind of heated debate that Nature vs. Nurture always involves, any participant can step in as the voice of reason. For instance, Niko Tinbergen builds his 1968 *Science* essay around the apparent opposition between ethology and comparative psychology, Lorenz and Schneirla, which he can then reconcile with his own more moderate reading of both positions. Wilson devotes part of an introductory chapter of *Sociobiology* to listing the various "Dualities of Evolutionary Biology," many of which crumble into "semantic ambiguity" upon his closer examination. He uses the device of oppositions in the passage I have discussed when he defines his apparently moderate position against the "extreme environmentalism" described in the quotation from Dobzhansky.

The Sociobiology Study Group are equally careful not to be identified with an extreme position.

> We are not denying that there are genetic components to human behavior. But we suspect that human biological universals are to be discovered more in the generalities of eating, excreting, and sleeping than in such specific and highly variable habits as warfare, sexual exploitation of women and use of money as a medium of exchange.

Wilson responds to the SSG's criticism of his "biological determinism" by proposing two poles and placing himself between them, so that the SSG position, and not his own, is seen as the extreme:

> In their earlier *New York Review* statement (Allen et al. 1975) the group . . . maintained that although eating, excreting, and sleeping may be genetically determined, social behavior is entirely learned; this belief has been developed further in the *BioScience* article. In contrast, and regardless of all they have said, I am ideologically indifferent to the degree of determinism in human behavior. If human beings proved infinitely malleable, as they hope, then one could justify any social or economic arrangement according to his personal value system. If on the other hand, human beings proved completely fixed, then the status quo could be justified as unavoidable.
>
> Few reasonable persons take the first extreme position and none the second. On the basis of objective evidence the truth appears to lie somewhere in between, closer to the environmentalist than to the genetic pole. That was my wholly empirical conclusion in *Sociobiology: The New Synthesis* and continues to be in my later writings.

Wilson accuses the SSG of misrepresenting his views "in order to have a conspicuous straw man."[38] But the strategy on both sides actually requires two opposing straw men. Mary Midgely exploits this symmetry in her philosophical analysis, "Rival Fatalisms: The Hollowness of the Sociobiology Debate." What she misses, I think, when she ends by taking position between the two extremes (a moderate position she identifies with Lorenz and Irenäus Eibl-Eibesfeldt) is that all the other participants can make the same rhetorical move. Compare the passage I've just quoted, in which Wilson rejects both extreme positions and places himself closer to environmentalism, to this passage from Steven Rose's critique:

> The real failure of the sociobiologists lies in their seeming inability to avoid the either/or trap. Behavior must be either socially or biologically determined, or must represent the arithmetic sum of a biological (genetic) and an environmental component. On the contrary, a proper understanding of the interaction of the biological and the social in the production of humans and their society will only be possible following the simple recognition that both genes and environment are perfectly necessary to the expression of any behavior.[39]

38. Allen et al., "Against Sociobiology," p. 264; Wilson, "Academic Vigilantism," pp. 292, 293.

39. Rose, "It's Only Human Nature," p. 169.

It is hard to imagine Wilson, or any other biologist, disagreeing with Rose's position as it is stated in the last sentence. Indeed, on the evidence of all these passages, a literal-minded reader might think that all the participants in the debate, without their realizing it, are in agreement. Of course they are not; they are agreed only in using the same device, defining opposing positions by presenting them as extreme poles of nature or nurture.

Call in the Sociologists

Part of the controversy is in accounting for why there is a controversy in the first place. The controversy is presented by all participants as being in some way unpleasant and unscientific. "Beneath the smoke," says Wade "is a scientific issue." Participants on both sides see social forces at work in the scientific controversy, but both invoke them only to explain false positions—the sociologists are called in to talk to the other guy. Richard Burian, a critic of sociobiology, says, "It is of no little sociological interest that sociobiology has met with considerable success in the academic world; publishers, universities, professional associations, and many working scientists believe that it has already established itself as a legitimate scientific discipline." Albury sees the presence of "a sociopolitical element" as a problem for "the defenders of sociobiology and their philosophical allies," but does not seem to think it a problem for the scientists criticizing sociobiology. On the other hand, Gerald Holton quotes the comments of Alexander Morin of the NSF, accounting for the opposition to sociobiology in theological terms: "Why does it arouse such passionate opposition, even among people who, in other fields of enquiry, are (or appear to be) dispassionate in their scientific consideration of science? Because what we are seeing, I think, is not a scientific response to evidence but a doctrinal response to heresy."[40] Holton's analysis, too, takes the scientific response to evidence for granted, as undiscussable, while calling for the social analysis of the unscientific. In Burian's case, the anomaly is the social success of a pseudoscience; In Morin's case, the analysis is to be made in the familiar terms of religion versus science. These two kinds of explanation of controversy, one based on social factors and the other on psychological, are similar in being applied asymmetrically. Good science apparently needs no explanation.

A number of accounts explain the controversy by saying that the other side has an unscientific emotional interest in the debate. The

40. Wade, "Sociobiology," p. 325; Burian, "Methodological Critique," p. 392; Albury, "Politics and Rhetoric," p. 529; Holton, "The New Synthesis," p. 82n.

SSG's letter in the *NYRB* says, "What Wilson's book illustrates to us is the enormous difficulty in separating out not only the effects of the environment (e.g., cultural transmission) but also the personal and class prejudices of the researcher." In his response, Wilson comments on the "remorseless zeal" of the SSG. Arthur Caplan refers to the SSG's letter as "impassioned" and to Wilson's response as "equally impassioned." The geneticist James King says, of controversies about field studies of animals, that "The intense emotional involvement of sociobiologists makes it hard to achieve scientific give and take with them." But a sociobiologist might detect signs of King's own emotional involvement, when, in the conclusion to his article, he says that in sociobiology "a jerry-built doctrine has been compounded of old hat genetics that current research has already rendered obsolete, of sophomorically cynical interpretations of social relations, and of a doctrinaire rejection of the contribution of ontogeny to the behavioral phenotype." This is not an invitation to scientific give and take.[41]

Burian's reference to the academic success of sociobiology is an example of another sort of attribution of unscientific, socially contingent influences; both sides argue that the other side is accepted merely because it is fashionable. Washburn regrets that it is "fashionable to minimize the nature–nurture argument" as Wilson does in his introduction. Waddington calls altruism "a fashionable topic for a rather foolish controversy." Steven Rose says, of attempts to account for social change in sociobiological terms, "at best the exercise becomes a piece of fashionable Harvard or Oxford intellectual games-playing; at worst a way of ideologically justifying the status quo."[42]

The suspicion of the fashionable remains even when the writer is not entirely hostile to the fashionable idea. In one of the first reviews, Donald Stone Sade expresses his skepticism about the manner of proponents of inclusive fitness and kin selection. "In the conference chambers of scientific meetings I have seen these ideas, like the sweet smoke of a forbidden weed, create a sense of euphoria among their advocates, who seem on the verge of some hidden truth, obscure until the inhalation of these heady notions. Wilson, by contrast, appears to intend his book to be a challenge." Sade and other supporters of Wilson use this device to distinguish him from less scholarly writers. Mary Midgely considers Wilson's followers to be more dangerous

41. Allen et al., "Against Sociobiology," p. 264; Wilson, "Academic Vigilantism," p. 298; Caplan, "Ethics," p. 309; King, "Genetics of Sociobiology," pp. 101, 104.

42. Washburn, "Animal Behavior," p. 57; Waddington, "Mindless Societies," p. 254; Rose, "It's Only Human Nature," p. 167.

than Wilson himself, because their convictions, unlike his, are based on the blind following of fashion. "Like any flag-waving movement, as it gathers strength it is bound to attract a mass of supporters who will catch their leaders' confidence without his scruples and without understanding his limitations . . . The academic world is full of people who ask nothing better than to settle into such an army. Wilson is a prophet, and he isn't going to lack acolytes." Of course, the same phenomenon of a new figure rapidly gathering support can be described as "consensus" or as "broad agreement" or as "a new paradigm" if it supports one's own position.[43] (And as we saw in the biologists' comments in ten-year retrospectives, there seems to be, if anything, a tendency for those whose ideas are closest to those of the controversial figure to try to distance themselves from him, not to appear as his acolytes.)

One version of this explanation in terms of fashion associates the view one opposes with popularity outside the scientific community. Wilson himself uses this popularity against the earlier popularizing sociobiologists, and his supporters contrast his difficult and scholarly work with the bestsellers. On the other hand, almost any essay critical of Wilson, by the Sociobiology Study Group, Gould, Rose, Montague, or others, starts with the popular success of *Sociobiology* and the appearance of its argument in such nonscientific magazines as *House and Garden* and *People.* We have seen that Jerry Hirsch criticizes Wilson for including citations to such journals as *Atlantic* and *Scientific American.* In each case, public interest and acceptance is itself evidence of the unscientific nature of the argument. It may seem odd to find this tactic being used by authors like Gould, Rose, and Wilson himself, who are highly successful popularizers as well as prominent researchers. But as we saw in chapter 5, there is a curious ambivalence in analyses of the process of popularization; it can be seen either as the vulgarization of pure science to pander to the tastes of the ignorant, or as the stripping away of the obfuscation of the specialists by talented writers who can make essential ideas accessible. So here the attempts to reach a wide audience with one's claims can be seen either as a laudable awareness of the social implications of scientific ideas, or as a dangerous attempt to enlist the authority of science for one's personal political beliefs.

43. Sade, "Evolution of Sociality," p. 244; Midgley, "Rival Fatalisms," p. 26. Nigel Gilbert and Michael Mulkay analyze one case of the representation of consensus in *Opening Pandora's Box,* (pp. 112–40).

Methodological Parodies

It might seem that the controversy would come down to questions of methodology and philosophies of science. And since most participants agree in using the vocabulary of Karl Popper (those who make their case based on Kuhn or Paul Feyerabend are looked upon with suspicion by both sides) it might seem that there were grounds for distinguishing scientific from nonscientific practice. But as in other controversies, there is disagreement, especially between practitioners of various disciplines, about what counts as a falsifiable hypothesis and what counts as a test. The language of Popper becomes a general rhetorical resource that has little to do with the structures of the texts.[44]

The most common form of methodological argument in the controversy is to present a parody of the methods given by the opposing side. For instance, both the Sociobiology Study Group and Rose, Lewontin, and Kamin structure their attack around a story of how the sociobiologist proceeds. They present sociobiological method as a "Just-So Story" in which present-day features are reified and then projected into the past to give a pseudohistorical explanation.

> Sociobiology, as a theory of human society, is built of three parts. First, there is a description of the phenomenon it is meant to explain, that is, a statement of human nature. This description consists of an extensive list of characteristics that are thought to be universals in human societies, including such diverse phenomena as athletics, dancing, cooking, religion, territoriality, entepreneurship, xenophobia, warfare, and the female orgasm.
>
> Second, having described human nature, sociobiologists claim that the universal characteristics are encoded in the human genotype. . . .
>
> The third step in the sociobiological argument is the attempt to establish that the genetically-based human universals have been established by natural selection during the course of human biological evolution. . . .

44. See Jonathan Potter, "Testability, Flexibility; Kuhnian Values in Scientists' Discourse Concerning Theory Choice," *Philosophy of the Social Sciences* 14 (1984): 303–30, for a study of the use of this rhetoric by psychologists.

> In what follows, we look more closely at these three elements of sociobiology: the description of human nature, the claim of its innateness, and the argument for its adaptive origin.[45]

This version simply reverses the adaptive narrative that Wilson uses in his book, so that now the causes run from the trait, through demography, to the environment, and the explanations move from humans to animals rather than from animals to humans. The force of such a parody comes, not from comparison to good science, to the authors' own scientific work, or to some philosophical model, but from the ironic reversal of Wilson's own methodological model.

In *Sociobiology,* Wilson's methodological criticisms, and his methodological parodies, are reserved for researchers in the disciplines closest to his own; he uses them to suggest the strength of this own method by contrast. For instance, he criticizes the best-selling books on sociobiology that preceded his for using what he calls "the advocacy method" (p. 28).

> In sociobiology, it is still considered respectable to use what might be called the advocacy method of developing science. Author X proposes a hypothesis to account for a certain phenomenon, selecting and arranging his evidence in the most persuasive manner possible. Author Y then rebuts X in part or in whole, raising a second hypothesis and arguing his case with equal conviction. Verbal skill now becomes a significant factor. Perhaps at this stage author Z appears as an *amicus curiae,* siding with one or the other or concluding that both have pieces of the truth that can be put together to form a third hypothesis—and so forth seriatim through many journals and over years of time.

This often-quoted passage comes early in the book. Wilson goes on to present the correct method for pursuing sociobiology, through "strong inference." This method, though attractive, is both unconvincing and unwieldy, and Wilson refers to it again only in token passages through the book. It is not his use of strong inference, but his irony in

45. The quotation is from Rose et al., *Not in Our Genes,* p. 243; SSG, "Sociobiology," p. 282.

describing the advocacy method that makes us see him as doing something different.[46]

Wilson also gave methodological accounts of the various disciplines that are to go into the sociobiological synthesis. These provoked angry responses from anthropologists, geneticists, and comparative psychologists, despite, or perhaps because of, the apparent respect Wilson has for their findings. The following description of the work of some comparative psychologists can serve as an example of Wilson's double-edged prose which, without exactly criticizing, produces a description that I imagine would not be accepted by comparative psychologists (p. 349).

> Although vertebrate studies are marked by eclecticism, as Lehrman and Rosenblatt said, much of the work remains motivated by a very few strong, albeit implicit themes. One is environmentalism. The background of a majority of the researchers is in anthropology or experimental psychology, in which there exists a bias to assign as much of the measured intraspecific variance of behavioral traits as possible to environmental influences. There is nothing wrong with this attitude; it can be quite heuristic as long as it is kept explicit. The bias results in a determined probe to catalogue and weigh all possible environmental factors, both those manifest in naturalistic studies of free-ranging populations and others that become apparent only when their effects are magnified through experimental manipulation. . . . The developmental psychologists cannot be too

46. The irony of Wilson's paraphrase and commentary also serves to distance him from these methods of John C. Lilly's popular books on dolphins, which he fears have misled the public and other scientists about the methods of sociobiology. I shall quote enough to give a sense of the tone (p. 474):

> Although Lilly never states flatly that the dolphins and other dolphinids are the alien intelligences he seeks, he constantly implies it. . . .
> Anecdotes are used to launch sweeping speculations. . . .
> This fantasy is then turned into a premise for even stronger discussion and speculation. . . .
> This example fairly represents the overall quality of Lilly's documentation and logic. Objective studies of behavior under natural conditions are missing, while "experiments" purporting to demonstrate higher intelligence consist mostly of anecdotes lacking quantitative measures and controls. Lilly's writing differs from that of Herman Melville and Jules Verne not just in its more modest literary merit but more basically in its humorless and quite unjustified claim to be a scientific report.

With different quotations where I have left ellipses, this could be from an article by the SSG attacking Wilson.

> far off the correct path; it is better to have too much information than too little, especially when a discipline has only weakly defined its questions.

In the literal sense of this and similar passages, there is nothing but praise. But Wilson implies that an unacknowledged bias underlies the whole discipline, and even the praise at the end of the quoted passage places this discipline, which is at least several generations old, as still rather undeveloped and immersed in detail.

The responses to such descriptions by comparative psychologists like Frank Beach, Jay Rosenblatt, and Ethel Tobach may serve as typical of the responses of angry population geneticists, ethologists, and anthropologists. Although they make a number of fundamental criticisms, focusing on the failure to distinguish between levels of causation,[47] they do not establish universal scientific standards, and show how Wilson deviates from them, but instead defend the questions and practices of comparative psychology. Beach, for instance, does this by using loaded language and heavy irony to identify science with his discipline and nonscience with sociobiology (p. 133).

> The model-building sociobiologist may be unconcerned by a primatologists's criticisms to the effect that some reports of infanticide are of dubious reliability or that at most infanticide can be considered a rare form of behavior associated with abnormal ecological conditions. Such complaints may be seen as irrelevant by a theorist who is concerned neither with understanding the behavior of langurs as a species nor with analyzing the proximate causation of a particular behavioral incident. This attitude obfuscates effective communication with the comparative psychologist whose principal goals are to describe, measure, and compare analogous behavior patterns in different species and to analyze behavior in terms of its motivational and mediational components. To such an individual, the sociobiologist may fit in the category described by B. F. Skinner (1938, p. 44) as "men whose curiosity about nature is less than their curiosity about the accuracy of their guesses."

This passage does not show that sociobiology is not a science; it only shows that it is not good comparative psychology. Comparing this passage to Wilson's methodological parodies, we might think that

47. Professor Wilson argues that he has responded effectively to the criticisms of comparative psychologists, especially in his more recent work.

philosophy of science does not seem to provide the sciences with agreed-on methods, but only with a generally shared vocabulary of polemical abuse. Still, the fact that both sides use this vocabulary implies that they believe the controversy about human nature is to be settled on scientific grounds.

Science versus Ideology

One way in which Marxists have responded to the many texts that use sociobiology as scientific support for capitalism, sexism, or racism is to make a distinction between real science and ideology. This line of rhetoric, too, is a mirror image of that used by Wilson, when he accuses his critics of putting political considerations ahead of science. Wilson is, of course, using *ideological* in a sense significantly different from the various senses the term has in Marxist thought. Rose, Kamin, and Lewontin take their definition from Marx's *The German Ideology*, in which ideology is "the ideas of the ruling class" that "in every epoch are the ruling ideas"; ideology then is to be traced outward to the relations of production.[48] Wilson uses the term in the more popular sense, to describe the taint of politics in what is thought to be the nonpolitical; ideology then is to be traced inward to personal prejudices. Despite this basic difference, both sides use the the opposition of science to ideology to explain why other scientists could have other ideas. But both the explanation which sees ideology as the reflection of the interests of the ruling class and that which sees it as the dark irrational receding before the progress of science lead to some rhetorical problems. Both assume that there are agreed-on methods that allow us to tell real science from ideology. Thus neither questions the absolute authority of real science in the social arena. And neither narrative can account for the scientific beliefs the authors hold themselves; neither narrative can account for "real" science.

Wilson says in his response to the SSG's letter that it "is an openly partisan attack on what the signers mistakenly conclude to be a political message in the book."[49] As we have seen, all parties have rhetorical reasons to present disagreements as misinterpretations of an unambiguous text. In his more detailed response, "Academic Vigilantism," Wilson says he will account for the SSG's misinterpretation. "How is it possible for the Science for the People Group to misrepresent so consistently the content of a book, in contrast to all

48. *Not in Our Genes,* p. 4n.
49. Wilson, "For Sociobiology," p. 265.

the many reviewers among their scientific colleagues?" He starts with demographic terms, "the size and composition of the group," and one might think we are to get a sociological explanation of the opposition in terms of the population biology of academic groups, leading from certain environmental factors in Boston-area universities to demographic shifts in university faculties to political behavior. But the fact that he refers to the "political significance of sociobiology" in the title does not mean he is going to do a political analysis of his own science; he is referring to the significance conferred on the science by the activities of its opponents.

Wilson, like his critics, sees human affairs as governed by forces and relations that are hidden from them by a veil of unreality. In Wilson's case, the unreality is not capitalist ideology, but irrational impulses held over from earlier stages of the evolution of culture, especially from the adaptations of paleolithic hunter-gatherer societies. "Value systems are probably influenced, again to an unknown extent, by emotional responses programmed in the limbic system of the brain." It is part of the progress of science to tear away such illusions, letting us see these values as they really are, in terms of our present environment. The views of his political critics must be treated separately from those of his scientific critics, for he sees the political critics—just as they see him—as starting with nonscientific goals that lead to a commitment to one scientific theory.

> The belief-system they promote is clear-cut and rigid. They postulate that human beings need only decide on the kind of society they wish, and find the way to bring it into being. Such a vision can be justified if human social behavior proves to be infinitely malleable.

Marxism, then, is cast in the role the church has in nineteenth-century debates; he describes it in terms such as *belief system* and *vision:*

> When the attacks on sociobiology came from Science for the People, the leading radical left group within American science, I was unprepared for a largely ideological argument. It is now clear that I was tampering with something fundamental: mythology. Evolutionary theory applied to social systems is an extension of the great Western traditions of scientific materialism. As such, it threatens to trans-

> form into testable hypotheses the assumptions about human nature made by some Marxist philosophers.[50]

In his narrative, then, the persistent progress of science is opposed by both religion and Marxism, and both sources of opposition represent prescientific forms of thought that are defended by powerful interests, and that appeal to deep emotional (and therefore neurological) responses. (Thus, as Albury and others point out, defenders of Wilson repeatedly invoke such figures as Galileo and Darwin).

I noted in chapter 1 that the Strong Programme in the sociology of scientific knowledge requires "symmetrical" explanations that can account for "true" as well as "false" beliefs. Wilson, like the scientists studied by Nigel Gilbert and Michael Mulkay in *Opening Pandora's Box*, has two different kinds of explanation. The ideological beliefs of his opponents are to be attributed to social causes: their politics and their careers.

> In retrospect there appear to be two levels of meaning to their protestations. The outer meaning is the literal argument they gave, that "genetic determinism" of any kind will inevitably be used to justify reactionary political doctrines, racism, sexism, aggression, and other undesirable social responses associated with acceptance of the status quo. The deeper meaning, in my opinion, was the challenge they sensed to their own authority as natural scientists devoted to the study of social problems. ("Foreword," p. xiii)

But Wilson presents his own scientific beliefs as the outcome of processes entirely internal to science: the synthesis of population biology and behavioral ecology. In this account, it would be irrelevant to ask about Wilson's political views, or about Stephen Jay Gould's paleontology.

An account of sociobiology as ideological is given by the SSG:

> Determinist theories all describe a particular model of society which corresponds to the socioeconomic prejudices of the writer. It is then asserted that this pattern has arisen out of human biology and that human social arrangements are either unchangeable or if altered will demand continued conscious social control because these changed conditions will be "unnatural." Moreover, such determin-

50. On value systems, see Wilson, "Academic Vigilantism," quotations from pp. 297, 300, and 292, respectively. On Marxism, see Wilson, "Introduction," p. 2.

> ism provides a direct justification for the status quo as "natural," although some determinists disassociate themselves from the consequences of their arguments.

The "socioeconomic prejudices" of the writer are the motivating force behind the theory. Though such a theory can have no evidence to support it, the SSG argue, it is "seized upon and widely entertained, not so much for its alleged correspondence with reality as for its obvious political value." This is what Joe Crocker calls a "conspiracy theory" of sociobiology. The narrative is oddly parallel to the narrative of evolutionary adaptation in which environmental changes (here socioeconomic structures) lead to demographic changes (here ideological structures) which lead to certain behaviors (here historical events). Steven Rose says we must ask of the theory, "Who benefits?" But this is just what a sociobiologist asks of an instance of animal behavior.[51]

Sometimes texts by SSG members seem to put forward a view of the relation between science and ideology that sees all science as inherently political, but this view, if it is there, soon dissolves. So they say:

> Our central point is that sociobiology—like all science—proceeds in a social context; "pure objectivity" is as much a myth for sociobiologists as for science reporters. All attitudes towards sociobiology—ours as much as any—reflect certain political preoccupations which need to be made explicit.

This starts off by seeming, in the phrases set off by dashes, to admit a symmetry of interpretation in which the views of both sociobiologists and their critics would emerge from "social context." But the passage goes on to explain that social context affects attitudes towards ideas; it apparently does not construct the ideas themselves. And then it isolates sociobiology on a scale of the sciences as being further from facts and closer to human concerns: "The weaker the constraint of fact, the closer the subject to immediate human concern, the greater the influence of these preoccupations." Finally, the Marxist analysis applies only to sociobiology, not to all science. "What we have argued, and continue to assert, is that sociobiological ideas do not arise in a social vacuum but rather reflect the dominant interests and attitudes of the classes to which the authors belong."

51. SSG, "Sociobiology," pp. 280, 281; Rose, "It's Only Human Nature," p. 162.

When the SSG attack Wilson, they point out that he can give no account of how one gets from one level to another, from one stage of the theoretical narrative to the next—for instance, he does not actually locate specific genes for specific behaviors and show how they are selected and how they operate in the animal's development. But in the same way, they do not show, and do not need to show, exactly how Wilson's ideas, or anyone's ideas, actually arise from the forces and relations of production, or how Wilson's statement of these ideas causes events that would not otherwise have happened. Instead they argue, just as they say Wilson does, from analogy between two systems.[52]

The SSG have an explanation for why the ruling classes have the ruling ideas, and for how they are reproduced in books like *Sociobiology*. But no account is given in the SSG's text of "those of us who would change the way things are," of what interests and attitudes they reflect, or how they came into being in this socioeconomic environment. Another and more fundamental problem this view of ideology poses for Marxists is that it leaves intact the authority of science (that is, "good science" as opposed to the "bad science" of sociobiology) as something outside and opposed to ideology.[53] Rose,

52. Alper et al., "Implications of Sociobiology," quotations on pp. 334, 336, respectively.

Rose, Kamin, and Lewontin trace this form of argument to Boris Hessen's analysis of Newton in the 1931 collection *Science at the Crossroads* (see Werskey *The Visible College*, for an historical account), an analysis that has been enormously influential in later attempts to show the relation between science and society. Hessen's argument implies that if one set of concepts, such as Newtonian physics, can be seen to be structured like another, such as capitalist exchange, then it can be assumed that the social system caused the concepts in the scientific system, even though the details of cause and effect would be far too complicated to trace. Similarly, Wilson's critics assume that if they can show that his text incorporates terms and concepts from capitalist economics, which it does, then it both arises from and contributes to capitalism.

53. One critique of sociobiology that does not rely on the distinction between good science and bad science is an article in the *Radical Science Journal* by Joe Crocker, "Sociobiology: The Capitalist Synthesis." Crocker supports the conclusions of Steven Rose's critique, but rejects Rose's view of science: "Because ideology and truth are mutually exclusive in Rose's philosophy of science, it would be disastrous for his politics if sociobiology were in any sense true. From the start, he is obliged to dismiss it as false. In this article, on the other hand, it is insisted that all science ("good" or "bad") is incorrigibly ideological. Sociobiology is ideological precisely because its practitioners aspire to be good scientists in the tradition of Newton and Galileo, seeking to deduce universal social laws from individual behavior" (p. 61).

The problem with this approach for many scientists is that Crocker would see ideology, not only in the argument of sociobiology, but in such basic processes as quantification. "Scientific categories," he says, "are *constituted* by social relations such

Kamin, and Lewontin argue that a science that draws its terms and concepts from dialectical materialism does not arise from capitalism, and will not lend scientific authority to the maintenance of the capitalist system.[54] But such an approach just shifts the scientific metaphor from optimization to dialectic. One alternative to such tactics, I would argue, is to promote skepticism about the production of all knowledge and about its authority in political discourse. In this view, we do not undermine the political authority of sociobiology by saying that Wilson is just telling stories. Wilson is telling a story very well, and the only effective answer to a story, as I think his critics show, is to tell another story.

The Polar Bear and the Whale

I began this chapter by noting the surprising persuasive power that *Sociobiology* has even for someone like me who came to it forearmed with criticisms, and who still does not agree with it. One of the anomalies that the analyst of rhetoric finds in the controversy that followed the book is that neither side seems particularly interested in persuading anyone who does not already agree with them. Indeed, their texts, like the first SSG letter, or like the aggressive passages at the beginning and end of *Sociobiology*, would tend to alienate anyone coming to them with even slightly different views. In this sense the debate never seems to get anywhere, and yet it goes on and on. I have noted that the methodological arguments of the two groups of experts do not lead to a resolution in which one side can clearly claim the authority of science, because there is no agreed-on method, but only a shared rhetoric of praise and abuse, and because in practice each scientific discipline takes its questions and practices as the standard.

Two sociological analyses of controversies have suggested functions for such debates, both focusing on the way the participants appeal to an audience outside science. Yaron Ezrahi notes a sort of rhetoric similar to that we have seen in the sociobiology controversy in his analysis of the controversy over alleged links between race and lower I.Q. test scores.

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that the world is comprehended in their image" (p. 64). Such a position requres that one give up a lot; as the last sentence of the passage suggests, Crocker would call physics into question as well as sociobiology. This does not lead Crocker to the acceptance of sociobiology or the rejection of all scientific knowledge, but it does lead to the rejection of any scientific claim to objective knowledge of a world that transcends social processes.

54. *Not in Our Genes*, pp. 265–90.

> My first contention is that the strategies of the principal participants in the controversy prove that the controversy was not focused on the effort of settling differences of scientific opinion by the application of scientific norms of discourse and proof, but was primarily a contest between rival scientific groups and their respective supporters over which definitions of fact and reality would be certified by the collective authority of science as valid premises of public policy or social interaction. The issue was not one of defining social fact but of establishing as facts in the social context certain propositions.[55]

I would not see such rhetoric in either the race/I.Q. controversy or the sociobiology controversy as a sign of deviance from scientific norms under social pressure, for as I have argued in earlier chapters, I see the "scientific norms of discourse" as another kind of rhetoric. But Ezrahi's suggestion that such debates are about rival claims to authority can help us understand the odd construction of these texts. They must address the public audience while seeming not to, for in the competition among scientists for public authority, to acknowledge that one wants political authority is to disqualify oneself as a scientist.

Ezrahi's analysis can help us understand the odd tone of texts that do not seem intended to persuade their nominal scientific audience. Ullica Segerstrale, in an excellent analysis of interviews with participants in the sociobiology debate, can help us understand the persistence of the controversy over the last ten years. Segerstrale argues that both sides have an interest in keeping the controversy going.

> Once the sociobiology controversy began, strategic interests came into play on both sides. As the debate developed, it was in neither party's interest to straighten out misunderstandings—instead the point became to develop one's own position while dismissing the opponent's one as "extrascientifically motivated. This way Lewontin let Wilson graduate to a leader first of the "adaptationist" and later of the "reductionist" program, while Wilson chose to retain Lewontin as a useful strawman for *tabula rasa* "Marxist" environmentalism.

55. Yaron Ezrahi, "The Authority of Science in Politics" in *Science and Values*, ed. A. Thackrey and E. Mendelsohn (New York: Humanities Press, 1974), p. 232.

In quoting a comment on the race/I.Q. detate, I do not mean to imply that it is necessarily the same issue as the sociobiology debate. As I have noted, such a claim is part of the political genealogy the SSG tries to draw for Wilson. For a Marxist analysis of the I.Q. controversy, see Les Levidow, "IQ as Ideological Reality," in *Radical Science*, ed. Les Levidow (London: Free Association Books, 1986).

Segerstrale's analysis helps us understand why neither side makes the sort of moves towards consensus one might expect in a controversy within a scientific discipline. Though I find Segerstrale's analysis of Wilson's and Lewontin's views very useful, I do not think that the origins of the controversy can be traced to the personal careers and goals of two scientists.[56] Segerstrale draws on extensive (and fascinating) interview material, and that may be why in that analysis the basic causes seem to be on the level of the strategies of individual scientists, as in the quotation given above. My study has drawn only on some of the published texts in the controversy, and in them one gets no sense of authors in control of their own rhetoric, nor of the wide divergence of strategies. The characters of Wilson and Lewontin, central to Segerstrale's analysis, seem to me to be creations of the controversy, not the creators of it. The two sides do not produce two different kinds of texts, in accordance with their radically different views about science and society. Instead, their rhetorical tactics seem to reflect each other, and their phrases and arguments echo back and forth, as if each text was made out of bits and pieces of the preceding text, reinterpreted to read ironically.

A number of studies cited in chapter 4 have shown how a limited controversy within a scientific discipline (geology, high-energy physics, biochemistry) tends to be resolved in the course of repeated interpretation such that the whole controversy, and the losing side, are forgotten, and all the remains is a fact. The participants in the sociobiology debate act as if it too can be resolved without leaving a trace, as soon as *their* facts are accepted by the public as *the* facts. Indeed, the tendency in more recent writings has been to act as if this has already happened, so that critics tend to act as if sociobiology were publicly discredited, whereas sociobiologists tend to act as if the political criticism were a thing of the past (neither of which views is supported by a survey of articles published in the last few years). The story of the sociobiology controversy looks more like that of the controversy over *Cnemidophorus* we saw in chapter 4, in which the two

56. Segerstrale, "Colleagues in Conflict," p. 79. The conflict between Wilson and Lewontin is a good story, and the story was used in some journalistic accounts of the controversy (see, for instance, Colin Campbell, "Anatomy of a Fierce Academic Feud," in the *New York Times*, which is based on Segerstrale's article). The SSG is right to say this focus trivializes the debate, which is about more than personalities. Despite the title of Segerstrale's article, it is not just about personalities, and it says at the outset, "the sociobiology controversy would be misconstrued if it were seen as merely 'an in-house quarrel between Harvard professors,' whether politically motivated or not" (p. 54).

sides continue their own lines of work, without addressing the other. Such nonresolution is even more likely in the sociobiology debate. In a controversy between members of different disciplines, a controversy about the origins and future of human society, a controversy between what Segerstrale calls "scientific-cum-moral agendas," no facts are likely to be agreed on.

In "From the History of an Infantile Neurosis," Freud comments on his ongoing controversy with Jung and Adler, and their failure to agree on what he considered basic postulates: "The whale and the polar bear, it has been said, cannot wage war on each other, for since each is confined to his own element they cannot meet." There is no reason why the sociobiology controversy should not continue indefinitely.[57]

57. Professor Wilson disagrees with my conclusion that the controversy can go on indefinitely. But then, when Freud used the metaphor of the polar bear and the whale, he did not mean that his controversy with Jung and Adler could go on forever; he was pointing out that it was not worth arguing with them when they would not accept his postulates, and was implying that his argument would triumph.