"Naturalist" By E.O. Wilson 1995

chapter seventeen

THE SOCIOBIOLOGY CONTROVERSY

THE SPATE OF REVIEWS THAT FOLLOWED THE PUBLICATION OF *Sociobiology* in the summer of 1975 whipsawed it with alternating praise and condemnation. Biologists, who as a rule had little stake in the human implications, were almost unanimously favorable. They included Lewis Thomas and C. H. Waddington, elder statesmen of the day. Researchers closest to sociobiology were especially supportive, and they grew more so as time passed. In a 1989 poll the officers and fellows of the international Animal Behavior Society rated *Sociobiology* the most important book on animal behavior of all time,

edging out even Darwin's 1872 classic, *The Expression of the Emotions in Man and Animals*.

Social scientists already engaged in biology-accented research also leaned in favor. They included Napoleon Chagnon, ethnographer of the "Fierce People," the Yanomamö of Brazil and Venezuela; and the sociologists Pierre van den Berghe and Joseph Shepher, who sought biological explanations of incest avoidance, marriage customs, and other key aspects of human behavior. Paul Samuelson, Nobel laureate economist turned public philosopher, favored the approach in one of his *Newsweek* columns but said *beware*—this subject is an intellectual and doctrinal minefield.

Samuelson was right. A wave of opposition soon rose among social scientists. Marshall Sahlins, a cultural anthropologist, made a strong attempt to exempt human behavior from the tenets of sociobiology in his 1976 book, *The Use and Abuse of Biology*. In November of that year the members of the American Anthropological Association, gathering in Washington for their annual meeting, considered a motion to censure sociobiology formally and to ban two symposia on the subject scheduled earlier. The arguments of the proposers were mostly moral and political. During the debate on the matter Margaret Mead rose indignantly, great walking stick in hand, to challenge the very idea of adjudicating a theory. She condemned the motion as a "book-burning proposal." Soon afterward the motion was defeated—but not by an impressive margin.

Because such events were widely publicized, with some journalists calling the controversy the academic debate of the 1970s, it is easy to exaggerate the depth of the opposition. The serious literature was in fact always strongly disposed toward human sociobiology. In the nearly twenty years since 1975, more than 200 books have been published on human sociobiology and closely related topics. Those more or less in agreement outnumber those against by a ratio of twenty to one. The basic ideas of sociobiology have expanded (their critics might say metastasized) into fields such as psychiatry, aesthetics, and legal theory. Four new journals were created in the late 1970s to accommodate a rising number of research and opinion articles.

Regardless of its real strength, much of the controversy might have been avoided, and for that I must bear the responsibility. I had written *Sociobiology* as two different books in one. The first twenty-six chapters, composing 94 percent of the text, was an encyclopedic review of social microorganisms and animals, with the information organized according to the principles of evolutionary theory. The second, the twenty-nine double-columned pages of Chapter 27 ("Man: From Sociobiology to Sociology"), consisted mostly of facts from the social sciences interpreted by hypotheses on the biological foundations of human behavior. The differences in substance and tone between books one and two give rise to the dual sociobiologies of popular perception. The first is sociobiology as I intended to portray it: a discipline, the systematic study of the biological basis of social behavior and advanced societies. And then there is the evil twin as perceived by Marshall Sahlins and some members of the American Anthropological Association, the scientific-ideological doctrine that human social behavior is determined by genes.

Genetic determinism, the central objection raised against book two, is the bugbear of the social sciences. So what I said that can indeed be called genetic determinism needs saying again here. My argument ran essentially as follows. Human beings inherit a propensity to acquire behavior and social structures, a propensity that is shared by enough people to be called human nature. The defining traits include division of labor between the sexes, bonding between parents and children, heightened altruism toward closest kin, incest avoidance, other forms of ethical behavior, suspicion of strangers, tribalism, dominance orders within groups, male dominance overall, and territorial aggression over limiting resources. Although people have free will and the choice to turn in many directions, the channels of their psychological development are nevertheless—however much we might wish otherwise—cut more deeply by the

genes in certain directions than in others. So while cultures vary greatly, they inevitably converge toward these traits. The Manhattanite and New Guinea highlander have been separated by 50,000 years of history but still understand each other, for the elementary reason that their common humanity is preserved in the genes they share from their common ancestry.

It was the commonality of human nature and not cultural differences on which I focused in *Sociobiology*. At this level what I said could by no stretch be considered original; many others had advanced a similar thesis for decades. Darwin, who seems to have anticipated almost every other important idea in evolutionary biology, cautiously advanced theories of genetic change in aggression and intelligence. But no scientist before me had employed the reasoning of population biology so consistently to account for the evolution of human behavior by natural selection. The human genome is there in the first place, I argued, because it enhanced survival and reproduction during human evolution. The brain, sensory organs, and endocrine systems are prescribed in a way that predisposes individuals to acquire the favored general traits of social behavior.

In order to use models of population genetics as a more effective mode of elementary analysis, I conjectured that there might be single, still unidentified genes affecting aggression, altruism, and other behaviors. I was well aware that such traits are usually controlled by multiple genes, often scattered across many chromosomes, and that environment plays a major role in creating variation among individuals and societies. Yet whatever the exact nature of the genetic controls, I contended, the important point is that heredity interacts with environment to create a gravitational pull toward a fixed mean. It gathers people in all societies into the narrow statistical circle that we define as human nature.

Mine was an exceptionally strong hereditarian position for the 1970s. It helped to revive the long-standing nature–nurture debate at a time when nurture had seemingly won. The social sciences were being built upon that victory. But I hoped that even if sociobiology was dismissed by some of the more established scholars, evolutionary biology, including models of population genetics, would prove attractive to a younger generation of researchers in the social sciences, who might then connect their field to the natural sciences.

That expectation was desperately naive. The sociocultural view favored by most social theorists, that human nature is built wholly from experience, was not just another hypothesis up for testing. In the 1970s it was a deeply rooted philosophy. American scholars in particular were attracted to the idea that human behavior is determined by environment and therefore almost infinitely flexible.

If in fact genes did surrender their control sometime back during human evolution, and if the brain simply resembles an all-purpose computer, biology can play no contributory role in the social sciences. The appropriate domain of sociology would then be variation within cultures, interpreted as the product of environment. And cultural anthropology should concentrate on the internal detailed study of alien societies accepted on their own terms, with minimal reference to extraneous Westernized schemes, including those from biology. There were also important political implications. If human nature is mostly acquired, and no significant part of it is inherited, then it is easier to conclude, as relativists do with passion, that different cultures must be accorded moral equivalency. Differences among them in ethical precepts and ideology deserve respect, for what is thought good and true has been determined more by power than by intrinsic validity. The cultures of oppressed peoples are to be specially valued, because the histories of cultural conflict were written by the victors.

The hypothesis that human nature has a genetic foundation called all these assumptions into question. Many critics saw this challenge from the natural sciences as not just intellectually flawed but morally wrong. If human nature is rooted in heredity, they suggested, then some forms of social behavior are probably intractable or at least can

be declared intractable by ruling elites. Tribalism and gender differences might then be judged unavoidable, and class differences and war in some manner "natural." And that would be just the beginning. Because people unquestionably vary in hereditary physical traits, they might also differ irreversibly in personal ability and emotional attributes. Some people could have inborn mathematical genius, others a bent toward criminal behavior.

In the 1970s a great many ordinary people believed these hereditarian propositions to be more or less true. But anyone who advanced such ideas in colleges and universities risked the scalding charges of racism and sexism. In contrast, those who attacked the hereditarian position were praised as defenders of truth and virtue. The psychobiologist Jerre Levy parodied the politically correct formula as follows: "Even without supporting evidence, the sociocultural hypothesis is assumed to be true unless proved false beyond any possible doubt. In contrast, the biological hypothesis is assumed to be false unless evidence is completely unassailable in its support."*

Understandably, then, American scholars, in a society grown hypersensitive to its internal divisions, shrank from the word "sociobiology." When American researchers formed a professional association on human sociobiology in 1989, they named it the Human Behavior and Evolution Society, and they used the word "sociobiology" only sparingly thereafter at their annual meetings.

The Europeans were less chary. One circle of researchers formed the European Sociobiological Society, headquartered in Amsterdam. Another established the Sociobiology Group at King's College, Cambridge University. A third began the Laboratory of Ethology and Sociobiology at the University of Paris–Nord. The word "sociobiology" and the ideas behind it were freely used in China, the Soviet Union, and other socialist countries, with articles written both for and against it in a scattering of journals.

What made *Sociobiology* notorious then was its hybrid nature. Had

*Jerre Levy, "Sex and the Brain," *The Sciences* 21, no. 3 (1981): 20–23, 28.

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the two parts of the book been published separately, the biological core would have been well received by specialists in animal behavior and ecology, while the writings on human behavior might easily have been dismissed or ignored. Placed between the same two covers, however, the whole was greater than the sum of its parts. The human chapters were rendered creditable by the massive animal documentation, while the biology gained added significance from the human implications. The conjunction created a syllogism that proved unpalatable to many: Sociobiology is part of biology; biology is reliable; therefore, human sociobiology is reliable.

Some of the critics, assuming that I must have a political motive, suggested that the main purpose of the animal chapters was to lend credence to the human chapter. The exact opposite was true. I had no interest in ideology. My purpose was to celebrate diversity and to demonstrate the intellectual power of evolutionary biology. Being an inveterate encyclopedist, I felt an additional obligation to include the human species. As I proceeded, I recognized an opportunity: the animal chapters would gain intellectual weight from their relevance to human behavior. At some point I turned the relationship around: I came to believe that evolutionary biology should serve as the foundation of the social sciences.

Hence my conception of human sociobiology did not spring from any grand Comtean scheme of the relation between the natural and social sciences. I simply expanded the range of the subjects that interested me, starting with ants and proceeding to social insects, then to animals and finally to man. Believing the time ripe for the melding of biology and the social sciences, I used strong, provocative language to start the process. The last chapter of *Sociobiology* was meant to be a catalyst dropped among reagents already present and ready to combine.

Then everything spun out of control. In my calculations I had not counted on the ferocity of the response at my own university. During the McCarthy era, Harvard had been a celebrated—if imperfect—sanctuary for academics accused of being members of the Communist Party. It was supposed to be a forum in which people could exchange ideas with civility, protected from defamation by political ideologues. Yet the fact that it was well populated by leftist ideologues put that genteel goal at risk. Shortly after the publication of *Sociobiology*, fifteen scientists, teachers, and students in the Boston area came together to form the Sociobiology Study Group. Soon afterward the new committee affiliated itself with Science for the People, a nationwide organization of radical activists begun in the 1960s to expose the misdeeds of scientists and technologists, including politically dangerous thinking. The Sociobiology Study Group was dominated by Marxist and New Left scholars from Harvard. Two of the most prominent, Stephen Jay Gould and Richard Lewontin, were my close colleagues and fellow residents of the Museum of Comparative Zoology. Three others, Jonathan Beckwith, Ruth Hubbard, and Richard Levins, held faculty posts in other parts of the university.

Although the unofficial headquarters of the Sociobiology Study Group was Lewontin's office, located directly below my own, I was completely unaware of its deliberations. After meeting for three months, the group arrived at its foreordained verdict. In a letter published in the *New York Review of Books* on November 13, 1975, the members declared that human sociobiology was not only unsupported by evidence but also politically dangerous. All hypotheses attempting to establish a biological basis of social behavior "tend to provide a genetic justification of the *status quo* and of existing privileges for certain groups according to class, race, or sex. Historically, powerful countries or ruling groups within them have drawn support for the maintenance or extension of their power from these products of the scientific community . . . [Such] 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."

I learned of the letter when it reached the newsstands on November 3. An editor at Harvard University Press called me to say that word about it was spreading fast and might prove a sensation. For a group of scientists to declare so publicly that a colleague has made a technical error is serious enough. To link him with racist eugenics and Nazi policies was, in the overheated academic atmosphere of the 1970s, far worse. But the self-proclaimed position of the Sociobiology Study Group was ethical, and therefore implicitly beyond challenge. And the purpose of the letter was not so much to correct alleged technical errors as to destroy credibility.

In the liberal dovecotes of Harvard University, a reactionary professor is like an atheist in a monastery. As the weeks passed and winter snows began to fall, I received little support from the Harvard faculty. Several friends spoke up in interviews and public radio forums to oppose Science for the People. They included Ernst Mayr, Bernard Davis, Ralph Mitchell, and my close friend and collaborator Bert Hölldobler. But mostly what I got was silence, even when the internal Harvard dispute became national news. I know now after many private conversations that the majority of my fellow natural scientists on the Harvard faculty were sympathetic to my biological approach to human behavior but confused by the motives and political aims of the Science for the People study group. They may also have thought that where there is smoke, there is fire. So they stuck to their work and kept a safe distance.

I had been blindsided by the attack. Having expected some frontal fire from social scientists on primarily evidential grounds, I had received instead a political enfilade from the flank. A few observers were surprised that I was surprised. John Maynard Smith, a senior British evolutionary biologist and former Marxist, said that he disliked the last chapter of *Sociobiology* himself and "it was also abso-

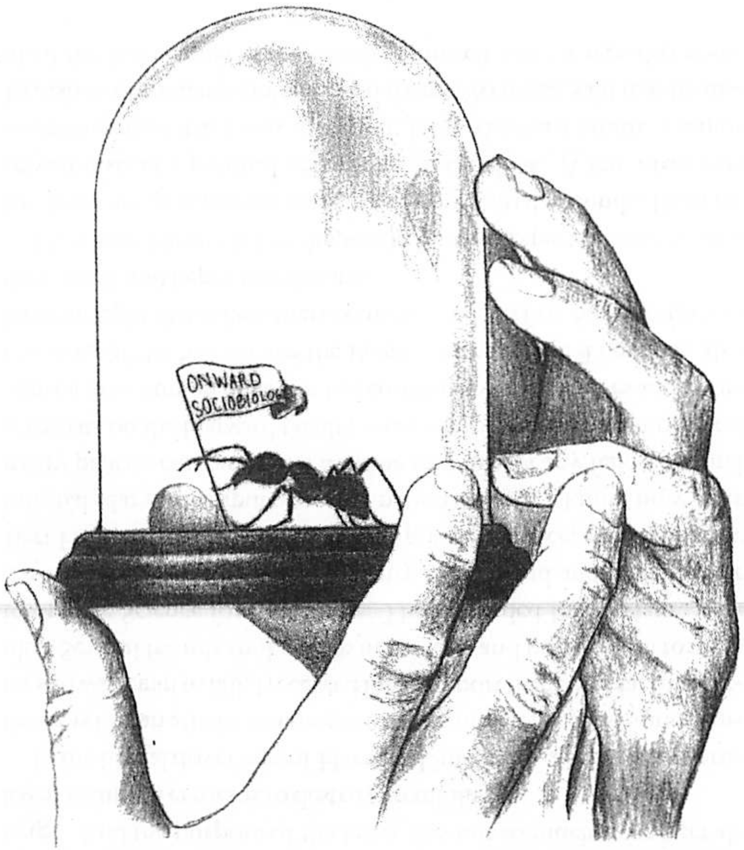
lutely obvious to me—I cannot believe Wilson didn't know—that this was going to provoke great hostility from American Marxists, and Marxists everywhere."* But it was true. I was unprepared perhaps because (as Maynard Smith further observed) I am an American rather than a European. In 1975 I was a political naïf: I knew almost nothing about Marxism as either a political belief or a mode of analysis, I had paid little attention to the dynamism of the activist left, and I had never heard of Science for the People. I was not even an intellectual in the European or New York–Cambridge sense.

Because of my respect for the members of the Sociobiology Study Group I knew personally, I was at first struck by self-doubt. Had I taken a fatal intellectual misstep by crossing the line into human behavior? The indignant response of the Sociobiology Study Group stood in shocking contrast to the near silence of the other biologists in my department, who failed to offer even casual encouragement during corridor talk. My morale was not helped by the fact that Dick Lewontin, the most outspoken of the critics, was also chairman of the department. I faced the risk, I thought, of becoming a pariah—viewed as a poor scientist and a social blunderer to boot.

Then I rethought my own evidence and logic. What I had said was defensible as science. The attack on it was political, not evidential. The Sociobiology Study Group had no interest in the subject beyond discrediting it. They appeared to understand very little of its real substance.

As my mind settled on the details, anger replaced anxiety. I penned an indignant rebuttal to the *New York Review of Books*. In a few more weeks anger in turn subsided and my old confidence returned, then a fresh surge of ambition. There was an enemy in the field. An important enemy. And a new subject—which, for me, meant opportunity.

*Quoted in Ullica Segerstråle, "Whose Truth Shall Prevail? Moral and Scientific Interests in the Sociobiology Controversy" (Ph.D. diss., Department of Sociology, Harvard University, 1983).



I set out to learn the elements of Marxism. I was encouraged in my amateur's effort by Daniel Bell, the distinguished sociologist, and Eugene Genovese, a leading Marxist philosopher. Neither of them cared very much for sociobiology, but they disliked even more the aggressive tactics of Science for the People. I expanded my reading into the social sciences and humanities. I acquired a taste for the history and philosophy of science. Two years after the Sociobiology

Study Group published their letter, I wrote *On Human Nature*, which won the 1979 Pulitzer Prize for General Nonfiction (granted, a literary award and not scientific validation). The following year I began an all-out attempt to build a stronger theory to explain the interaction between genetic and cultural evolution.

The sociobiology controversy, I came to realize, ran deeper than ordinary scholarly discourse. The signatories of the Science for the People letter had come to the subject with a different agenda from my own. They viewed science not as separate objective knowledge but as part of culture, a social process compounded with political history and class struggle.

The spirit of their exertions was most clearly embodied, I believe, in the person of Richard C. Lewontin. He was later overshadowed by the scientific and literary celebrity of Stephen Jay Gould, but in 1975 the two men were equally well known and of mostly common political opinion. Gould shared Lewontin's Marxist approach to evolutionary biology, and he afterward maintained a drumfire of criticism in his monthly *Natural History* column and essays published elsewhere. But it was Lewontin who explored more deeply and thoroughly than anyone else every level of the implications of human sociobiology. He was the principal author of the letter in the *New York Review of Books*. Afterward he gave the greatest number of lectures opposing sociobiology, drawing on his extensive knowledge of genetics and the philosophy of science. He devoted the greatest amount of time to rallying opposition among potential converts, and his vigilance never slipped. If there is a truly fatal flaw in the sociobiology argument, he will have explicated it somewhere.

Without Lewontin the controversy would not have been so intense or attracted such widespread attention. He was the kind of adversary most to be cherished, in retrospect, after time has drained away emotion to leave the hard inner matrix of intellect. Brilliant, passionate, and complex, he was stage-cast for the role of contrarian. He possessed a deep ambivalence that kept both friend and foe off balance: intimate in outward manner, private inside; aggressive and demanding constant attention, but keenly sensitive, anxious to humble and to please listeners at the same time; intimidating yet easily set back on his heels by a strong response, revealing a fleeting angry confusion that made one—almost—wish to console him. Robert MacArthur told me, when we three were young men, that Lewontin was the only person who could make him sweat.

Unafflicted by shyness, at committee meetings he almost always seated himself near the head or center of the conference table, speaking up more frequently than others present, questioning and annotating every subject raised. He was the boy prodigy you surely encountered at least once in school, the first to raise his hand, the first reaching the blackboard to crack the algebra problem. His youthful demeanor was preserved into middle age by a round face, easy grin, and knowing stare, a shock of unruly dark hair, and a tieless shirt, always blue, said by amused friends to advertise his solidarity with the working class. Journalists referred to his countenance as owlish, but that was true only in freezeframe. Lewontin was too nervous and active in real life for the strigid image to fit.

He would pivot from one role to another, first the thoughtful and cautious dean, now the lecturer expanding a philosophical idea, then the hearty joking companion, and abruptly, on occasion, the angry radical. To accentuate a point, he would raise his hands above his head with fingers opened, and as his voice evened out and the argument unfolded, slide them back to the table top palms down, at first placed side by side and then eased apart, the mood having turned reflective, then quickly up again to chest level and windmilled one around the other, the subject grown more complex and the listener thereby commanded to pay close attention. He spoke in complete sentences and paragraphs. The stream of words was punctuated at intervals by a slowing delivery, sometimes almost a slurring, to reinforce a key phrase and, finally, the approach of the concluding argument. While he spoke he turned about to make eye contact with each listener

within range, flashing the grin, signaling a confidence in his choice of words, revealing an attention to technique as well as to substance.

His self-confidence and style were potent in the academy of the 1960s and 1970s. It was the era when students clamorously asserted their independence and at the same time searched desperately for leaders. Lewontin's lectures at Harvard and abroad were enthusiastically received. His antiestablishment barbs, delivered with the panache of a stand-up comedian, were marvelously witty, even when you happened to be the target; they drew dependable laughter. Here was a scientist, the students knew, and a thinker, drawing from a deep revolutionary wellspring. He impressed journalists, too, who commonly referred to him as "the brilliant population geneticist." Lewontin was an intellectual who preached social change from the temple of hard science.

His scientific credentials were beyond challenge. His genetic research was of the highest caliber. In the mid-1960s, while at the University of Chicago, he collaborated with J. L. Hubby to make the first estimates of gene diversity within populations by means of the electrophoretic separation of closely similar proteins. Their technique soon became standard and inaugurated a new era of quantitative studies in evolutionary biology. He was also one of the first to use computers to study the role of chance in microevolution. Striking out from the same base of expertise, he explored the border area between genetics and ecology by linking the evolution of demography to changes in the rate of population growth.

Very early, at the age of thirty-nine, Richard Lewontin was elected to the National Academy of Sciences, one of the highest honors in American science. Then the contrarian side of his nature emerged. In 1971, amid verbal fireworks, he resigned in protest over the Academy's sponsorship of classified research projects for the Department of Defense. He was one of only twelve members out of the thousands elected during the 130-year history of the organization to quit it for any reason. He had placed himself in distinguished com-

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pany; the others included Benjamin Peirce, William James, and Richard Feynman.

In the early spring of 1972 a Harvard committee, of which I was a member, recommended to the Department of Biology that Richard Lewontin be offered a full professorship. He was at that time considered the best population geneticist of his generation in the world. Under ordinary circumstances the appointment would have received quick approval and been passed on to the dean and president; but circumstances were no longer normal. Dick by that time was more than just a leading scientist. He had also become a political activist targeting other scientists. At the 1970 annual meeting of the American Association for the Advancement of Science he had been one of a small group who disrupted a session on a politically sensitive topic.

Several of the senior professors, alarmed by what they saw as a trend in his personality, were prepared to vote against Lewontin's candidacy. Wouldn't he be disruptive in his own department, they asked, if brought to Harvard? At the critical meeting of the tenured professors Ernst Mayr and I defended him. We argued (rather stuffily it seems in retrospect) that political beliefs should not influence faculty appointments. Some of the members remained unpersuaded: beliefs are one thing, they said, but what about personal attacks and disruption? I badly wanted Lewontin to come to Harvard. I said, let me call a friend in his department at the University of Chicago and ask if Dick has attacked his own colleagues there on ideological grounds. The proposal was accepted, and the decision postponed. In the interim George Kistiakowsky, one of Harvard's most respected senior professors and wise adviser to the university administration, got wind of the proceedings and telephoned me from the Department of Chemistry. He said in effect, you're going to be sorry if Lewontin comes. I was committed; I made my own call and was assured that Lewontin had not created problems at the University of Chicago. At the next meeting we voted unanimously to recommend

him for a professorship. President Derek Bok approved his appointment on November 8, 1972, and the following year he came to Harvard.

Once he was installed, and increasingly after the sociobiology controversy began, I realized that we were opposites in our views of the proper conduct of science. Lewontin was the philosopher-scientist, tightly self-constrained, critical at every step, a stern guardian of standards who opposed—indeed, would have banned, if given the opportunity—plausibility arguments and speculation. I was the naturalist-scientist, in agreement on the need for strict logic and experimental testing but expansive in spirit and far less prone to be critical of hypotheses in the early stages of investigation. A collector and pragmatist by lifelong experience, I believed that every scrap of information and reasonable hypothesis should be put on record, then kept or discarded as knowledge grows. My notebooks were an indiscriminate hodgepodge. To be restrictive in the early stages, to make a moral issue of plausibility arguments, was in my view antithetical to the spirit of science. I wanted to move evolutionary biology into every potentially congenial subject, roughshod if need be, and as quickly as possible. Lewontin did not.

By adopting a narrow criterion of publishable research, Lewontin freed himself to pursue a political agenda unencumbered by science. He adopted the relativist view that accepted truth, unless based upon ineluctable fact, is no more than a reflection of dominant ideology and political power. After his turn to activism he worked to promote his own accepted truth: the Marxian view of holism, a mental universe within which social systems ebb and flow in response to the forces of economics and class struggle. He disputed the idea of reductionism in evolutionary biology, even though it was and is the virtually unchallenged linchpin of the natural sciences. And most particularly, he rejected it for human social behavior. "By reductionism," he wrote in 1991, "we mean the belief that the world is broken up into tiny bits and pieces, each of which has its own properties and which combine together to make larger things. The individual makes society, for example, and society is nothing but the manifestation of the properties of individual human beings. Individual properties are the causes and the properties of the social whole are the effects of those causes."*

This reductionism, as Lewontin expressed and rejected it, is precisely my view of how the world works. It forms the basis of human sociobiology as I construed it. But it is not science, Lewontin insisted. And according to his own political beliefs, expressed over many years, it could not possibly be true. "This individualistic view of the biological world is simply a reflection of the ideologies of the bourgeois revolutions of the eighteenth century that placed the individual at the center of everything."† Lewontin sought instead laws that were transcendent, beyond the reach of natural science. "There is nothing in Marx, Lenin, or Mao," he wrote in collaboration with Richard Levins, "that is or can be in contradiction with the particular physical facts and processes of a particular set of phenomena in the objective world."‡ Only antireductionist, nonbourgeois science would help humanity attain the ultimate, highest goal, a socialist world.

That a distinguished scientist could advocate an approach to science guided by a radically sociocultural version of Marxism in the service of world socialism may seem odd today, and perhaps most of all in the former republics of the Soviet Union. But it helps to explain the distinctive flavor of the controversy at Harvard in the 1970s. In the standard leftward frameshift of academia prevailing then, Lewontin and members of Science for the People were classified as progressives, admittedly a bit extreme in their methods, while I—

*Richard C. Lewontin, *Biology as Ideology: The Doctrine of DNA* (New York: HarperPerennial, 1991), p. 107.
†Ibid.
‡R. C. Lewontin and R. Levins, "The Problem of Lysenkoism," in Hilary Rose and Steven Rose, eds., *The Radicalisation of Science* (London: Macmillan, 1976), pp. 34, 59.

Roosevelt liberal turned pragmatic centrist—was cast well to the right.

After the Sociobiology Study Group exposed me as a counterrevolutionary adventurist, and as a result of it, other radical activists in the Boston area conducted a campaign of leaflets and teach-ins to oppose human sociobiology. As this activity intensified through the winter and spring of 1975–76, I grew fearful that it might reach a level embarrassing to my family and the university. I briefly considered offers of professorships from three universities—in case, their representatives said, I wished to leave the physical center of the controversy. But it all came to very little. For a few days a protester in Harvard Square used a bullhorn to call for my dismissal. Two students from the University of Michigan invaded my class on evolutionary biology one day to shout slogans and deliver antisociobiology monologues. When it became apparent that they had not read *Sociobiology* and were more interested in using it as a stick to beat the Harvard ruling class, they were heckled by my own students. I received almost no hate mail, and never a death threat.

The most dramatic episode was the water dousing in Washington in 1978. On February 15 I arrived at the Sheraton Park Hotel to speak at a symposium on sociobiology planned as part of the annual meeting of the American Association for the Advancement of Science. The largest organization of scientists in the world, the AAAS was and remains especially concerned with the relation of science to education and public policy. A large crowd was expected at the symposium, which featured a half-dozen of the principal researchers on human sociobiology, as well as one of its most articulate critics, Stephen Jay Gould.

The moderator was to be Margaret Mead, and I looked forward to meeting her for the second time. A year before, at a conference on human behavior in Virginia, she had invited me to have dinner with her to discuss sociobiology. I was nervous then, expecting America's mother figure to scold me about the dangers of genetic determinism.

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I had nothing to fear. She wanted to stress that she, too, had published ideas on the biological basis of social behavior. One was that each society contains an array of people genetically predisposed toward different tasks, say artist or soldier, and this differentiation creates a more efficient division of labor. Over roast beef and red wine (I was too mesmerized by her presence to taste either) she recommended several of her own writings that she thought I might want to read.

Sadly, I was not to see her again. Shortly before the AAAS meeting, she was stricken with the cancer that would soon take her life.

As the time approached for the symposium to begin, the atmosphere in and around the meeting hall grew tense. I was told that some kind of demonstration was planned by the International Committee Against Racism (INCAR), a group known for violent action. Its leaders, on learning that a session on human sociobiology was scheduled and that I would be present, had alerted members throughout the country. On hearing this news I walked by the INCAR booth to collect the literature they were distributing and to pick up a lapel button. As the crowd of several hundred began to settle in the nearby lecture hall, two INCAR members moved about distributing copies of a protest leaflet. I reached for one, but the young woman offering it recognized me and snatched it away.

Nothing happened as the substitute moderator, Alexander Alland, Jr., an anthropologist from Columbia University, opened the session and several other speakers presented their papers. When my turn came I chose to stay in my seat rather than stand at the lectern; my right leg was in a cast from an ankle fracture incurred while jogging over black ice two weeks previously. As soon as I was introduced, about eight men and women—I never managed an exact count—sprang from their seats in the audience, rushed onto the stage, and lined up behind the row of speakers. Several held up anti-sociobiology placards, on at least one of which was painted a swastika. A young man walked to the lectern to take the microphone

away from Alland. AAAS officials had earlier issued instructions to session chairs to surrender their microphones if demonstrators demanded them, to avoid physical scuffling, and then to inform the protestors that if the microphones were not returned within two minutes, hotel security would be called. Alland announced that he was following the AAAS official procedure and turned over the microphone. Meanwhile, some of the members of the audience, fearing a riot, began to move out of their seats and away from the stage. They made little progress, however, because all the seats were filled and the aisles were crowded. Napoleon Chagnon, seated in a middle row, struggled to move the other way, determined to reach the stage and eject the protestors, but his way was also blocked. With several other audience members he shouted back at Alland and the protestors: the surrender of the microphone was wrong; no group should be allowed to take over a session by force. But this was the era of parity and equivalence, and every form of expression was considered free speech. The crowd began to settle down.

Then, as the INCAR leader harangued the audience, a young woman behind me picked up a pitcher of water and dumped the contents on my head. The demonstrators chanted, "Wilson, you're all wet!" In a little over two minutes they left the stage and took their seats. No one asked them to leave the premises, no police were called, and no action was taken against them later. After the symposium, several stayed behind to chat with members of the audience.

As I dried myself off with my handkerchief and a paper towel someone handed me, Alland, in possession of the microphone again, expressed his regret to me for the incident. The audience then gave me a prolonged standing ovation. Of course they did, I thought. What else could they do? They might be next. Before I could proceed with my brief lecture, other members of the panel rose to condemn the INCAR action. Steve Gould seemed to be speaking to the demonstrators when he quoted Lenin on the inappropriateness of violence for mere radical posturing, as opposed to the attainment of worthy political goals. Gould referred to the AAAS incident, using Lenin's words, as an "infantile disorder" of socialism. In that he was correct. It was the grown-up intellectuals I knew I had to worry about.

How did I feel during the incident? Calm—dare I say icy cold, as I let the protestors' anger wash over me? That evening I joined Napoleon Chagnon for dinner and then debated Marvin Harris on human sociobiology at the Smithsonian Institution, with another large audience in attendance—no takeover by radicals this time. Afterward I taxied to Union Station to catch the Night Owl sleeper to Boston. There I ran into the physicist Freeman Dyson, who was on his way home to Princeton. Well, I said, I've had quite a day. I had water dumped on me by protestors at the AAAS sociobiology symposium. Well, he said, I've also had quite a day. I was just in a train wreck. The engine had derailed a few miles north of Washington and the passengers had been ferried back to the station to await a later northbound train.

By this time it was obvious to me that human sociobiology would remain in trouble, both intellectually and politically, until it incorporated culture into its analyses. Otherwise the critics could always cogently argue that since semantically based mind and culture are the defining traits of the human species, explanations of human social behavior without them are useless. This shortcoming was on my mind when Charles Lumsden, a young theoretical physicist from the University of Toronto, arrived in early 1979 to work with me as a postdoctoral research fellow. His interests had lately turned to biology, and he saw great opportunity in the analysis of social behavior. We talked at first about a collaboration on social insects, but soon our conversation gravitated to the subject of heredity and culture. I said, the possible payoff justifies the high risk of failure; let's give it a try. So two or three times a week for eighteen months we sat together and framed the subject piece by piece.

We reasoned as follows. Everyone knows that human social be-

havior is transmitted by culture, but culture is a product of the brain. The brain in turn is a highly structured organ and a product of genetic evolution. It possesses a host of biases programmed through sensory reception and the propensity to learn certain things and not others. These biases guide culture to a still unknown degree. In the reverse direction, the genetic evolution of the most distinctive properties of the brain occurred in an environment dominated by culture. Changes in culture therefore must have affected those properties. So the problem can be more clearly cast in these terms: how have genetic evolution and cultural evolution interacted to create the development of the human mind?

No doubt we went out of our depth in embarking upon this subject. But so was everyone else, and no one can be sure of anything until the attempt is made. Undaunted then, we sifted through a small mountain of literature in cognitive psychology, ethnography, and brain science. We built models in population genetics that incorporated culture as units of learned information. We studied the properties of semantic thought to make our premises as consistent as possible with current linguistic theory.

We were looking for the basic process that directed the evolution of the human mind. We concluded that it is a particular form of interaction between genes and culture. This "gene-culture coevolution," as we called it, is an eternal circle of change in heredity and culture. Over the course of a lifetime, the mind of the individual person creates itself by picking among countless fragments of information, value judgments, and available courses of action within the context of a particular culture. More concretely, the individual comes to select certain marital customs, creation myths, ethical precepts, modes of analysis, and so forth, from among those available. We called these competing behaviors and mental abstractions "culturgens." They are close to what our fellow reductionist Richard Dawkins conceived as "memes."

Each time an individual modifies his memories or makes decisions, he entrains intricate sequences of physiological events that run first from the perception of visual images, sounds, and other stimuli, then to the storage and recall of information from long-term memory, and finally to the emotional assessment of perceived objects and ideas. Not all culturgens are treated equally; cognition has not evolved as a wholly neutral filter. The mind incorporates and uses some far more readily than others. Examples of heredity-bound culture that Lumsden and I found from the research literature include the peculiarities of color vision, phoneme formation, odor perception, preferred visual designs, and facial expressions used to denote emotions. All are diagnostic of the human species, all part of what must reasonably be called human nature.

Such physiologically based preferences, called "epigenetic rules," channel cultural transmission in one direction instead of another. By this means they influence the outcome of cultural evolution. It is here, through the physical events of cognition, that the genes act to shape mental development and culture.

The full cycle of gene-culture coevolution as we conceived it is the following. Some choices confer greater survival and reproductive rates. As a consequence, certain epigenetic rules, those that predispose the mind toward the selection of successful culturgens, are favored during the course of genetic evolution. Over many generations, the human population as a whole has moved toward one particular "human nature" out of a vast number of natures possible. It has fashioned certain patterns of cultural diversity from an even greater number of patterns possible.

Lumsden and I presented our scheme in several technical articles and two books.* The reviews were mixed; some were enthusiastic, but those in several key journals were unfavorable: Edmund Leach

*C. J. Lumsden and E. O. Wilson, *Genes, Mind, and Culture* (Cambridge, Mass.: Harvard University Press, 1981) and *Promethean Fire* (Cambridge, Mass.: Harvard University Press, 1983). The summary of the theory of gene-culture coevolution presented here is drawn, with minor changes, from our article "Genes, Mind, and Ideology," *The Sciences* 21, no. 9 (1981): 6–8.

was enraged in *Nature*; Peter Medawar was contemptuous in the *New York Review of Books*; Richard Lewontin, by his own later description, was nasty in *The Sciences*. The subject of gene-culture coevolution simply languished, mostly ignored by biologists and social scientists alike. I was worried, and puzzled. The critics really hadn't said much of substance. Had we nevertheless failed at some deep level they saw but we failed to grasp? During the 1980s a handful of other researchers investigated the subject along conceptual pathways of their own devising. Gifted scientists with diverse expertise from genetics and anthropology, they included Kenichi Aoki, Robert Boyd, Luigi Cavalli-Sforza, William Durham, Marcus Feldman, Motoo Kimura, and Peter Richerson. They too met with only limited success, at least as measured by the spread and advance of the total research enterprise. Kimura, Japan's foremost geneticist, told me that he had received almost no requests for his article on the subject.

It is possible that gene-culture coevolution will lie dormant as a subject for many more years, awaiting the slow accretion of knowledge persuasive enough to attract scholars. I remain in any case convinced that its true nature is the central problem of the social sciences, and moreover one of the great unexplored domains of science generally; and I do not doubt for an instant that its time will come.