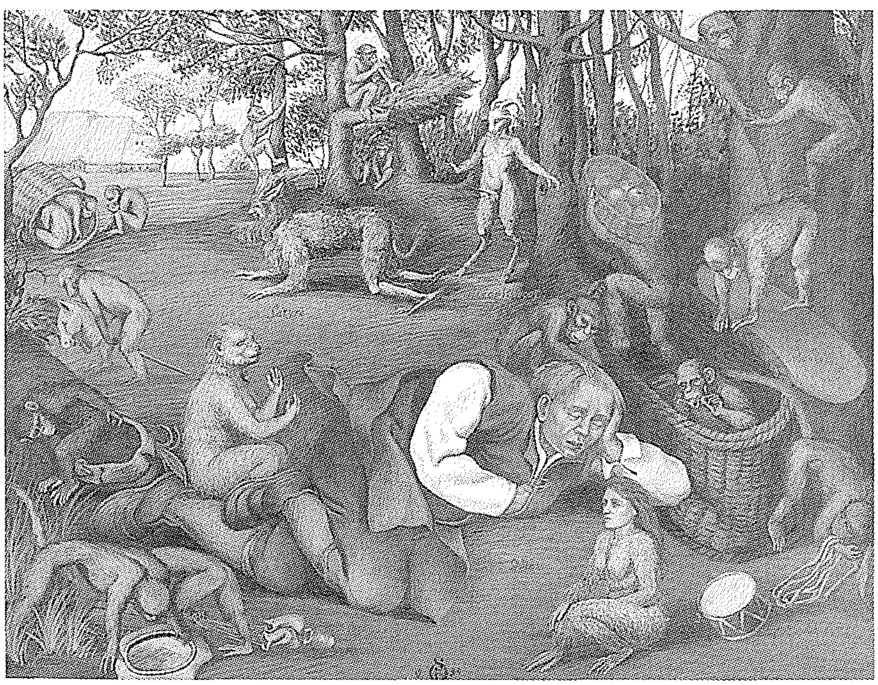
THE RETURN OF HUMAN NATURE

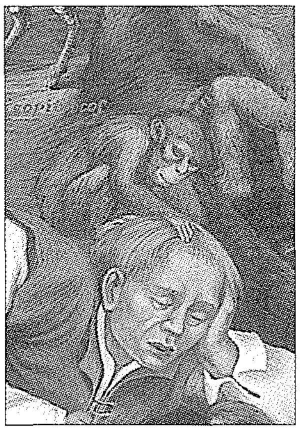
Since World War II, the academy has been the site of more than a few heated intellectual debates. None has been more passionate than the one set off by efforts to apply the bio-evolutionary perspective to human behavior. Even while provoking vicious criticism, the new applications of Darwinian principles—whether called sociobiology, biosociology, or evolutionary psychology—have shed valuable, and appreciated, light on everything from violence to sexist practices. The debate, however, is far from over. The very notion of an underlying human nature flies in the face of contemporary postmodernist theories held dear by many intellectuals and artists. Here we offer a history of the modern human nature debate, as seen by two participants.



My Life in the Human Nature Wars

by Lionel Tiger

For venturing to explore the role of biology in our social lives, I have had more than my share of interesting moments. In addition to slander and calumny—depressingly standard fare in the academy today—I have received bomb threats at lectures in Vancouver and Montreal and the promise of a "kneecapping" at the New School for Social Research in New York. I have been the object of a demonstration of angry male transvestites at the Royal Institution in London, and I have seen one of the books I co-authored, *The Imperial Animal,* compared to *Mein Kampf*! All in a day's work, you might say, though some 35 years' is closer to the truth.



If the toll exacted by my career has occasionally been steep, it has been well worth the price to be able to participate in the most consequential intellectual debate of our time, a debate that goes back at least to Charles Darwin and the mid-19th-century publication of his magnificent and scandal-provoking theory of natural selection.

The main antagonists then were scientists and clerics. The former thought Darwin's theory explained a great deal about nature and possibly even human nature. The latter considered it a rebuke to stories of divine creation as well as a potential threat to their power to define reality. But in recent years, the argument over the influence of biology on human society has been far more raucous within science itself, particularly within the social sciences.

The evolving "biosocial" view that I have helped pioneer poses a direct challenge to some of the premises of 20th-century social science—and by extension, the cherished beliefs of many intellectuals and reformers. Foremost among these is the assumption that human beings and their institutions have largely transcended the biological constraints that govern the animal world, and, accordingly, that humans are all but free to make the worlds they choose.

I had not originally set out for such contentious territory. In fact, I took only the most conventional (that is, biology-free) courses toward my first two degrees at McGill University in Montreal, where I had been born and raised in the Jewish quarter immortalized by Mordecai Richler's novels. Perhaps the closest I came to biology in my childhood were the featured herring in my father's small grocery. Their immodest aroma joined with the waxing and waning of items in the produce section to alert me to the facts of seasonality and the reality of genuine physical decay. The one biology course McGill demanded I take, complete with ritual dissection of frog limbs and organs, confirmed my lack of interest in nonhuman life forms. At the time (the late 1950s), my energies were far more strongly directed toward student journalism and the local literary and political scenes, which included such figures of later fame as Leonard

Cohen and Pierre Trudeau.

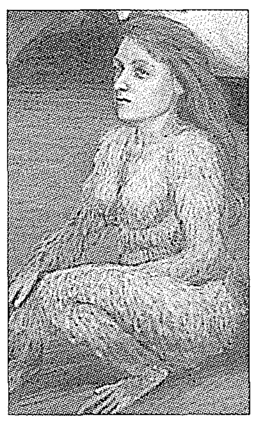
After completing my master's degree at McGill with a thesis on the links between scientists and administrators in a research institute, I enrolled at the London School of Economics and turned to doctoral work on decolonization in Africa, a process I had witnessed earlier on a summer fellowship to Ghana and Nigeria. The focus of my research in 1960—the colonial service of Ghana as it became the newly independent nation's civil service—came with a bonus: it allowed me to study the colorful Kwame Nkrumah, Ghana's first president and a seminal figure in postcolonial African history.

What I specifically wanted to determine was whether Max Weber's theory of the "routinization of charisma"—the process by which the almost magical power of the great leader is subtly but decisively transformed into the mechanisms of bureaucratic authority—applied to the political realities of newly independent Ghana. My research led me to a phrase in Weber's work that presumably reflected his desire to see sociology become an authoritative science. It is at the same time a surprising comment given the rest of his scholarship, and remains almost wholly ignored by those who mine his work. Weber wrote that charisma was especially difficult to understand and that "within the narrow limits of sociology" was comprehensible only "in its imperceptible transition to the biology."

Why, I wondered, was one of the founding fathers of sociology conceding so much ground to biology? I was intrigued for two reasons. First, the differences between Canadians and Ghanaians struck me as far less interesting and important than their similarities. Second, in West Africa in 1960–61, I became aware of the work of such figures as Raymond Dart and Louis Leakey then underway in southern Africa concerning hominid fossils and what they implied about our longevity as a species. It appeared we were a much older species than we had thought. Not only that, the breaking of the DNA codes in the early 1950s provided a way of understanding how very complex information about living systems could be passed from generation to generation.

Natural science seemed to be throwing up other teasing clues. Emergent long-term research in East Africa on primates in the wild revealed the complexity of their social systems. Just as William Foote Whyte in his extraordinary *Street Corner Society* (1943) had shown the previously overlooked intricacy of social life in an American working-class neighborhood, so primatologists such as John Crook and Jean and Stuart Altmann now identified rules and patterns behind primate hierarchies, matrilineal groups, socialization, and sexuality. And as primatologists became more sophisticated in their research techniques, they became increasingly aware of the importance of individual differences among animals of the same species. Suddenly, almost as if in a thrilling conspiracy, science was offering us an unexpected insight into nonhuman social complexity and the existence of "personality differences" among individual animals.

Here was a fundamental challenge to the accepted wisdom of social scientists. The dominant orthodoxy of the time was that only humans displayed ongoing and intelligent agency as opposed to the reflexive "instinctive" behavior of animals. Humans could fashion immensely variable and sophisticated social systems, but other species could sustain only relatively automatic patterns of group behavior. This remarkably rigid system of intellectual apartheid went almost completely unexamined. No major doctoral program in social science required or even encouraged its students to become familiar with the lives and systems of other species. To the contrary, the formal distinction between natural and social science was seen as self-evidently correct. And the implication was



that somehow social behavior was not natural and could not be analyzed with the same lens used to inspect other animals.

But new questions threatened the old boundaries. Was there a common human nature? Could we return to the concerns of the 19th century about that nature? What did it mean that there appeared to be a natural substrate, rooted in genetics, of complex animal social behavior? Did this substrate also extend to humans? Or did our kind of DNA, combined with the rich tapestry of our culture, secure us a fully distinct and privileged exemption from the rules governing the rest of nature?

A book that pulled much of this inquiry together in a lively but honest way was Robert Ardrey's *African Genesis* (1961), which I devoured when I laid hands on it in London in 1962. Ardrey was a Chicago-born playwright and screenwriter who, after a Broadway failure, sought solace in a *Life* magazine assignment that took him to southern Africa to learn about the archaeology and biology that was beginning to attract the attention of a few alert scientists. His path-breaking book influenced many people looking for new directions in biology and social science, and in their relationship. Ardrey's accomplishment, unique at the time, was to integrate findings in studies of human evolution, animal behavior, and the long archaeological and historical record. Not all readers were thrilled by the resulting synthesis. Some social scientists strongly objected to its emphasis on the role of aggression in evolution and its challenge to the then-orthodoxy that *Homo sapiens* originated in Asia. "Not in innocence, and not in Asia, was Man born" was Ardrey's defiant opening line.

Inspired by Ardrey's boldness and cogency, I finished my thesis on bureaucracy and charisma in Ghana and included Weber's note about charisma and biology in the concluding chapter. The members of my thesis committee, distinguished social scientists all, approved my thesis for publication on the condition that I remove some "offensive" pages dealing with Weber's "lapse" about charisma and a few others in which I discussed primate political systems and the potential role of biology in social science. Though it was an unexpected irritation, the committee's censorship was a clue to something rotten in the state of scholarship.

What I had come up against, I later realized, was the hegemony of behaviorism. A doctrine with deep and varied roots, it goes back at least as far as John Locke's notion that human beings begin their mental lives as blank slates and are formed, morally and socially as well as intellectually, by the sum of all subsequent sense impressions. By this logic, environment, and environment alone, makes the human.

The doctrine acquired formal scholarly shape in the early 20th century, notably in the work of the French sociologist Émile Durkheim. His *Rules of Sociological Method* (published in English in 1938) established the unacceptability of using a biological or even a psychological explanation for social behavior when a sociological one would do. To violate this principle was to succumb to "reductionism," the supreme sin in Durkheim's catechism.

In the United States, the most forceful advocates of the doctrine were the social scientists Franz Boas and John T. Watson. Emphasizing the principle of cultural relativism, they pointed to the enormous variety of existing social patterns as proof that nearly any other social arrangement was possible as well. From his base at Columbia University, Franz Boas introduced a generation of anthropologists, including Ruth Benedict and Margaret Mead, to the orthodox view.

But the triumph of behaviorism was more than an intellectual matter. After Nazism tainted all efforts to bring genetics and other biological considerations into the study of human beings and their collective lives, the behaviorist position occupied the moral high ground as well. The ambient liberal progressivism of the academy in the early postwar period all but assured the dominance of the doctrine.

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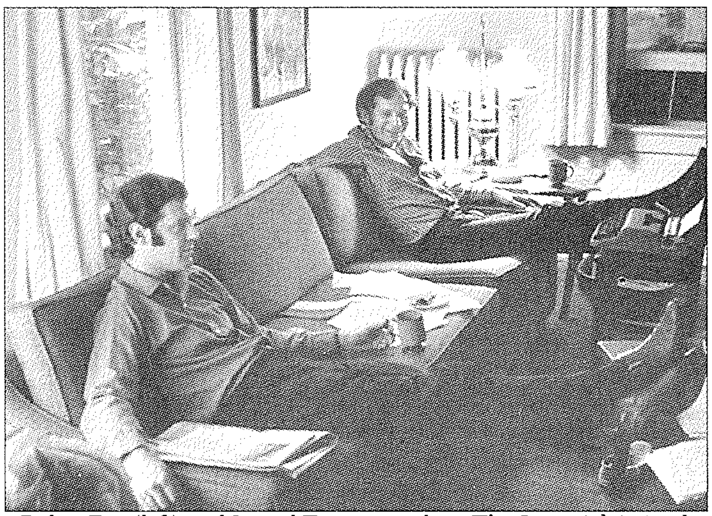
Despite my thesis committee's best efforts to keep me on the straight and narrow, my interest in the potential uses of the biological perspective did not wane when I took my first job, teaching political sociology at the University of British Columbia, in 1963. I was still eager to connect with that invisible college of scholars—Konrad Lorenz, Raymond Dart, and Sherwood Washburn, among others—to whose work Ardrey's book had alerted me.

In 1965, I had the opportunity to work directly with some of this college, including one of the brighter young lights, the anthropologist Robin Fox, who would become a close friend, colleague, and collaborator. The occasion was a symposium organized by Julian Huxley at the Zoological Society of London. I had been invited to be social scientist in residence by Desmond Morris, then still curator of mammals. Shortly after being introduced, Fox and I withdrew to his office at the London School of Economics and, after a few days' discussion, penned a brief and impudent paper on the deadness of most social science and the vitality of contemporary biology. Our proposed solution to this state of affairs was to bring the disciplines together. "The Zoological Perspective in Social Science" appeared a few months later in *Man: Journal of the Royal Anthropological Institute*. But a paper seemed inadequate to the severity and scope of the problem. We resolved one day to take up the subject in a book.

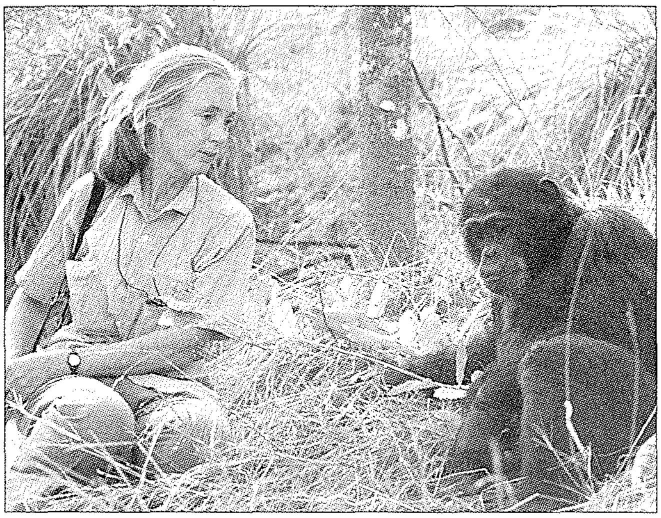
Back in British Columbia, I returned to a question that had earlier captured my attention: sex and politics. As a graduate student, I had read Simone de Beauvoir's *Second Sex* (1953) and been completely convinced by its argument that inequalities between men and women were thoroughly entrenched throughout Western society. Turning to research in my own field, I found the reigning theories on discrimination and antifemale bias only partly convincing.

Far more promising, I thought, was the work of such scientists as Jane Goodall, Desmond Morris, and Irven DeVore, who were learning that other animals, including primates, had social and political systems marked by equally sharp distinctions between males and females. We knew, too, that primates sustained these divisions without benefit of the cultural conditioning considered the overwhelming cause of the human pattern. Primates had no magazines, no *Father Knows Best* on television, no cultural stereotypes, no patriarchal legal and religious systems. Furthermore, the human cross-cultural record was impressively consistent on the subject of male-female differences in political as well as other behavior. This was in fact precisely one of the major grievances expressed by early feminist writers. Did the primate data and the consistent human record suggest we had to look again for deeper causes of sexual politics?



Robin Fox (left) and Lionel Tiger at work on The Imperial Animal.

By then I had some interest in the relatively simple matter of how human males related to each other in basic ways. Virtually no research had been done with humans on the subject, even though we already knew that other primates engaged in what I called "male bonding" and that the coalitions between males were as important for politics and defense as male-female bonds were for reproduction.



Jane Goodall's work with chimpanzees helped to illuminate the complexity of primate societies—and to show that chimp behavior, both aggressive and cooperative, was not entirely different from that of humans.

(Later we would learn from Jane Goodall, Jean Altmann, and others how important female-female bonds were for social integration and stability.)

In *Men in Groups* (1969), I put forth my hypothesis about the evolutionary basis of the cross-cultural regularity of male bonds and groups. At first, the work met with an open and even receptive response—and with sales that astonished no one more than its author. Having intended it to be an academic book, I had signed a contract to write a popular version for a series edited by Alex Comfort in England. That proved unnecessary. The book took off, first in Canada and then in the United States. It made the *New York Times* best-seller list for a brief moment, was translated into seven languages, and was hailed by Robert Ardrey in *Life* as "the most creative contribution to the social sciences since David Reisman's *The Lonely Crowd*."

But there were ominous developments as well. An anthropologist reviewing the book for *Science* compared my search for a biological element in human behavior to the early Greeks' enthusiasm for that ubiquitous all-purpose substance, phlogiston. Other reactions were more directly hostile. A near-riot broke out when I appeared on *The David Frost Show* in 1969 in New York, and angry feminists staged a noisy demonstration outside *Maclean's Magazine* in Toronto when an article about *Men in Groups* appeared as the cover story of the June 1969 issue.

Despite the hue and cry, the phrase "male bonding" quickly passed into the popular discourse, possibly because it accounted for clearly observable patterns in male behavior, from weekly gatherings for bowling to Pall Mall clubs in London to the secret societies of Sierra Leone. Now it is hardly possible to read a review of a movie for the 18-to-24-year-old set without seeing the phrase, and I am told women use it to categorize irritating behavior of the men they know.

The anger left me troubled, though. I had been stunned by the sharp political reactions to the book, some delivered with almost lethal fury. Embracing a liberal political stance very common among Canadian academics, I regarded myself as a feminist. It seemed to me that the firmness and pervasiveness of obstacles women faced in human communities were serious indeed. I thought I had identified the depth of the issue, even its possible basis in an elemental primate struggle for dominance. It seemed clear to me that unusually fundamental social changes such as quota systems and remedial legislation would be necessary to achieve sexual equity in what was obviously a rapidly changing industrial system. Robin Fox thought the main resistance to the book would come from men because I had revealed one hitherto concealed source of their hegemony, one of their precious trade secrets.

But the resistance was two-headed and different. Women thought I was advertising a version of the Freudian view that biology was destiny and that therefore they should accept a barefoot-and-pregnant image of female behavior. Social scientists had been burned before by the crude connection of biology to social policy. Now they saw any effort to introduce biology into social science as a perilous echo of Nazism and a goad to potentially genocidal racism. And the work of such towering anthropological figures as Bronislaw Malinowski, Ruth Benedict, and Margaret Mead was a substantial contribution to our knowledge of human variety and a stimulus to a wholesome kind of cultural relativism that did not equate social value with economic might.

These were reasonably cautious responses to a rather large hypothesis with wide ramifications. They came from a far higher level of intellectual and scientific integrity than did the subsequent broad and vicious ideological attacks of assorted barons of politically correct and "progressive" science. The reasoned responses were the ones to which I tried to respond carefully and fairly. Above all, I tried to make clear in my teaching and writing that a diagnosis is not a recommendation and that because something "is," and is natural, is no reason that "ought" should follow. After all, my demonstration of how male bonding works could be used very profitably by women who wanted not only to understand men's organizational behavior but also to build networks to promote their own political and economic advantage. And many women have done so.

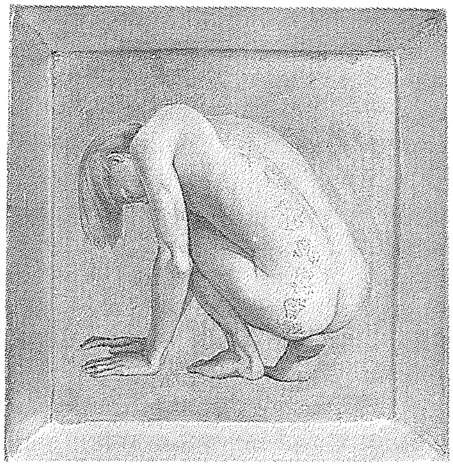
My early failure to anticipate the errors and enormities that would be imputed to my work placed me in somewhat the same position E. O. Wilson found himself after his masterful *Sociobiology* (1975) appeared. Attacked by the Marxist Science for People group, whose ranks included his Harvard University colleagues Richard Lewontin and Stephen Jay Gould, Wilson saw his work pilloried as "a genetic justification of the status quo of existing privileges for certain groups according to class, race, or sex."

His and my situations were no doubt made more difficult by the passions stirred up by the Vietnam War, passions that had largely driven civil discourse from American public life, especially from university campuses. Virtually all controversies in those years—particularly those related in any way to science, technology, and the despised technocracy—partook of the almost demonic fury that had been unleashed by a surrealistically awful war.

One's own personal political convictions made no difference. In 1971 Warren Farrell, who then worked for the National Organization for Women, asked me to debate Kate Millett, author of *Sexual Politics* (1970). I declined on the grounds that I agreed with much of Millett's agenda, but I proposed instead that we participate in a discussion. She refused. Clearly a civil intellectual exchange was not what she wanted or even thought possible with someone so far beyond the culturalist pale as was I.

If the largely political responses to my biosocial assertions were shortsighted and even narrow-minded, they were nevertheless understandable as the heartfelt response of certain agitated citizens. What was not forgivable or even comprehensible was the view expressed by social and natural scientists that the introduction of biology into human social science was, ab initio, wrong.

Evidently the law of parsimony had been repealed. Finding ever more basic explanations of causality in nature is, of course, the glory of science. But not in social science, apparently. Anyone who tried to obey the law was clearly suspect, especially when it came to sexual issues.



Detail from Atavism *(1994), by Suzanne Scherer and Pavel Ouporov*

Even before we became colleagues at Rutgers University in 1969, Robin Fox and I had agreed that this was a dangerous state of affairs, politically and scientifically. Working together in the newly created anthropology department that he chaired, we decided to start work on the book we had earlier contemplated. Fox would contribute his expertise on kinship, having written one of the classic books on the subject, *Kinship and Marriage* (1967), and I would bring in what I knew about state structures, bureaucracy, and the like.

While I can't speak for Fox, I think it is fair to characterize our approach in *The Imperial Animal* (1971) as aggressively synthetic and radical with respect to our own academic traditions. Essentially, we wanted to draw a plausible picture of human nature that accorded with materials from the study of evolution, other animals, human physiology and cognition, and the cross-cultural record.

Searching for an organizing framework, we came across Noam Chomsky's hypothesis of a "universal grammar" for language. Chomsky claimed that the necessary neural equipment for language and some of the core operating "hard-wiring" was part of the human genetic make-up. People might learn different languages, but they would do so using a common program for language with which all children are born. How else could inexperienced children acquire such phenomenal skill at such a demanding task? They could because they were born with the rules, a "universal grammar," in their heads.

We called our behavioral grammar the "biogrammar." Before publication, we sent a description of the use of his concept to Chomsky, who remarked in a warm return letter that while we had misused his minor point about deep structure, he was in accord with our approach. Furthermore, he allowed that he viewed that approach as "the only possible non-trivial approach to social science." And if language, again a relatively recent human characteristic, was linked to a biosocial substrate, it seemed all the more likely that earlier behaviors such as politics, sexuality, nurturance, and grooming were also anchored in a phylogenetic history to which the discovery of DNA had given a technical foundation.

The book was well reviewed, sold briskly, and found its way into eight other languages. (Konrad Lorenz's gratifyingly supportive introduction to the German edition was added to all subsequent editions.) Partly on its merits, Fox and I were asked by the H. F. Guggenheim Foundation to direct research support to people studying causes of violence and inequality. Because Guggenheim was almost unique among foundations in thinking such phenomena were biologically grounded, we were able to underwrite the efforts of a disparate but commonly driven group of scientists and scholars who otherwise would have gone wanting for support. It was a rewarding 12-year experience.

As exhilarating in another way was the nastiness the book occasioned. A rump assembly of radical anthropologists at its annual meeting debated the proposal of one of its constituent groups, the all-female Ruth Benedict Collective, that (1) there be no Stalinism in the women's movement and (2) that Fox and Tiger be forbidden to speak at any American campus. In the *New Statesman* in London, Maureen Duffy made the famous comparison of our book to *Mein Kampf*. The *American Anthropologist*, having received a positive review, sought another from a known opponent of our position and ran both of them—the last review of any of my publications in that journal of anthropological record. Sir Edmund Leach of Cambridge University produced a characteristically inept assessment for the British journal *New Society* in which, among other things, he accused us not only of ignoring the work of someone who had been in our department for two years but also of overlooking a relevant thesis on kibbutz incest written (the ever-solipsistic Sir Edmund failed to register) by our first Rutgers doctoral student.

Understandably, Fox and I were largely unimpressed by the quality and balance of the response we had from many social scientists. We became more convinced than ever that the issue was not the book itself but its challenge to the jurisdictional boundaries of the academy—a new version of arguments about the soul and the body.

For all the attacks, however, the book and the science it reflected were now part of the international game. The intellectual discourse was changing. Opponents of our view were moving ever more firmly away from empirical natural science and toward the chilling nihilism of poststructuralism and deconstruction, where all descriptions of reality are held to be subjective, culturally biased, and politically motivated.

The opposition was determined not only to banish the notion of objectivity but to further isolate humankind from its connections with the natural order. As Alexander Argyros of the University of Texas shrewdly observed, such scholars were creationists of a special kind: they had no God, but they had an unshakable faith in radical human exceptionalism in the scheme of nature. That faith in turn supported much of their fuzzy utopian thinking.

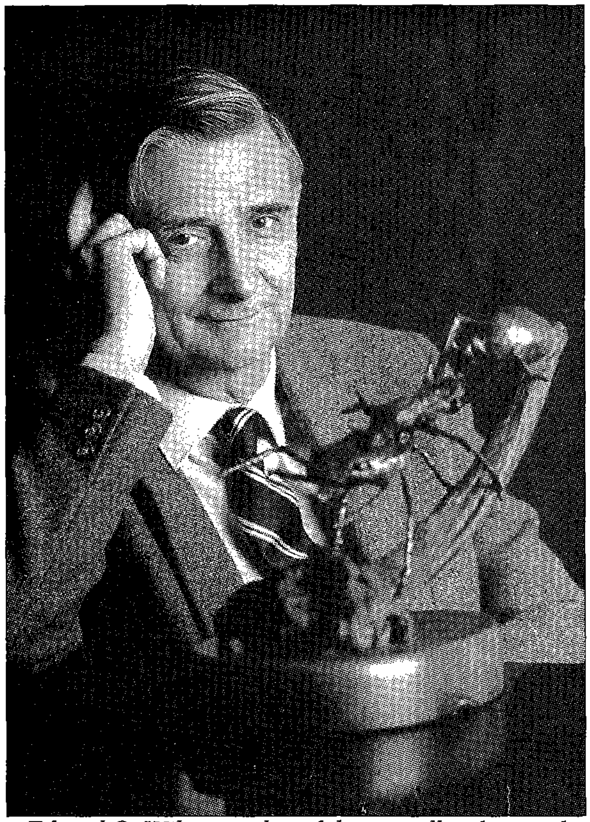
The impulse behind utopianism and other forms of idealism is not a trivial matter. I would treat it directly in a later book, *Optimism: The Biology of Hope* (1979). But I have long had a fascination with utopian schemes—a fascination tempered equally by sympathy and skepticism.

Back in my high school years in Montreal, I briefly belonged to a Labor Zionist organization that offered discussions of socialism and the building of new worlds. The group's ultimate goal was to recruit young people for collective settlements in Israel, where we would join in creating little socialist utopias far superior to our petit-bourgeois worlds. In my case, though, the proselytizing didn't take. I soon left the organization with neither drama nor regret.

Nevertheless, the kibbutz movement as a human experiment continued to intrigue me—so much so that I jumped at the chance to undertake a large-scale study of kibbutz women with Joseph Shepher of the University of Haifa, a former Rutgers doctoral student. Predicting that mammalian imperatives such as mother-infant bonding would overwhelm ideological purity, we studied three generations of men and women in two of the three federations of the kibbutz system—34,040 people in all. We possessed detailed census data on these subjects, and conducted interviews in four kibbutzim and detailed ethnography in two.

I couldn't imagine why someone had not taken on the subject before. It was the perfect venue for testing assumptions about human nature held dear by a range of ideologues. What would happen when men and women received the same income—that is, none at all? When everyone worked? When all decisions were taken by all men and women in public? When all children were raised in "children's houses" from six weeks on? When all food, laundry, and purchasing were handled by the community at large? In a word, what would happen when nearly all the fundamental conditions against which much contemporary feminist and political thought were struggling were absent?

The book that we published in 1975, *Women in the Kibbutz*, showed the division of labor by sex to be greater in the kib-



Edward O. Wilson, author of the groundbreaking and controversial Sociobiology: The New Synthesis *(1975)*

something close to a kiss of death, the *New York Times Book Review* gave it to Juliet Mitchell, whose peculiarly convoluted psychofeminism we had criticized in the book. The only feminist journal that reviewed the book dismissed it on the grounds that the kibbutz experiment was itself impure because it was conducted by Jews who—don't forget—carried the patriarchal spirit in their blood. No matter that all the kibbutzim we studied were at least agnostic and some were aggressively atheistic.

The pettiness aside, Shepher and I were far more surprised that the crucial finding of the book—that deep, very long-range, and substantial social engineering had failed to change certain fundamental sex roles—had so little impact. That revelation, so salient to what was going on at the time, was almost swept away by a tide of studies of attitudes, scales of self-esteem, and gaseous seminars about expressing human potential. Possibly the most depressing part of the adventure was the unwillingness of critics to accept that kibbutz women made conscious choices in a dignified and skillful manner. It was more comforting to attribute their behavior to patriarchal brainwashing.

butz than in the rest of Israel. We found sharp differences in what men and women cared about in public life and in their choices of political and managerial behavior. Amusingly, we found a negative relationship between attitudes about sexual similarity and actual behavior. Each generation was more divided than the last—a clear rebuke to the role-model theory of behavior. And most important, we could see the beginnings of what has now become an almost total shift to family housing. Children began to live with their parents, a move overwhelmingly supported by women and their mothers who routinely outvoted the men of the communities. The men considered this move a reversion to bourgeois pathology and an expensive violation of the founding dream. At times, indeed, it seemed as though we had been studying two different communities, men and women.

There were some positive reviews of the book—by the respected Zionist writer Marie Syrkin in the *New Republic* and by many commentators in Israel itself. But in

Throughout the 1970s and early '80s, the opposition to the biosocial—or sociobiological—enterprise grew more heated. I felt a sense of almost physical apprehension, knowing how easily I could become the object of censure. At meetings of the American Anthropological Association, conversation would stop and people would stare when I entered an elevator and they saw my name tag. I wasn't alone. There was an unseemly ruckus over E. O. Wilson's further elaborations of his sociobiological insights, opponents going so far as to dump water on him when he made an appearance at the 1979 meeting of the American Association for the Advancement of Science in Washington. The

American Anthropological Association tried to censure Napoleon Chagnon of the University of California for chronicling aggression among the Yanomani of Venezuela, as though he had caused it by describing it. The same association voted unanimously to support the "Seville Declaration," a sanctimonious assertion by a number of otherwise sensible scientists that any effort to explore human nature factors in human aggression was ethically wrong and scientifically inappropriate. As part of a series of seminars at the University of Chicago in the mid-1970s, a few of us who shared the biological perspective tried to invite Wilson and his colleague Richard Lewontin to discuss their differences over sociobiology. But Lewontin refused to be in the same room with the man who had been among those responsible for Harvard's hiring him in the first place. For their part, many radical feminists were convinced that anyone who disagreed with them was politically reactionary, pathological, or an agent of a devious male conspiracy.

Such ideological zealotry drew sustenance from major social changes that were already under way in the United States and Europe, stimulated in large measure by the "pill" and other birth control devices that had become widely available in the mid-1960s. There was surprisingly rapid abandonment of the conventional certainty that it was man's role to work and provide and woman's to bear children, raise them, and keep house. While we did not think that modifications of gender roles were impossible or undesirable, we did believe that they raised profound biological questions. But biological analysis was still largely kept out of the conventional national dialogue. One reason, no doubt, was that proponents of biological approaches, who confronted such issues as aggression, hierarchy, sexual differences, and xenophobia, were seen as bearers of bad news.

Nevertheless, Fox and I continued together and separately to play active roles in academic life, and in such practical precincts of government and business where biosocial perspectives and information were wanted. One project grew out of an opinion I offered in *Men in Groups* to the effect that contraceptive pills would likely influence the sexual enthusiasms of men. Nature, being economical, surely would see to it that pregnant females would have less appeal to males seeking reproductive success. Since females taking the pill were chemically pregnant, we wondered whether nature's design would apply to them as well.

With a small amount of money from the Guggenheim Foundation and working with colleagues at Rutgers Medical School and other parts of the university, we administered the contraceptive drug Depo-Provera (the basis of Norplant) in injections effective for three months to monkeys in a colony we were able to establish on an island off Bermuda. The medicine completely extinguished hitherto robust sexual relations. When its effects had worn off, the original dating game resumed. We tried to publish the report in *Science* but were told we had no control group. We protested that we had produced an ethological record of a community over a year, under carefully controlled and evaluated conditions. But to no avail. And so subsequent publications appeared in more specialized journals.

Were the findings too controversial in the light of then-current sexual practices and beliefs? It appeared as though no one wanted to challenge a widely appreciated medical innovation—or even to see whether the primate pattern also applied to human communities where the pill was widely used.

It seemed to me then and does still that there had to be some discernible effect when a large percentage of women in a community were chemically pregnant. No other drug, until Prozac perhaps, had been given on a daily basis to healthy people. And the contraceptive pill affected nothing less than sexual selection, the core relationship at the heart of biological process and evolution itself.

It had no behavioral effect at all? Please.

Here was a case in which biosociology had direct policy relevance. Most tests of drugs by the Food and Drug Administration and equivalent agencies deal first with clear physiological and systemic effects—your liver clogs, your eyebrows turn orange, there are carcinogens, and so on. Behavioral impacts are far less thoughtfully and substantially evaluated, even with drugs such as contraceptives or psychoactives explicitly designed to affect behavior. For example, it took the FDA years to realize what any biosocial scientist could have seen right away: that Valium and Librium (the widest-selling drugs in the world for a while) were not harmless social lubricants but powerful drugs with substantial addictive and cultural effects.

Unfortunately, the bias of the industrial system is to look for easily quantifiable technical factors and deal primarily with them. It does not emphasize the kind of sensitive full-life-cycle assessment that even mediocre zookeepers currently demand when they manage the routines and housing of their charges.

The prejudice against biological analysis did not immediately abate when I published *Optimism: The Biology of Hope*, but it was around that time that we began to witness some turning of the tide. In *Optimism*, I suggested that idealism and social vision, to say nothing of love affairs and feelings we have on the first day of school, are as much part of our nature as tribal antipathy and sexual ruckus. "Hope springs internal," I announced, because it seemed obvious to me that if neurophysical substances (about which more and more was being learned) were associated with depression and hence treatable with other substances, then there must be a comparable material basis of happiness and optimism. About halfway through my research on the subject, in the mid-1970s, endorphins were discovered. If not the elusive substance themselves, these were certainly signs of the the material neural basis of feelings of well-being.

I went further to speculate that any species with as large and fertile a thought factory as ours had somehow to discipline what it produced. There had to be a neurophysiological basis for our getting up in the morning and deciding it was a great day to trap an elephant or court a partner. Human beings had surely evolved the capacity to plan ahead, to hope, to create, and to believe in the value of life itself.

Perhaps that evolved knack was even the basis of religious behavior, which is virtually ubiquitous in human communities. Was it not reassuring to think that human idealism and hopefulness had their roots in brain physiology and other mechanisms that supported our evolution?

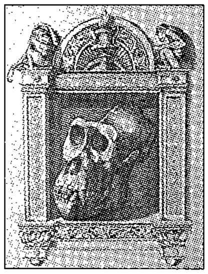
Not until 1992, however, when I published *The Pursuit of Pleasure*, with its argument that pleasure was an evolutionary entitlement as important to our species as discipline and the goad of pain, did the formal recalcitrance change to a suspension of disbelief and distaste. Perhaps it was the failure of various utopian schemes, including industrial-strength Marxism, that made it easier to argue that human behaviors were related to human evolution and constrained by the particular pattern of species. But this need not be a gloomy conclusion, I argued. To change a system, one must first understand it, and a knowledge of human biology can be as much a basis for idealism and action as for paralysis and despair.

In fact, in an earlier book, *The Manufacture of Evil: Ethics, Evolution, and the Industrial System* (1987), I had argued that our species was still trying to make do with skills that it had slowly acquired for dealing with the social and ethical dilemmas of a hunter-gatherer existence in communities of between 25 and 200 souls. I pointed out the obvious: that such skills, products of lengthy evolutionary change, are ill-suited to the social and economic realities of modern mass industrial societies; and, furthermore, that our prevailing ethical systems, which arose during the adoption of agriculture and pastoralism, do little to address the incompatibilities.

Take for example the vexed issue of the growing chasm between the leaders and the led, the elite and the common folk, the winners and the losers in what is called our "winner-take-all" system. One of the bonds that used to tie leaders to their constituencies was kinship, a deeply biological tie.

Yet for the sake of justice, a noble ethos, modern legal strictures against nepotism discourage the exercise of such primal connections. One biologically predictable result is that our leaders feel less and less responsible for those beneath them. It is an unhappy biological reality, but our refusal to face it, and others like it, may actually aggravate social inequalities and tensions. The biosocial perspective I urged in this book clearly offers little easy reassurance to idealogues of either the right or left persuasions. But as anthropologist Melvin Konner noted in a review in *Science* magazine, the book's argument is "probably a far more radical critique of modern industrial capitalism than was Marx's and Engels's."

Today it is clear that the biological account has left its mark on the intellectual landscape. Even daily newspapers purvey information about behavior involving definitive brain images of sex differences in human cortical function. More importantly, there is now a sophisticated body of work that knits together the biological and social sciences. And there is every reason to expect that the expansion of the explanatory power of biosociology will continue. Developments in Darwinian medicine, neurophysiology, paleoanthropology, economics, and political science, and a host of other disciplines will continue to help sketch a picture of *Homo sapiens* rooted in nature, in history, and—critically—in prehistory. It is no longer heart-stopping to discuss human biology in the academic community, while among feminists there is at last a potentially productive dialogue between those who still regard all sex differences as social constructs and those prepared to see them as embedded in the nature of humanity.



However satisfied one might be with such developments, large areas of darkness remain in the intellectual community. In my own discipline, anthropology, the majority social-construction-of-reality crowd has created a world of solipsistic relativism founded on a commitment to the notion that positive, objective science is an impossibility. Though they may have as much impact on international science as phrenologists, their impenetrable obfuscation of behavioral matters may cause physical anthropologists to drift away from the main association. They will continue to produce a barrier between the worlds their readers experience and the one the professors describe. As Robin Fox says, "If it walks like a duck, quacks like a duck, and looks like a duck, it is a social construct of a duck." The most dispiriting feature of the delta of pressure toward political correctness is not its apparatchik banality but its scientific ludicrousness and its utter impracticality.

As I started these reflections on my service in the human nature wars, I recalled the quotation that I was required to supply for my college yearbook. The words were supposed to be self-epitomizing, and so I had chosen William Blake's "I must create a system or be enslaved by another man's." For a moment, I shivered at the thought that my whole career amounted to a petulant and antisocial act of intellectual defiance.

But then I realized: no, I am firmly in Mr. Darwin's system. I love his commitment to the elegance of life's flow and the vast importance of individual decisions about whom to love, to hate, to play with, to avoid, to feed. I admire and applaud his precise awareness of the meld of physical form and behavior and his tutored bystander's appreciation of the artfulness, the color, and the intimate drama of animal life. This scientific community in which I found myself so unexpectedly has been no cruel master, no impediment to exploration. To the contrary, it has provided me passage to a world in which the marriage of precise perception and broad thought is celebrated, where open and inquiring minds are free to stride, wander, and wonder.