

## Science and ideology

by Edward O. Wilson

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I have composed this text with considerable humility because it is addressed to scholars and scientists many of whom speak more authoritatively on the history and philosophy of science than I. My own preferred reading list on the subject would include Gerald Holton's *Science and Anti-Science* and the wonderfully scorching book, *Higher Superstition*, by Paul Gross and Norman Levitt. For a full reading list that could compose a complete college course on the subject, I would add John Passmore's *Science and Its Critics*, published in 1978, and Steven Weinberg's adamant image of the power and ideology-demolishing reach of modern physics in his book *Dreams of a Final Theory*. In many ways I would defer to these authors.

I hope they nonetheless might agree with me that the nobility of science as a human endeavor was well encapsulated by the physicist Subrahmanyan Chandrasekhar when he used the Icarus metaphor in praise of Sir Arthur Eddington. He said, "Let us see how high we can fly before the sun melts the wax in our wings." And on the appropriateness of the rosette of the National Academy of Sciences, the other NAS, that is splendidly symbolic in this sense: the gold of science is placed solidly in the center, surrounded by the purple of natural philosophy. Members are elected primarily or solely on the basis of objective discoveries they have made, expressible in clear declarative sentences, and not by any ideological test.

By science in common parlance is meant natural science, which gathers knowledge of the world as an organized, systematic enterprise and attempts to condense it into testable laws and principles by a wide-ranging and shifting set of methods. The diagnostic features of science that distinguish it from pseudoscience are, first, repeatability: the same phenomenon is sought again, preferably by independent investigation, and the interpretation given it confirmed or discarded by means of novel analysis and experimentation. And second, economy: scientists attempt to abstract the information into the form that is simplest, most easily recalled, and most esthetically pleasing--the combination called elegance--while yielding the largest amount of information with the least amount of effort. Third, mensuration: if something can be properly measured, using universally accepted scales, generalizations about it will be rendered less ambiguous. And fourth and finally, heuristic: the best science stimulates further discovery, often in unpredictable new directions, whose content confirms or modifies the parent formulation.

Science is thus not just a profession. Nor is it a delectation of mavens. Nor is it a philosophy. It is a combination of mental operations that has increasingly become the habit of educated peoples. It's a culture of illuminations hit upon by a fortunate turn of history, of uncountable small and large steps, of adjustments to reality during the past four centuries that yielded the most powerful way of knowing about the world ever devised.

Is this triumphalism? Not, I think, in the sense of a mental force impinging on history, art, and ritual. But, blended with technology, science as a way of knowing has already transformed human existence. Being richly self-rewarding and universally distributed, it feeds upon itself and grows exponentially. Scientific knowledge doubles every ten to fifteen years, as measured by articles, new journals, and the number of professional scientists. Understanding based on the new information now reaches into virtually every sphere of human activity and every moral dilemma. One need think only momentarily on nuclear armament, the Green Revolution, genetic engineering, cloning, artificial intelligence, visits to the planets, and human activity as an atmosphere-altering force--all changes that have originated or accelerated during the past fifty years--to see where science is taking us. The future, if we are to have one, is increasingly to be in the hands of the scientifically literate, those who at least know what it is all about. There can be no multicultural solution to the genetics of cystic fibrosis; the ozone hole cannot be deconstructed; there is nothing whatsoever relativistic or culturally contextual about the dopamine transporter molecules whose blockage by cocaine gives a rush of euphoria, the kind that leads the constructivist to doubt the objectivity of science.

Science isn't easy; that's why it took so long to get started, and then mostly in one place, western Europe. Part of the reason is that the actual process of scientific discovery is relatively rare and intellectually distinct from the body of accumulated knowledge it creates. Scientists as a rule do not discover in order to know. Instead, as Alfred North Whitehead said, they know in order to discover. They learn what they have to know, often remaining poorly informed about the rest of the world, in order to get to the frontier of knowledge where they can make discoveries. They move forward, each along a deliberately narrowed sector. Make one significant discovery and you're a scientist in the true, elitist sense, the sense most desired by scientists themselves, and you go into the textbooks. No discoveries and you are nothing, no matter how much you otherwise learn and write about science. When a scientist practicing this way begins to sort out knowledge in order to look for meaning, and especially when he carries that knowledge outside the circle of discoverers, he becomes a humanist. Thus in science there exists a fundamental distinction between process and product. That is why so many important scientists are narrow, foolish people and why so many learned, wise scholars in the field are not considered by their peers to be strong scientists, unless they find something new about how the world works.

It follows that the process of discovery, the inner fire of the scientific enterprise, cannot be communicated effectively to the citizen who doesn't already know a substantial amount of science. Only when he possesses some of the content of science can he grasp its living culture. Then he can understand how scientific knowledge is validated and how best to make judgments on his own accord. Graphs and "margins of error" make sense to him. He can explain them to others. Controls, multiple competing hypotheses, and disconfirmation become habits of thought. Accounts of science in newsmagazines are read with an engrained reserve, and scientists are viewed less as savants than as the artists and lucky conjurers they are in fact. Moral-tinged controversies are weighed with close attention to testable reality in the physical world. Of course these abilities are very limited today, and that is why anti-science ideologues and other charlatans get away with so much.

Which brings me to anti-science. I know less about postmodernism than most of you here, but let me give you my impression of how it relates to science. Postmodernist critics present a Disney World representation of science, a fantasy of what science is, and how scientists work, and why they work, a distortion embellished variously by obsolete theories of psychoanalysis and the battle cries of political ideology. Within the academy, it seems to me that postmodernism and the divisive forms of multiculturalism are substantially a

revolt of the proletariat, wherein second-rate scholarship is parlayed into tenured professorships and book contracts--not by quality, not by originality, but by claims of entitlement of race, gender, and moralistic ideologies. But as I will show in a moment, some of it runs deeper, to turn the minds of even a few otherwise respected scientists.

There is also a tendency to think of these toxic developments as a recent replacement within the academy of Marxism, which, as Irving Howe nicely put it, has now retreated to departments of English in hopes of dying a comfortable death. But virulent anti-science was rampant twenty years ago. Here is how Science magazine described some of the disruption by radical leftists of the 1971 meeting of the American Association for the Advancement of Science, held in Chicago: "Glenn T. Seaborg, president-elect of the AAAS, took the advice of convention officials and fled from a meeting room to avoid being 'indicted' by young radicals; Edward Teller, the so-called 'father of the hydrogen bomb,' was repeatedly badgered despite the two bodyguards who trailed him everywhere," and "Mrs. Garrett Hardin, wife of a distinguished biologist, got so angry that she poked a young radical with her knitting needle."

The podium at one meeting was seized from Philip Handler, president of the National Academy of Sciences, who was then denounced as a "lackey of the ruling class." A group calling itself the Women's International Terrorist Conspiracy from Hell (or WITCHES for short) pronounced a hex on the AAAS meeting: "Science, Technology. We declare its use a sham. And subject all who use it ill to the witches' damn."

Now, the relevance of this frivolity to later attacks on science from the political extreme Left is closer than it might at first seem. The demonstrations were planned and executed by Science for the People, an organization devoted to anti-science ideology, and a key figure in the demonstrations was Richard Lewontin, then at the University of Chicago and soon to join the faculty at Harvard.

I introduce the organization and the man because they were to be key forces in my own life during the sociobiology controversy of the late 1970s, which I'll now relate briefly to you as a case history of ideologically motivated anti-science. There is no substitute for personal experience.

The sociobiology episode was one of the most conspicuous in the history of political correctness in academic life in the dark time before the expression, p.c., was coined and before the National Association of Scholars or any other form of organized resistance arose to blunt its excesses.

Let me say that sociobiology, the study of the biological basis of social behavior, was never in dispute when applied to animals. The discipline was accepted from the beginning by biologists. In 1989 the members of the Animal Behavior Society, the main international organization in the discipline, ranked my 1975 text *Sociobiology: The New Synthesis* the most important book on animal behavior ever published. But my speculative extension of the theories of sociobiology to human beings in the twenty-seventh and final chapter of the book received an entirely different reception, at least outside biology. The prevailing view in the social sciences in the seventies was essentially that there is no biologically based human nature, that human behavior is almost entirely sociocultural in origin, and therefore that the genes play little or no role except in bestowing intellectual and emotional capacity. I urged the contrasting view, that biology plays a larger role, in close concert with culture, and that human behavior cannot be understood without biology. I think it fair to say that this perception, as heretical as it was in the 1970s, is mainstream today. Actually, the opposing view never had the intensity displayed by Science for the People and a few other extreme critics on the Left. Among more than 200 scholarly books on human sociobiology and closely related subjects

published since 1975, those generally favorable outnumber those mostly critical twenty to one. Social theorists who disagreed with sociobiology for the most part just steered around it.

The radical activists, however, went ballistic on this issue. Shortly after the publication of *Sociobiology*, Richard Lewontin organized fifteen scientists, teachers, and students in the Boston area as the Sociobiology Study Group, which then affiliated with Science for the People. The latter, larger aggregate of radical activists was begun in the 1960s to expose the misdeeds of scientists and technologists, including especially thinking considered to be politically dangerous. It was and remains nation wide, although greatly attenuated in its tone and influence.

What was correct political thinking? That has been made clear by Lewontin during the debate and afterward. "There is nothing in Marx, Lenin, or Mao," he wrote with his fellow Marxist Richard Levins, "that is or can be in contradiction with a particular set of phenomena in the objective world." True science, in other words, must be defined intrinsically to be forever separate from political thought. Ideology can then be constructed as a mental process insulated from science.

In formulating sociobiology, I wanted to move evolutionary biology into every potentially congenial subject, including human behavior and even political behavior, roughshod if need be and as quickly as possible. Lewontin obviously did not.

By adopting a narrow criterion of acceptable research deserving the title of science, Lewontin freed himself to pursue a political agenda unencumbered by science. He purveyed the postmodernist view that accepted truth, unless based upon unassailable fact, is no more than a reflection of dominant ideology and political power. After his turn to political activism, around 1970, he worked to promote his own accepted truth: the Marxian view of holism, envisioning 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 as a whole. And most particularly, he rejected it for human social behavior. He said, in 1991, "By reductionism, 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. "

Now 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. It cannot be made into science. And according to his own political beliefs, expressed over many years, sociobiology or any other social theory based on the biology of individuals cannot even possibly be true. Here is how he summarized his postmodernist argument: "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 as the center of everything."

That much being understood, Lewontin concluded, and the shackles of bourgeois ideology cast aside, we are then freed to proceed along more progressive--that is to say, Marxist--political guidelines. These do not require scientific validation, at least not by any connection with genetics, neurobiology, or evolutionary theory. The genes, Lewontin declared, "have been replaced by an entirely new level of causation, that of social interaction with its own laws and its own nature that can be understood and explored only through that unique

form of experience, social action." Hence the inviolable wisdom of Marx, Lenin, and Mao to which he alluded elsewhere.

Now I can come to the essence of the radical science movement. As loopy as it all may seem today, and especially after the collapse of world socialism, the argument has to be taken seriously, since it has been accepted to varying degrees by a few influential scientists, including Stephen Jay Gould, Richard Levins, and Ruth Hubbard, who are highly regarded in the public eye as scientists, even as they continue to promote a Marxian view.

Here then is the argument in its raw form: only an anti-reductionist, non-bourgeois science can help humanity attain the highest goal, which is a socialist world. In the 1984 book *Not in Our Genes*, Lewontin, Steven Rose, and Leon Kamin, all worthies of radical science philosophy, explained their purpose as follows:

We share a commitment to the prospect of the creation of a more socially just--a socialist--society. And we recognize that a critical science is an integral part of the struggle to create that society, just as we also believe that the social function of much of today's science is to hinder the creation of that society by acting to preserve the interests of the dominant class, gender, and race. This belief--in the possibility of a critical and liberatory science--is why we have each in our separate ways and to varying degrees been involved in the development of what has become known over the 1970s and 1980s, in the United States and Britain, as the radical science movement.

That well respected scientists, two of whom, Lewontin and Levins, had been elected to the National Academy of Sciences (and soon removed themselves in ideological protest) could advocate an approach to science guided by a radically sociocultural version of Marxism may seem odd today given recent history. But it helps to explain the distinctive flavor of the controversy at Harvard in the 1970s, in an atmosphere of unfettered political correctness. In the standard leftward frameshift of academia prevailing at that time, Lewontin and members of Science for the People were classified as progressives, admittedly a bit extreme in their methods, while I--Roosevelt liberal turned pragmatic centrist--was cast well to the right.

Now to return to my story. Although the unofficial headquarters of the Sociobiology Study Group was Lewontin's office, located directly below my own at the Museum of Comparative Zoology, 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* (one might ask, where else?) on 13 November 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, they wrote,

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 3 November. 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. And the purpose of the letter was not so much to correct alleged technical errors as to destroy credibility. Furthermore, the position of the Sociobiology Study Group was ethical in tone and therefore very difficult to challenge. The idea of human sociobiology as a field of study is both intellectually and morally wrong, the critics in Science for the People said. It would of course have been impolite for me to point to the imperfections of the Soviet Stalinists or to question the Marxist view. That in the view of many would have been a revival of McCarthyism and confirm my critics' opinion that I had a political agenda.

In the liberal dovecotes of Harvard University of the seventies and eighties, a reactionary professor was like an atheist in a Benedictine monastery. As the weeks passed and winter snows began to fall, I received little support from my colleagues on 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 Holldobler. 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. Both Lewontin, who was chairman of my department, and Gould, a respected member, continued to be treated with deference. The department members 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 absolutely 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 that I didn't know. I was unprepared perhaps because, as Maynard Smith further observed, I am an American rather than a European. In 1975 I was a political naif: 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 an intellectual in the European or New York/Cambridge sense.

After the Sociobiology Study Group exposed me as a counterrevolutionary adventurist, and as they intensified their attacks in articles and teach-ins, other radical activists in the Boston area, including the violence-prone International Committee against Racism, conducted a campaign of leaflets and teach-ins of their own to oppose human sociobiology. As this activity spread 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 the pressure was tolerable, since I was a senior professor with tenure, with a reputation based on other discoveries, and in any case could not bear to leave Harvard's ant collection, the world's largest and best. 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. I withdrew from department meetings for a year to avoid embarrassment arising from my notoriety, especially with key members of Science for the People present at these meetings. In 1979 I was doused with water by a group of protestors at the annual meeting of the American Association for the Advancement of Science, possibly the only incident in recent history that a scientist was physically attacked, however mildly, for the expression of an idea. In 1982 I went to the Science Center at Harvard

University under police escort to deliver a public lecture, because of the gathering of a crowd of protestors around the entrance, angered because of the title of my talk: "The coevolution of biology and culture."

Gerald Holton has warned of the rivulets of unreason that can come together at different times and different circumstances to form a threatening floodstream. The evidences remind us that, in Bertrand Russell's words, the mass of people would rather believe than know. Holton, Gross, Levitt, and others have shown that politicized anti-science is a flourishing trade within the academy. I will add with conviction that on occasion it can take root in the very entrails of science. And not just in a totalitarian state, exemplified by Soviet Lysenkoism and Nazi eugenics, but in a democracy and promoted by people who feel they are doing the morally right thing.

The sociobiology controversy as an example is not unique in recent history, although I wish it were. On 16 May 1986, a group of academic luminaries, including Robert Hinde, John Paul Scott, and several other prominent behavioral scientists, issued the Seville Declaration (following a conference in Spain), declaring invalid any theories or claims that aggression and war have a genetic basis. Such thinking is according to them, "scientifically incorrect." "Wars," the Declaration said, "begin in the minds of men." Warfare is a product of culture; biology contributes only in providing language and the capacity to invent wars. Case closed. The authors of the Declaration suggested, in effect, that if you have any thoughts otherwise about these matters, keep your mouth shut. The Seville Declaration was adopted that same year as the official policy of the American Anthropological Association. Eighty percent of the members who returned ballots on the motion to adopt voted in favor. Virtually all the main premises and conclusions of the Seville group are contradicted by the evidence, but no matter--the Declaration seemed to its signers and ratifiers the politically and morally correct thing to do. All the participants must have felt good about supporting it.

But as we shall see as the new IQ wars develop over the coming months [they have since proved virulent on the anti-genetic side--Author], as ideologues on both sides spring into their accustomed positions, feeling good is not what science is all about. Getting it right, and then basing social decisions on tested and carefully weighed objective knowledge, is what science is all about.