Politics by Scientific Means and Science by Political Means: Trojan Horses in the Sociobiology Debate

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The Sociobiology Debate: A Second Look

In an article in 1981 at the height of the sociobiology controversy, biologist and essayist Lewis Thomas marveled at the fact that two intelligent scientists like Harvard's Edward O. Wilson and Richard Lewontin would wish to engage themselves in something as unyielding as a dispute about human nature. For Thomas, that was the same as "debating the unknowable" (Thomas, 1981). Still, the sociobiology controversy seems to have held deep fascination for a great number of people for a quarter of a century – perhaps just because it was dealing with such metaphysically appealing issues as human nature, morality, and free will. Today the debate continues in a new guise in the conflict surrounding "evolutionary psychology," a take-off from sociobiology. But on June 26, 2000, with the announcement that the sequencing of the human genome was soon to be completed, it may have seemed to many as if the metaphysical dispute of the last three decades or so – in the latest manifestation of its at least 2000 year old history – was finally nearing its resolution. Still, the sociobiology debate was never about real physical genes. Sociobiology had to do with hypothetical genes "for" behavior appearing in certain new theoretical models, and it was these genes that were under dispute.

What attracted the participants in the sociobiology debate to this particular academic feud? No doubt metaphysical questions gave the controversy some of its appeal. Still, we have to remember that the protagonists were *scientists*. And whatever other concerns and commitments scientists may be entertaining, they are typically interested in *science*. I will here develop the argument that the controversy actually served the participants on both sides well in their ambition to promote what they took to be "good science." But just how did this happen? I identify two different scenarios. One represents the standard in-

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terpretation of the sociobiology debate, which says that what was happening was just "politics by other means." Already here, however, it is clear that the sociobiology controversy was not a "purely" political debate, but also involved deep-seated scientific beliefs and ambitions. The other is the more interesting possibility that the sociobiology debate was actually an example of a different strategy, which might be called "science by political means."

In this article, I will be limiting myself to a discussion of Edward O. Wilson and his two chief intellectual opponents Richard Lewontin and Stephen J. Gould. Obviously, the sociobiology debate has later become a major transatlantic controversy with Gould and Richard Dawkins as the chief combatants. I will be largely drawing on material presented in more detail in my book *Defenders of the Truth: The Battle for Science in the Sociobiology Debate and Beyond* (Segerstråle, 2000).

Sociobiology and Its Enemies

Wilson defined sociobiology as "the systematic study of the biological basis of all social behavior." The idea was, that just like other features, behavior, too, is undergoing evolution. But what upset a large number of academics and others at the time was that Wilson in the last chapter of his book included also humans, and hypothetical genes for all kinds of human behaviors.

The book *Sociobiology* was mostly about animals, of course. And Wilson did have a lot of interesting news to share about animal social behavior. There had recently been a scientific breakthrough in evolutionary biology: the mystery of animal altruism had finally been cracked by people like William Hamilton, Robert Trivers, and John Maynard Smith. New theories now shifted the focus from the individual organism to groups of relatives who shared genes. With the help of cost-benefit calculations with an eye to the genetic relatedness between a donor and a recipient of an altruistic act, it was now possible to show that from a gene's point of view it made sense for a bird, say, to sacrifice itself by letting out an alarm call, if it in this way could save a whole bunch of relatives. Richard Dawkins wrote the book *The Selfish Gene* to further explain the reasoning behind the new models in evolutionary biology.

What happened in 1975 was, that what Wilson wanted to present as exciting new findings, his critics declared to be "bad" and dangerous, ideologically influenced science. The critics were relentless in their attack. Among the most vocal ones were a number of Wilson's Harvard colleagues, including Lewontin and Gould. A letter in the *New York Review of Books* signed by them and a group of Boston area academics claimed that *Sociobiology* supported a conservative political agenda and linked the book to racism and Nazism. According to the critics, if you said that something was in our genes, that meant that it could not be changed, and this kind of biological determinism would in turn undermine the very idea of social reform. The most dramatic event was the 1978 meeting of the American Association for the Advancement of Science in Washington, where a group of activists poured a pitcher of ice-water in Wilson's neck, shouting: "Racist Wilson, you can't hide, we charge you with genocide!" or (the now empirically demonstrated): "Wil-

son, you are all wet!"

Many bought into the critics' view of sociobiology and Wilson's political motives. Very few ever read his book (it was a quite huge tome). And even fewer asked about Wilson's real agenda – or, for that matter, about the critics' agenda. And it is true that the critics' claims were plausible. In 1975 it was clearly too early to even talk about the possibility of a biological basis for human behavior. The "environmentalist" or culturalist paradigm reigned high, with people like Margaret Mead in anthropology and B. F. Skinner in psychology. And just before, there had been the controversy about IQ around psychologist Arthur Jensen's (1969) suggestion that the 15 point White and Black difference in measured IQ could have a genetic explanation. Wilson was actually extremely careful in his book when it came to statements about both IQ and race.[1] But he had committed a bigger crime. He had speculated that human characteristics, including some of our most cherished ones, could actually have a genetic basis: all the way from sex role divisions and aggressiveness to moral concerns and even religious beliefs.

Defenders of the Truth

In *Defenders of the Truth* I argue that one of the important dividing lines between the two camps in this long-standing controversy did, in fact, have to do with fundamentally different convictions about the nature of *science* – and this encompassed also moral/political aspects. It was basically a conflict between different conceptions about the way science ought to be done and different assessments of the social utility of research. While sociobiologists found it unproblematic to develop scientific models using hypothetical genes "for" social behavior, and behavioral geneticists felt free to posit genes "for" various personality traits or intelligence, this was an absolute anathema for the critics. Many of the opponents of sociobiology had been trained in the experimental laboratory tradition, and for them, for a gene to be even talked about, it would have to be identifiable in the lab!

Declaring the modeling attempts of sociobiology and behavioral genetics "bad science" automatically put the critics of sociobiology in an one-upmanship position, because the sociobiologists and behavioral geneticists clearly had no physical genes to show. Still, the modelers felt justified in hoping that in the future there would, indeed, turn out to exist real genes corresponding to the traits postulated in their formulas. They saw themselves as working with provisional genetic models, expecting the details to be worked out later as experimental science proceeded. In the meantime, they would be developing testable hypotheses and proceed *as if* behavioral genes actually existed. They considered this very standard as a scientific strategy and as representing normal "good science" in their fields.

But this was only part of the conflict. The contrasting positions on science were at the same time connected to larger agendas of the contending parties – and each side used the sociobiology controversy as a convenient vehicle to further these.

What was the nature of these conflicting agendas? If we focus on the chief opponents Wilson and Lewontin – two colleagues with offices dramatically lo-

cated one above the other – we can identify a "positive" and a "negative" agenda. Wilson's positive agenda was very ambitious. Epistemologically, it involved uniting the social and natural sciences. For Wilson, the social sciences were destined to become more scientific through the incorporation of population biology and statistics. He also saw sociobiology as swallowing up ethology by the year 2000. Philosophy, in turn, would take its lead from sociobiological truths about human nature, thus "grounding" the ethical realm in values derived from biology. Underlying all this was a noble goal: to secure the future of mankind and life on Earth. With the help of biological insights into the truth of human nature, we would be able to make wiser choices and steer away from unfeasible cultural courses, perhaps even self-destruction. For Wilson the biologist and humanist, the main concern was to preserve the diversity of life on Earth, including the diversity of the human gene pool.[2]

For Lewontin, the scientific and social critic, it was careful and painstaking experimentation, not fanciful sociobiological models, that was the route to good science. Lewontin wanted an experimental approach with real genes rather than models of hypothetical genes. He also wanted clear mechanisms of cause and effect, not correlations. And he seemed to particularly heartily dislike statistics as a scientific tool. Indeed, he sometimes acted as as if he thought that people who calculated such things as averages actually believed in the *reality* of averages, too, and that scientists who used a reductionist methodology automatically were also *ontological* reductionists, that is, believed in the actual existence of the atomistic entities they used as heuristic tools. In other words, Lewontin's program was mostly a negative one; he criticized sociobiology (emphasizing with Mies van der Rohe that "God is in the details"), but did not seem to have a positive alternative to offer. Later on, though, Lewontin developed what could indeed be seen as a kind of positive program, emphasizing such things as the complex mutual determination of organism and environment.[3] But that must be seen as a philosophical rather than practically oriented contribution – it was not clear how this would translate to actual laboratory practice, except stop people from making simplifying assumptions.

Wilson's wish to push the frontier of knowledge forward as quickly as possible, in bold leaps rather than by careful establishment of details, was certainly not unique to him. It represented a particular scientific style of risk-taking and the charting of unknown terrain.[4] There were, in fact, matters of taste, too, that deeply divided Wilson and Lewontin. Wilson saw himself as a visionary, drawn to unexplored, "messy" fields, which he would then sort out and structure. (In view of this he described Lewontin as someone who liked to play it safe, "hugging the coast" while questioning and criticizing). Lewontin, in turn, considered his own approach scientifically correct, but Wilson's scientific ambition overwrought and simply "not serious" (see Segerstråle, 2000, ch. 3 and 8).

What was clearly unusual in Wilson's agenda, though, was his explicit wish to derive *values* from the study of biology. In principle, the idea was not new: there were in fact a number of scientific predecessors, for instance Jacques Loeb and

Conrad Waddington. But this was the mid-1970s, and this kind of ambition for a biologist was certainly unheard of in the liberal academic milieu of the time.[5] Little wonder, then, that Wilson was perceived as committing "the naturalistic fallacy" of deriving "ought" from "is," a philosophical mistake which politically alert academics immediately attributed to nefarious political ambitions. Wilson tried to explain what he meant, but it was too late (Wilson, 1975b).[6]

Planters and Weeders

An important issue dividing the camps was the social responsibility of the scientist. What the Sociobiology Study Group feared was that any claims about genetic differences between individuals would be politically abused. Lewontin formulated the concern as follows:

> At present our ignorance on this question is so enormous, our investigatory techniques so primitive and weak, our theoretical concepts so unformed, that it is unimaginable to me that lasting, serious truths about human nature are possible. On the other hand the need of the socially powerful to exonerate their institutions of responsibility for the problems they have created is extremely strong. Under these circumstances any investigations into the genetic control of human behaviors is bound to produce a pseudo-science that will inevitably be misused. (Lewontin, interview, the Harvard Gazette, November 3, 1975)

Indeed, policy-wise it was possible to point to such things as earlier eugenics ambitions and sterilization programs based on purported state-of-the art scientific knowledge which later turned out to have been mistaken; and theory-wise one had only to mention the "mismeasurement" of skulls resulting in racist and sexist theories (Gould, 1981) and various biologically based theories of racial supremacy.

And it was this view that Lewontin shared with the Sociobiology Study Group. In fact, in the sociobiology controversy could be found a rather unusual category of scientific practitioners, who in addition to their own scientific work regarded it as their duty to debunk what they saw as other scientists' "bad science" in fields often far away from their own. I have characterized this group as "weeders." Because of this self-imposed mission, the weeders were in direct opposition to the large majority of scientists, whom I call "planters," traditional scientists who believed that the goal of science was simply to produce new "positive" knowledge.

Planter-type scientists may or may not have thought that all science produced by their colleagues was necessarily "good," but they did not feel they needed to take action beyond their traditional scientific duties. In other words, they expected possible errors to be identified and eliminated in due time by the regular scientific process, and they left it to the democratic social process to decide about the ultimate use of scientific knowledge. This is why it did, indeed, come as a surprise to many planters when in the sociobiology controversy (and the preceding IQ one) self-appointed weeders felt free to directly accuse individual planters for "bad science." Because in these planters' own minds, they were simply following standard practice in their respective fields. (And so it usually was – it was just that the weeders considered the planters'

standard science exactly the kind of "bad science" that was socially pernicious).

While a weeder such as Gould limited himself to warning in writing about the "mismeasure of man" and advocating "debunking" as a positive science (Gould, 1981), people like Lewontin and his British colleague Steven Rose, coauthors of *Not In Our Genes* (Lewontin, Rose, and Kamin, 1984), took upon themselves to demonstrate why the factual claims of the sociobiologists and IQ measurers were wrong. Other members of the Sociobiology Study Group – notably Science for the People activist, Harvard molecular biologist Jon Beckwith – were involved in direct efforts to close down "dangerous" research, such as an early study at Harvard Medical School intended to diagnose and follow up boys with an XYY ("criminal") gene (see e.g., Davis, 1986, and Segerstråle 2000, ch. 11).

Trojan Horse I: Politics by Scientific Means

But of course it was not only a matter of conflicting scientific "world views," there were also strategical concerns involved in the sociobiology controversy on both sides. I will now turn to the well-known early accusation that Wilson's *Sociobiology* was basically a political manifesto.

Was the real aim with *Sociobiology* political? This is what the critics claimed again and again. Indeed, in what might be called their "sandwich model" of *Sociobiology*, Wilson was accused of putting 500 pages on animals between his two all-important chapters on humans to camouflage his real message which had to do with our species.[7] The critics seemed to take for granted that Wilson's aim with his last chapter, where he suggested a genetic underpinning for a number of human traits, was to legitimize the social status quo by advocating a biological determinist view of human nature. He wanted to defend social inequality as a natural state of affairs. Incidentally, the members of the Sociobiology Study Group were so sure about the political nature of Wilson's book that they even felt free to challenge the readers of *Science* to "see for themselves," assuring them:

> There is politics aplenty in *Sociobiology*, and those of us who are its critics did not put it there (Alper *et al.*, 1976).

According to this model, then, Wilson was trying to promote a conservative political agenda, disguising it as new and exciting science – he was engaged in "politics by scientific means." Even some colleagues had a hard time believing that Wilson could be so out of touch politically (e.g., Maynard Smith, interview). Wilson himself, however, has steadfastly denied that he was considering these kinds of political consequences when he wrote his book. According to himself, his primary goal was to provoke the social sciences into taking biology seriously (e.g., Wilson, 1991, 1994 and interview with me in 1981). Still, the fact remains that he did put the last chapter into the book at a time when the late sixties' debate around Konrad Lorenz' *On Aggression* and Robert Ardrey's *The Territorial Imperative* was still in good memory.

But Wilson did not take the criticism lying down. He soon responded to his critics in kind. And now it was his turn to insist that the critics' opposition to sociobiology was purely political (he described them, for instance, as "tabula rasa Marxists"). Incidentally, Wilson has

persisted in this till today, in various presentations and publications (e.g., Wilson, 1994, 1995), most recently in an interview in June 2000 in *Times Higher Education Supplement*, where he describes Gould's and Lewontin's attack on sociobiology as purely ideologically motivated. (Wilson's friend Bernard Davis went further, accusing particularly Gould of "Neo-Lysenkoism," Davis, 1983).

It soon became clear why Wilson was going from is to ought in his writings. One of his larger goals was to establish "a genetically accurate and therefore completely fair code of ethics" as an alternative to the teachings of "the theologians." This theme of grounding ethics in biology got developed later in *On Human Nature* (1978) and in Wilson's famous Tanner lecture (1980a) where he compares human morality to would-be termite morality. But the critics already had their interpretation ready and extracted suitable political quotes from *On Human Nature*, even in places where Wilson was obviously engaged in a critical commentary on established religion.

The Spandrels of San Marco Paper

Gould and Lewontin had early on joined forces with the Sociobiology Study Group, and were thus participating in the political campaign against sociobiology. Later, they turned their interest to a scientific critique of what they called "the adaptationist program," of which sociobiology was seen as a prime example. Their famous paper, "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme" (Gould and Lewontin, 1979) accused evolutionists of trying to demonstrate that every trait of every animal, including its behavior, was perfectly adapted. Adaptationists were just like Dr. Pangloss in Candide : they believed that this was the best of all possible worlds. (This was at least what many thought the paper said). The point with using the architectural notion of spandrels was to demonstrate that panadaptationism does not hold up: a trait may have simply come about as a by-product of evolution acting on something else, just like four "spandrels" are automatically created by two arches crossing in the ceiling of San Marco in Venice.[8]

The Spandrels paper, delivered at a Royal Society symposium, in turn triggered protests from the so-called "adaptationists" (who objected to being so classified). For instance, Dawkins (1982) pointed out that the assumption that a feature was adaptative was simply a *research tool*, not a belief. He said he considered it "unfair" to equate modern adaptationism with naive perfectionism in the style of Dr. Pangloss, because despite the claims of Gould and Lewontin (1979), "there are many kinds of adaptive, indeed Panglossian, explanations which would be ruled out by the modern adaptationists" (1982, p. 50). Maynard Smith, too, dryly observed that the aim was not to demonstrate that nature optimizes, but rather to test *particular* hypotheses (Maynard Smith, 1978).

For the critics, on the other hand, the possibility that an adaptationist framework was a mere research heuristic never even arose; it was automatically interpreted as a *metaphysical* belief with dangerous consequences. Indeed, adaptation had from the very beginning been made to sound as a political conspiracy.

Already in their first letter the Sociobiology Study Group charged that "for Wilson, what exists is adaptive, what is adaptive is good, therefore what exists is good," and "It is a deeply conservative politics, not an understanding of modern evolutionary theory that leads one to see the wonderful operation of adaptation in every feature of human social organization" (Allen *et al.*, 1975). It was this kind of position that Gould and Lewontin later spelled out in more scientific detail.

No wonder that many saw the Spandrels paper as a politically motivated attack on sociobiology. This was, for instance, what a discussant of the symposium thought when he heard the paper (Cain, 1979). And Queller (1995) later asserted in *Quarterly Review of Biology* that

> In the case of Spandrels, the context was the attempted intellectual lynching of a young science, sociobiology, which at its most uppity claimed to account for human nature in ways that were distasteful to many, not the least those with Marxist inclinations.

The conception that the debate was politically motivated was also strengthened by the social psychological mechanism of attribution of error. Each participant believed that he himself was pursuing "good science" and the truth, while the opponent's "incorrect" position had to be explained somehow. It obviously "could not" be scientifically based, but "had to" be influenced by "ideology." Neither side acknowledged the possible legitimacy of the other side's scientific convictions – a situation which appears quite typical in the world of science (evidenced e.g., by Mulkay and Gilbert, 1982, in their study of biochemists).

In other words, there were many good reasons for seeing the storm around sociobiology as an example of *politics by scientific means*.[9]

Trojan Horse II: Science by Political Means

But what if the aim of the Spandrels paper was, after all, *not* primarily political? Let us consider another interpretation, one which actually has some empirical backing. This is what Gould said in 1993:

> We faced a special and unusual sort of problem in gaining attention and understanding for alternatives to adaptation. … How can you challenge something if most people simply regard it as true and therefore haven't even conceptualized the possibility of another reading? *You can't initiate this sort of reform from within* (Gould 1993, p. 325, italics added)

In other words, something had to be done to rattle the complacency of the received view of evolution at the time. But there was a practical problem. How could a scientific protest become legitimate unless journal space was granted for it? And here the problem was that anti-adaptationist reasoning was too far from prevailing orthodoxy to be seriously considered. But there was a way. This is what Gould said at a meeting for Science for the People in the spring of 1984:

> We opened up the debate by taking a strong position. We took a definitive stand in order to open up the debate to *scientific* criticism. Until there is some legitimacy for expressing contrary opinions, scientists will shut up. A scientist will reason: "If I say this, they will accuse me of something unbiological" (Gould, spoken comment in 1984)

What Gould seems to be saying is that the sociobiology controversy was, in fact, a *vehicle* for the desired scientific debate about adaptation, a debate that would have had no chance on its own. In other words, on this view Gould's and Lewontin's *political* involvement with sociobiology would have been a strategical maneuver aiming at gaining a later hearing for their basically *scientific* argument about adaptation. The anti-adaptationist wedge could not have been driven in without the political controversy about sociobiology. (Indeed, Lewontin at the same meeting observed that "the brouhaha about sociobiology has had good effect in biology" and that "the debate had helped evolutionary biology").

Here we have, then, another type of Trojan Horse approach. The problem is how to create legitimacy for an unpopular idea that may easily be rejected out of hand. The answer is: draw attention to the new idea by making it "interesting" to scientists through its *moral/political* connotations. Then, use this newly generated interest to gain journal space, perhaps in the form of an "opinion paper." Finally, at the right moment, eliminate the moral/political envelope, and what emerges is the original scientific position (the critique of adaptationism), which could not have been considered in an unsupported form, since it would have been dismissed out of hand as not scientifically sound. In the meantime, more scientific support would have been mobilized for the new, unorthodox view.[10]

This interpretation – based on a tip from the horse's mouth – depicts Gould and Lewontin as deliberately breaking the rules of the scientific game in order to be able to make their voices heard among their colleagues. If so, what was their motivation? Were they defenders of the "real" scientific truth of "pluralism" – fighting what they saw as the "counter-reformation" of sociobiology (cf. Gould 1993, p. 315)? Or were they, in launching and promoting their particular anti-adaptationist program, rather defending their own scientific interests? Or both? Was the whole sociobiology controversy skillfully engineered by the two pair-hunting raptors, Gould and Lewontin, for the singular purpose of boosting an – at the time – scientifically vulnerable position? If so, the whole political upheaval by the Sociobiology Study Group would fall into the rather odd category of *science by political means*.

What, then, of the other members of the Sociobiology Study Group, who had no scientific agenda of this sort and nothing obvious to gain career wise (since they were outside the field of evolutionary biology)? Were they when they stirred up political dust around sociobiology and adaptation, unwittingly serving Gould and Lewontin, helping the latter to put their two-step plan in action? This may have been the case. Still, the moral/political concern was enough to unite the group, which spontaneously formed when Wilson's book was announced as an "event" on the first page of the *New York Times* on May 28, 1975. And certainly Gould and Lewontin genuinely shared the group's outrage about sociobiology.

Now let us go to Wilson and ask if a similar analysis can be applied to him. What if Wilson, too, wanted to do science by political means, using the second Trojan horse strategy? How would that have been engineered? Let us take

a second look at the publicity campaign around *Sociobiology*. Wilson had put in several years' worth of work in that tome, including learning to master the (for him) new field of vertebrate biology. Obviously he wanted the world to know about and appreciate his synthesis. Just like other academics, he wanted recognition.

As a matter of fact, there is a Trojan horse theory involving Wilson as well. There are actually those who suspect that Wilson's last chapter on humans was deliberately included to generate scandal, and through this, interest in *Sociobiology*. For instance, some biologists I interviewed in 1981 at the International Ethology Congress suggested that "Wilson wanted to make a splash", which might be interpreted along these lines. The critics of Wilson, of course, registered all the publicity surrounding the book: the first-page article announcing the book in the *New York Times*, an early review in *The New York Times Book Review*, and a vast pre-publicity campaign, all of which they unhesitantly gave a political interpretation (Alpert *et al.*, 1978; Beckwith, 1981-82). But the publicity campaign around *Sociobiology* can be given a different spin. Allan Mazur (1981) suggested that *New York Times* science journalist Boyce Rensberger's provocative suggestion in his first-page article – that Wilson's last chapter about humans was bound to create controversy – came to act as a self-fulfilling prophecy (Mazur, 1981). In other words, *someone somewhere* would be sure to react, and *voila*, the desired controversy would emerge!

That interpretation is interesting, but does throw a strange light on the actions of the fierce critics of sociobiology in the Sociobiology Study Group. Did they, in fact, with all their spontaneous anti-sociobiological campaigning, simply fall into a ready-made trap set by master strategist Wilson (with Harvard University Press and Boyce Rensberger)?

There is one difference, though, between Gould and Lewontin and Wilson in regard to this second Trojan horse model. *Sociobiology* would probably have sold well enough even without the controversy – just like Wilson's *The Insect Societies*, another large coffee table book, published in 1971 with the same press. This is suggested by the praise Wilson got for *Sociobiology* from many of his biological colleagues, who greatly appreciated his synthetic effort.[11] In other words, Wilson did not really *need* a scandal – although nobody doubts that the controversy greatly boosted the sales of his book and the spread of the gospel of sociobiology. And no doubt largely because of the controversy around his book and the name he gave the field, it was Wilson that now in the eyes of the world became identified with 'sociobiology.' (This was rather irritating to many of the British behavioral ecologists and functional ethologists who had been actually *doing* "sociobiology" for years – however, restricting themselves to animals).

Now what happens if we apply Trojan horse II simultaneously to both sides in the sociobiology debate? We get a situation where scientists on both sides in a controversy are doing their best to create *political* scandal in order to promote their *scientific* agendas. We would here have an extraordinary situation of *synergy* between the critics of sociobiology and their target – all while the world thought they were at each others' throats! This is a

model of "science by political means" pushed to the extreme. It is hard to believe that this would really have been the case at the outset of the sociobiology controversy. But something like this did seem true for a later stage of the debate, which I have characterized as a symbiosis (Segerstråle 2000, ch. 3 and 16).

Russian Dolls

In reality I see in the debate something like a nested hierarchy of Trojan horses, a type of Russian doll situation, with both sides in the controversy pursuing their particular moral-cum-scientific agendas.

I'll start with Wilson and give some examples. As can be gleaned from *Sociobiology*, *On Human Nature*, and particularly a talk he gave to a group of theologians at a Star Island conference in 1980, Wilson's early ambition was to provide a materialist alternative to religion with the help of evolutionary biology (Wilson, 1980b). He wanted to derive values from nature to provide an alternative to the theologians' moral teachings. What is more, he hoped to be able to derive a trajectory for mankind's future – as a substitution for divine prophecy – with the help of a set of formulas involving population genetics, the "hardest" branch of evolutionary biology. So Wilson's ambition to make sociobiology as quantitative and mathematical as possible (a move which irritated particularly Lewontin) was actually aimed to serve the moral agenda contained in his inner Russian doll. His other great ambition, his epistemological quest for uniting the natural and social sciences (which irritated especially social scientists) was, again, ultimately serving the moral and practical goal of effective management of the Earth as a whole (this became clearer with *Consilience*, 1998).

And if we follow Wilson's development over time, we might actually reconstruct his career as a series of Trojan horses. Wilson I, the "bad" sociobiologist over time emerged as Wilson II, the "good" environmentalist, known to a new generation of academics (particularly Wilson, 1992). Did this mean that Wilson later gave up on sociobiology? Not at all. Sociobiology was nested inside environmentalism, more closely in the form of "biophilia," our natural affinity with nature (Wilson, 1984). And sociobiology also appears as a Russian doll inside his recent idea of "consilience" or unity of knowledge (Wilson, 1998). Wilson's model for the unification of knowledge has at its core exactly the coevolutionary approach from *Genes, Mind, and Culture*, a mathematical explication of a new version of sociobiology that Wilson co-wrote with Charles Lumsden in 1981.

But let us now take a peek into the inner one of Gould's and Lewontin's Trojan horses. Here we have a scientific truth (anti-adaptationism) which contains inside itself a moral/political truth. Gould's and Lewontin's fear was that "vulgar adaptationism" would give the innocent layman wrong ideas about everything in society being for the best. That kind of biological Panglossianism would be easily interpreted as a support for the social status quo as the best of all possible worlds. Indeed, these critics seem constantly to have had the implications for *human* society in mind when they discussed adaptation and adaptationism. The following quote beautifully captures Lewontin's anti-adaptationist moral-cum-scientific belief. It is hard to

believe that he is talking strictly biology when he says:

> The truth is that evolution has taken and is taking place and that it is often direct natural selection for particular character states that is responsible for differences between species. But it is also true that *some significant fraction of evolutionary change has occurred without creating the best of all possible worlds.* It is true that "many are called but few are chosen," but it is equally true that "the race is not to the swift nor the battle to the strong nor yet bread to the wise … but time and chance happeneth to them all (Lewontin 1981, italics added)

And Gould has in a similar spirit over the years developed a number of objections to adaptation, from punctuated equilibria (Eldredge and Gould, 1972; Gould and Eldredge 1977), to the notion of "exaptation" (Gould and Vrba, 1982), and to the emphasis on "contingency" in evolution (Gould, 1989, 1996).

The Sociobiology Controversy: A Preparation for the Human Genome Debate

We have, then, two basic types of Defenders of the Truth. There are the naturalist sociobiologists, who think it is useful to consider hypothetical genes "for" behavior in their evolutionary models, and the critics who protest that no such genes have ever been seen and should not be speculated about – even in the case of animals, but particularly in regard to humans.

The battle continues, among the Defenders of the Truth. Meanwhile, many of the defenders are bestselling authors, and continue getting prizes and honorary doctorates. Wilson has no doubt won a victory: he is increasingly internationally celebrated. But his enemy Gould is also extremely popular. In 1999 Gould was elected the president of the American Association for the Advancement of Science; his books are popular bestsellers among the general public. And Gould has a quite different message for the public than Wilson, with conclusions that are comforting for many. Obviously, many truths can be defended at the same time! Controversy pays.[12]

What neither Wilson nor his opponents Gould or Lewontin are doing, though, is to *disconnect* science from values. That is what Thomas Henry Huxley, "Darwin's bulldog," tried to do with Darwin's theory. He said that we should work at the social level with the help of education to combat any seemingly negative implications from biology. But this is forgotten by both sides in the American sociobiology debate. On the other hand, the disjunction between science and values is the typical "default position" of British "sociobiologists" (they dislike the name!) such as Richard Dawkins and John Maynard Smith. (13)

I believe that we should see the sociobiology debate as a kind of "dry run" for the important discussion that is now emerging around the human genome – both its scientific and moral and political aspects. Much of the forthcoming discussion will have to do with the extent to which genes can really be said to represent "blueprints" and how much predictive value there is, in fact, in knowing the genetic makeup, without detailed information about how these genes get actually expressed in a particular individual. (For a detailed discussion of the problems involved here, see Bateson and Martin, 2000). But this was *exactly* what both

Francis Collins and Craig Venter, the collaborators on the final stretch of the Human Genome Project, were stressing repeatedly in the media on June 26, 2000, when they announced that the end of the genome project was in sight. Additionally they – and also President Clinton and British Prime Minister Tony Blair – emphasized the urgent need for legislation so that people cannot be discriminated against on the basis of their genetic makeup. They also pointed to the need for a health insurance policy that is fair and available to everybody. In other words, they seemed to be addressing head-on many of the typical objections that had been raised by critics of research in the genetic basis of human behavior.

It seems to me that the sensitivity to these kinds of issues on the part of scientific and political leaders surely has *something* to do with the intense debate around sociobiology over the last quarter century. The arguments pro and con in regard to the hypothetical genes in the sociobiology (and IQ) debate was a sort of rehearsal for the real debate about the real genes to be eventually identified by the human genome project. Whatever new, detailed genetic knowledge the genome project will bring about (and here we have an opposition between optimists such as Wilson (e.g., 1998) and skeptics, such as Lewontin (e.g., 2000 a,b), one thing is clear. The sociobiology debate served to remind the public that cutting edge science has sometimes been wrong: it brought to academic and public attention the potential problems of discrimination and the consequent need for appropriate social measures. The critics of sociobiology can regard it as a recognition for their efforts that the leaders of the genome project felt the need to recommend protective legislation and policy measures in the same breath as they announced their progress, and that Bill Clinton and Tony Blair, too, voiced this kind of concerns.

The sociobiology debate itself, as scaffolding for the future genome discussion, may actually have been the biggest Trojan horse of them all.

Notes

1 For instance, he downplayed the importance of IQ for social success and declared (with many other biologists at the time) that race was not a biologically useful characteristic.

2 Wilson had "inherited" many of these concerns from the 1960s discussion about the conflict between technological and human evolution, reflected in the thinking of Konrad Lorenz, Jacques Ellul, and others.

3 This was also connected to a type of dialectical Marxist thinking (see e.g., Levins and Lewontin, 1995).

4 For instance someone like Nobel laureate David Baltimore, who got in trouble in the so-called Baltimore affair (cf. Segerstråle, 1993) may have had a similar picture of science proceeding in bold leaps rather than petty detail.

5 Holton (1978) discussed a number of synthesizers before Wilson with ambitions on an even grander scale.

6 In *On Human Nature* (Wilson, 1978) it became rather clear that one reason for Wilson's "evangelist" style was that he actually had a bone to pick with established religion. He was raised as a Southern Baptist, but later became disenchanted it. His critics, however, interpreted even his explicit critique of the Church as having a conservative sociopolitical intent (see Segerstråle, 2000, ch. 3).

7 Meanwhile, animal behaviorists typically regarded the first and last chapters on humans in Wilson's book as a *distraction* from the interesting studies and theories regarding animal social behavior – in other words, they had a different type of "sandwich model."

8 Actually, the correct architectural term for those spaces is "pendentives," not spandrels, according to Daniel Dennett (1995).

9 No doubt the opposition to sociobiology also came in handy for organizing the academic left around a common cause.

10 Interestingly, from his own description of the paper, Gould himself seems to believe that also the original oral presentation of the scientific criticism required a number of extrascientific props (esthetic, moral, and so on) in order to be effective; that is why he mobilized spandrels and Candide (Gould, 1993). It was Gould who was the main author of the paper and the one who delivered it at the Royal Society Symposium (for more about this, see Gould, 1993 and Segerstråle, 2000, ch. 6).

11 In 1989 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* " (Wilson, 1994, p. 330-1)

12 And now with the emergence of "evolutionary psychology" there is a new round in the controversy Evolutionary psychology has also been duly criticized, largely along the same lines as sociobiology before and often by the same people (e.g., Rose and Rose, 2000). Still, evolutionary psychology has tried self-consciously to distance itself from sociobiology, by emphasizing that it is dealing with the evolved architecture of the human mind, and is interested in human universals. There is one difference that it recognizes, though, and that is male female differences. (The "Bible" of evolutionary psychology is Barkow, Cosmides and Tooby, *The Adapted Mind*, 1992, but there are also a number of more popular evolutionary psychology books).

13 In Segerstråle (2000), ch. 19, I distinguish between what I call a "regular" and a "hyper" Enlightenment quest. Representatives of the latter insist on pursuing scientific and moral/political truth at the *same* time; representatives of the former keep these ambitions apart.

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