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of confusing the origins with the content of a scientific theory[4]. Any student of sociology or philosophy of science is likely to be intrigued by the social framework of the emergence of sociobiological theory over the last decade and its relationship to the New Right ideology[5]. But having explored that link, it is the task of critics of sociobiology to expose what they see as its methodological, *internal* inadequacies (for example, refs 6 and 7).

If sociobiologists want to avoid the charge that they believe that biology is destiny, they should beware of telling magazines that they know "why we do what we do" or entitling their books *The Selfish Gene, The Inevitability of Patriarchy* or *On Human Nature*. The trouble is that they want to have their cake and eat it. They imperialize the human sciences (*vide* the first paragraph of *Sociobiology, The New Synthesis*) and are embarrassed by the outcome. This is why the final chapters of Dawkins' or Wilson's books are so confusing. Having set out the inexorable destiny of genetic predispositions to xenophobia, aggression, patriarchy or whatever, they invoke the possibility of a human conscious prospect of overcoming these predispositions. As Dawkins puts it in his letter, with respect to philandering males "many humans (have) some at least momentary intention of overcoming their polygamous tendencies. Many even succeed in this". For Wilson euphenics may overcome any "hereditary tendency" to acquire xenophobia. Free will, intentions and wishes (or Dawkins' memes) like the US cavalry, come galloping over the horizon in the nick of time to rescue us from our genes.

But *where* does our free will, etcetera come from? How can we be *both* genetically programmed robotic DNA survival machines and have the extraordinary capacity to transcend these programs? The truth is that sociobiological determinism, when challenged, collapses into the weakest sort of Cartesian dualism. For consistent materialists — like Gould, whom Dawkins quotes, and, I hope, myself too — we must argue that wishes, intentions etc. are as much, or as little, "given" by our genes as any other aspect of our human existence. The interesting thing about humans is that the fact that we can change what we do is as much part of our biology as how we do what we do. To give the example used in my review of Wilson — if humans were quadruped and not biped, their social arrangements would be different; that humans are biped is genetically "given", therefore the human social order is genetically given. The point is that such genetic syllogisms are boringly uninteresting about either any of the crucial aspects of differences *between* human societies or the changes that occur *within* any given society. They neither explain nor predict apartheid in South Africa, cultural revolutions in China, Born Again Christianity in the United States, the welfare state or its dismantling under Mrs Thatcher in Britain, still less any of our individual proclivities.

On such questions then, biology has to be silent. It is because self-styled sociobiologists are *not* silent (I challenge anyone to read *The Selfish Gene* and come away *without* a clear impression of Dawkins' views of what biology has to say about the Welfare State, sexual mores or microeconomics) that their work both trivializes important social and biological questions and is so amenable to neo-Nazi and New Right ideology.

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Conservation sites

SIR — The attack on conservation sites by Muir (*Nature* 12 March, p.82, 173), apart from ignoring that the scientific value is by no means the only value considered by conservationists, is based on several unacceptable assumptions.

In implying that conservation sites are primarily for the preservation of rare species Muir ignores the fact that most have been primarily designated in order to conserve particular vegetation types, associations of organisms, natural or semi-natural ecosystems. The argument that "value to science of any objects or phenomena lies not in themselves but in the information they yield to study. Once this information is recorded and published whatever value remains in the objects or phenomena is of no value to science" is ridiculous on two counts.

First, he specifies no criteria of "value" that are meaningful. He merely transfers the problem of what he means from conservation sites to published information about them. Even if one disregards this problem one is left, on his own arguments, with a strong case in favour of conservation sites. Ecology is still a young science and it has hardly scratched the surface in its study of the vast majority of ecosystem types. The rate of ecological advance does not compare favourably with the rate of loss of certain habitat types. There are, as yet, no vegetation types or ecosystems whose study has been exhausted to the extent that they are of no further interest to ecologists!

I am responsible for the "sites of special scientific interest" that make up the Malham Tarn Nature Reserve, one of the best documented nature reserves in the country. Its scientific documentation started with observations by John Ray in 1671 and has rapidly accelerated since 1947. We now know enough to realize that it will require much more study than that already accomplished before any scientist would be other than foolish to suggest the site was of no further interst to science.

The tragedy of the present rate of loss of sites of special scientific interest is that we know so little of what is being lost. What little we do know strongly suggests that the Nature Conservancy Council have correctly identified the sites of greatest interest to science apart from them being those necessary if we are to achieve the conservationists' aim to convey the maximum diversity of wildlife into the next century. The conservationist is not, as Muir implies, trying to halt evolution. He is primarily concerned with the perpetuation of a diversity of habitat types. A variety of forest organisms depends on a variety of forest habitats — regardless of the rates of evolution of their organisms.

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DMSO and immunity

SIR — The observations of Pestronk and Drachman[1] that dimethyl sulphoxide (DMSO) reduced anti-receptor antibody titres in experimental myasthenia gravis in rats are of considerable interest. The authors conclude: "It seems likely that DMSO might also be effective in ameliorating other active humoral immune responses. If so, DMSO may provide an effective new treatment for immune diseases mediated by autoantibodies." Lest this statement lead once more to proclamations of DMSO as a wonder drug, several important points need an answer.

The LD_{50} of DMSO by intraperitoneal (i.p.) injection is between 10 and 11 per kg for rats and mice. Pestronk and Drachman injected 1.0 ml of DMSO into rats daily for 14 days. Assuming a body weight of 250 g for a rat the dose of DMSO for each rat would be approximately 4 g per kg, or 36 per cent of the acute LD_{50}. The injection (i.p.) of this amount of DMSO into rats and mice produces a number of pharmacological and biological effects including an increase in peripheral blood pressure, changes in the oxygen levels, probable anoxia in the spleen and hypothermia[2,3]. In mice the level of hypothermia is severe, with rectal temperature reaching 33.5°C and recovery after a single injection of DMSO (4.5 g per kg) takes more than 6 h. Hypothermia represents a generalized but reversible toxicity. Many chemicals when injected at levels of approximately 20–40 per cent of the LD_{50} produce similar effects.

Thus the clearly demonstrated effects of DMSO in decreasing antibody titres illustrated by Pestronk and Drachman may not be specific. In any event the possible therapeutic usefulness of a chemical effective at doses so close to the LD_{50} is unlikely to prove clinically acceptable.

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SIR — The comments of Professor Ashwood-Smith should serve as a useful reminder that the pharmacological actions of dimethyl sulphoxide must be carefully examined. The mechanism of action by which the drug suppresses anti-acetylcholine receptor antibodies in the experimental animal model of myasthenia gravis is not yet understood. Moreover, the reduction in serum antibody levels must be taken into account as a possible undesirable effect if the drug is contemplated for other clinical applications.

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