Biology and the Behaviour of Man

Do we really need another critique of sociobiology? In general, probably not, but perhaps we need this one. Kitcher, like everyone else, approaches the problem with prejudices, but he tries harder and more successfully than most to rise above them. Prejudices are inevitable. It is natural for geneticists and evolutionary biologists to hope that their disciplines will throw new light on the human condition, and equally natural for social scientists to resist the threatened takeover. More important for many of us, previous efforts to apply biology to human affairs have too often ended up as justifications for racial, sexual and class inequalities. Kitcher, who grew up in England, has not forgotten that, in the post-war years, schoolchildren were divided at the age of eleven into sheep and goats, and that this division was justified by the leading experimental psychologists of the day. He and I share this experience – he as a tested child and I as a parent of tested children. It has left us cautious about proposals to use biological theory to plan human institutions.

Kitcher, then, is unsympathetic to the claim that evolutionary biology can guide political judgement, and I suspect he was unsympathetic before he started work on this book.
Unlike some other authors, however, he has undertaken a genuine study. He does understand the ideas he is criticising. He has the biological knowledge to evaluate the evolutionary background to sociobiology, and the mathematical ability to analyse the claims made for it. Above all, he presents sociobiology in its strongest and most coherent form, and avoids the easy option of attacking only its more idiotic manifestations.

He distinguishes sharply between the attempt to understand the evolution of social behaviour in animals, and attempts to understand man. He is sympathetic to the former enterprise. Correctly, he points out that there is no special underlying theory: 'There is no autonomous theory of the evolution of behaviour. There is only the general theory of evolution.' It may be that interactions between relatives, and frequency-dependent fitnesses, were more important in the evolution of the behaviour of birds than in the evolution of their wings, but they are not peculiar to behavioural evolution; kin selection and game theory are just as relevant to plant evolution.

There is, of course, good and bad work in animal sociobiology, and Kitcher gives examples of both. The bad, he points out, has two characteristics: data are quoted as supporting some specific hypothesis, without considering alternatives, and the hypotheses themselves are modified after the fact until data and predictions are brought into line. However, his chapter 'Dr Pangloss's Last Hurrah', which takes issue with the 'adaptationist program', seems to me only partly correct. He presents two genetical reasons for not expecting perfect adaptation. The first is that there are genetic systems, even with constant fitnesses, in which selection will not fix the fittest genotype. The simplest is that of heterozygous advantage: if $Aa$ is fitter than $AA$ or $aa$, selection cannot produce a population consisting entirely of $Aa$ individuals. This is of course true, but is it interesting? If we want to understand why some species does not have the phenotype predicted by theory, this kind of genetic detail is rather unlikely to be the reason. Suppose, for example, we are interested in the shape of vertebrate wings. Aerodynamic theory shows that the optimal shape is usually