

## Sociobiology: a reply

WE are sad and disturbed at the tone of the article by Robert May (*Nature*, **260**, 390; 1976) on E. O. Wilson's *Sociobiology: The New Synthesis*. A widespread debate, originating in America, is taking place around this book and its adoption as a text for college courses. May's article attempts to sidestep the issues being raised by this debate and to remove the discussion to the rather more traditional and secluded arena of scholarly exchange. The term 'debate' in this context is, we recognise, distasteful to many scientists with a traditional view of science and we use it advisedly. We feel, however, that the effects of science upon everyday thought and upon political doctrines are issues that cannot be resolved by courteous sword-crossing within the language of 'pure' science itself.

On the contrary, the issues raised evoke the deepest feelings about power and its abuse, and about social injustice in ways that inevitably force a confrontation between individuals and groups holding disparate sets of values. We must learn to see what May describes as 'a certain incivility' as a function of the importance of such debates/confrontations. Since consideration of the social bases and the social effects of science should be recognised as being integral to science and its practice, such impassioned and political arguments should appear more regularly in the columns of *Nature* and *Science*.

More importantly, May's focus on the so-called incivilities deflects attention from the serious nature of the issues posed by the publication of Wilson's book. The group of American critics (the Sociobiology Study Group of Boston Science for the People) to whom he refers, see in Wilson, essentially correctly we believe, a prime example of the scientific specialist (the naturalist in the broad classical sense) whose prolonged study of the bee and the ant have fostered in him a rather conservative expectation of the human capacity and potential for change. While such conservatism may well be largely unconscious and not overtly political in expression, untold damage is wreaked on 'informed' people's capacity to think critically about social institutions. By attempting to fit *Homo sapiens* into his evolutionary biological schema Wilson jeopardises any attempt to change present patterns of social inequality. As such it seems to us that 'a certain incivility' is more than reasonable.

Wilson's American critics have raised important objections to his work. They show how his case is based on specula-

tion. He postulates genes, or groups of genes, for such traits as spite, military discipline, blind faith and indoctrination. He uses metaphors from human societies to describe animal societies and in so doing imputes barter, religion and magic to animals. He closes off the theory from tests against the real world by postulating, for example, multiplier and threshold effects which make every observation fit the theory. May attempts to belittle this critique: "To suggest, for example, that the evolutionary considerations which determine the mating systems of mammals and birds have any light to shed on the tensions and asymmetries observed in human sexual relationships is [according to May] to invite reflexive dismissal as a 'sexist'!" Tensions and asymmetries are delicate words indeed to express centuries of economic and social exploitation of women. And in fact, Wilson does try to establish the inevitability of sexism by asserting it has a genetic basis. In an article in the *New York Times* (October 12, 1975) he states concerning the division of labour between the sexes: "This strong bias persists in most agricultural and industrial societies and on that ground alone appears to have a genetic origin . . . My own guess is that the genetic basis is intense enough to cause a substantial division of labour even in the most free and most egalitarian of future societies . . ."

That our society should continue to be so susceptible to such arguments expresses both a past and present deference to expertise in general and a deference of social science to biology in particular. At present we learn in school level biology courses and thereafter the roster of emergent properties that distinguishes life from inorganic matter. Life transcends the purely physical and chemical laws that govern inorganic matter, although matter in motion remains its physical basis. We must ask why, in our society, the human sciences cannot emancipate themselves from biology in the same way that biology is emancipated from the physical sciences. We are unique animals who, while having a physical basis in organic life, transcend it through language and culture.

The final issue is, of course, how does the cultural *status quo* change? It seems obvious to us that social change occurs when humans set about self-consciously to change unacceptable oppressive and outmoded social conditions.

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ROBERT MAY REPLIES: The above letter makes plain the differences of opinion between its authors and myself as to the place of passion and politics in science. In particular it is true that I represented the controversy over the political significance of *Sociobiology* as only a small (and unfortunate) corner of a large canvas; I think that is the balanced view.

It should be emphasised that, unlike other recent controversies, the dispute over the political implications of *Sociobiology* is not a clash over substantial and well-defined issues. The Science For The People people do not attack Wilson's book but rather their own chimaerical version of it; in Wilson's words they "furnish me with a political attitude I do not have and the book with a general conclusion that is not there". Readers in any doubt on this score should see the recent exchange between the US Science for the People Study Group and Wilson in *BioScience* (**26**, 182-190; 1976) and the impartial news article in *Science* (**191**, 1151-1155; 1976). □

## Panacea for injured nerves

from a Correspondent

WHEN a nerve innervating a muscle is cut the effect on the postsynaptic cell is profound. The muscle atrophies, as shown by a loss of mass and of proteins, there is a marked reduction in the cholinesterase activity present in the tissue, and there is a considerable increase in the number of extra-junctional receptor molecules. Since the onset of these effects is related to the length of the nerve stump left associated with the tissue, and since they can be duplicated by blocking release from the nerve terminal, it has been proposed that nerves exert trophic influences on the tissues they innervate (Guth, *Physiol. Rev.*, **48**, 645; 1968). Considerably less attention has been focused on a trophic effect in the opposite direction, from tissue to nerve, although this possibility was first implied 18 years ago (Brown, *J. Physiol., Lond.*, **142**, 7p; 1958). This situation is now being rectified.

Two approaches have been used to study this suggested transfer of information from the post to the presynaptic cells. First, morphological techniques have been used to examine the effects of damaging the postsynaptic cell on presynaptic or synaptic ultrastructure and, second, biochemical correlates to these structural changes have been sought.