CORRESPONDENCE

Genes and race

SIR — I write in reply to Dr Steven Rose's letter (*Nature* 22 January, p.335), which drew attention to the fact that a right-wing journal, *New Nation*, has quoted me, together with other evolutionary biologists, in support of their view that our genetic constitution makes it impossible for us to live in a racially integrated society. I welcome the opportunity to say that there is nothing in modern evolutionary biology which leads to this conclusion.

JOHN MAYNARD SMITH

University of Sussex,
Falmer, UK

Museum debate

SIR — I have been following the "great museum debate" in your pages with a profound sense of detached amusement. But as matters are quickly reaching a level of absurdity that may inspire me to write the 15th Gilbert and Sullivan opera, and as I am, in a sense, the focal point for Halstead's glorious uproarious misunderstanding, I suppose I should have my say.

Halstead began all this by charging that the venerable Natural History Museum is now purveying Marxist ideology by presenting cladism in its exhibition halls. The charge is based on two contentions: (1) a supposed link between the theory of punctuated equilibrium, proposed by Niles Eldredge and myself, and cladistic philosophies of classification; and (2) an argument, simply silly beyond words, that punctuated equilibrium, because it advocates rapid changes in evolution, is a Marxist plot. For the first, there is no necessary link unless I am an inconsistent fool; for I, the co-author of punctuated equilibrium, am not a cladist (and Eldredge, by the way, is not a Marxist, whatever that label means, as if it mattered). Under cladism, branching events may proceed as slowly as the imperceptible phyletic transitions advocated by the old school. Punctuated equilibrium does accept branching as the primary mode of evolution, but it is, fundamentally, a theory about the characteristic rate of such branching — an issue which cladism does not address.

For Halstead's second charge, I did not develop the theory of punctuated equilibrium as part of a sinister plot to foment world revolution, but rather as an attempt to resolve the oldest empirical dilemma impeding an integration of palaeontology into modern evolutionary thought: the phenomena of stasis within successful fossil species, and abrupt replacement by descendants. I did briefly discuss the congeniality of punctuational change and Marxist thought (*Paleobiology*, 1977, p.145) but only to illustrate that all science, as historians know so well and scientists hate to admit, is socially embedded. I couldn't very well charge that gradualists reflected the politics of their time and then claim that I had discovered unsullied truth. But surely Halstead, who has done some statistics in his day, knows that correlation is not cause. If I may make a serious point: I grew up frightened in a leftist household during the worst days of McCarthyism in America; and I know that what seems peripheral or cranky today can become a weapon tomorrow (consider the current creationist surge in America). May we avoid red-baiting; it may not always be harmless.

I saw the cladistic exhibits last December. I did not care for them. I found them one-sided and simplistic, but surely not evil or nefarious. I also felt, as a Victorian aficionado who pays homage to St Pancras on every visit to London, that most of the newer exhibits are working against, rather than with, the magnificent interior that houses them. But I would not envelop these complaints in ideological hyperbole; Halstead has said enough.

STEPHEN JAY GOULD

Museum of Comparative Zoology,
Harvard University,
Cambridge, Massachusetts, USA

Last word?

SIR — It is a little late in the day for Stephen Gould to try and come the innocent. If he did not want "Halstead's glorious uproarious misunderstanding" to get under way, he should have avoided dragging Engels on to his side in the first place[1]. Mind you, even this might not have worked, for the "political" implications of punctuated equilibria have not gone unremarked, as some of the recent correspondence has made manifest.

I have taken Engels and Lenin as my main sources on dialectical materialism, which I have sought to apply in the conduct of my own researches. There is only one point, but this is fundamental, at which I would part company with Marxism and that is the nature of qualitative changes, which I see not as sudden leaps but as gradual in the tradition of Charles Darwin. I have recently been non-plussed to learn from Stephen Gould that "many orthodox Marxists have been quite content with Darwinian gradualism" (ref. 2). This is equivalent to someone, who insists that Christ was a myth, being considered an orthodox Christian. But perhaps in the United States, I would be deemed an orthodox Marxist!

When it comes to the cladists with their punctuated tendencies, we run into a perfectly ludicious source of semantic confusion. If, as some of the correspondents have insisted, cladistics is concerned only with pattern and not process, then obviously there is no point in arguing further, because I am concerned primarily with process. I wrote about the kind of classical Hennigian cladistics actually being presented in the public galleries of the British Museum (Natural History) and clearly explained in their accompanying booklets, and not the new transformed variety of Patterson[3] and others.

Tempting though it may be, I am sufficiently modest to decline the mantle of oracle proffered by Rosen[4] with regard to the origin of *Homo sapiens* from *Homo erectus*. I claim no special insight in these matters but merely reported the considered and published consensus of the staff of the British Museum's own Sub-Department of Anthropology. Wood[5] has drawn attention to the "dubious academic practice" of ignoring "uncomfortable" evidence — the scandal of this is that it was deliberate, involving, as it did, the overruling of the museum's own experts. Critical scientific evidence is being deliberately withheld from the public who are, in consequence, being seriously misled as to the nature of "Man's place in evolution". All is apparently being subjugated to a chosen dogma. I have been roundly abused for implying that a far left political connection might be involved, but the present dogmatic policies seem to bear its unmistakable stamp.

The British Museum (Natural History), London, is a major public scientific institution in this country and as such should be accountable to the public. It is surely reasonable to expect the Director to answer the charges that have been levelled — a continued "dignified silence" in the present circumstances is simply not good enough.

L. B. HALSTEAD

Departments of Geology and Zoology,
University of Reading, UK

1. Gould, S. J. & Eldridge, N. *Paleobiology* 3, 225-251 (1977).
2. Gould, S. J. (personal communication).
3. Patterson, C. *Nature* **288**, 430 (1980).
4. Rosen, D. E. *Nature* **289**, 8, 105 (1981).
5. Wood, B. A. *Nature* **289**, 8 (1981).

Badger controversy

SIR — On 11 December 1980 the Mammal Society published a letter in *Nature* drawing attention to some of the many anomalies in the Zuckerman report. The reasons we started an open discussion of the report are obvious. Badger gassing started in 1975; nearly six years later, after the expenditure of a great deal of time, money and effort, it is our belief that there is no unequivocal evidence that the present gassing policy is likely to produce a long-term solution to the problem. We suggested that the Zuckerman report is one-sided in its interpretation of the evidence, and that the conclusions and recommendations in the report are too categorical and do not take sufficient account of the complexity of the problem.

Following our original letter, three letters have been published in *Nature*. Dr Plowright (1/8 January 1981, p.8) presented no new information, nor did he answer any of the points we raised, and so his letter will not be discussed further. Dr Yates (22 January 1981, p.218) questioned a graph sent to Lord Zuckerman for his comments. This graph was simply intended to show that the rate of decline of TB was similar in the South West to that elsewhere in England. That the incidence of TB was different in the two areas was never disputed. Dr Yates published an alternative graph, which was simply another presentation of the same data; it showed that the incidence of TB varies in different parts of the South West. That is the very point we stressed in our original letter. Dr Yates' graph also showed that there was a decline in TB in all the regions sampled, irrespective of whether badger gassing was carried out in that area. That is the only point our original graph was designed to demonstrate.

In fact Dr Yates' graph has highlighted the

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