

CORRESPONDENCE

Badgers and TB

SIR — I refer to the letter by Dr Stephen Harris in your issue of 11 December 1980 (p.532) which, while critical of certain aspects of Lord Zuckerman's Report to the Minister of Agriculture on "Badgers, Cattle and Tuberculosis", agreed with his basic conclusion that the badger is a major reservoir of bovine tuberculosis (TB) in some areas of South-West England. The main criticism was that, while the reservoir constitutes a potential danger to cattle in limited areas, there is no "scientific evidence" to justify the deduction that the level of tuberculous infection in badgers in the South West is "dangerously high" and may result in spread of the disease to adjacent populations.

Since Dr Harris writes for and on behalf of the Mammal Society, it may be appropriate to ask whether all the interested members of that society would fail to be convinced by the evidence. More than 4,000 badgers were examined in connection with official investigations in the years 1971–79 and 14 per cent at least harboured *Mycobacterium bovis* (Report: para. 130). Of animals found dead since 1972 in fields, woods and farms in Gloucestershire, Avon and Wiltshire, 54/194 (28 per cent) were found to be tuberculous. Of those killed in Gloucestershire, predominantly on the roads and presumably by chance, during this period 1976–80, 21/232 (9 per cent) were tuberculous; this admittedly was the highest recorded prevalence by county but for the whole of the South West the figure was still 4.2 per cent (Report; Table 3).

These figures should be a cause for concern when dealing with a social animal which occupies, for much of the time, confined spaces underground and whose movements above ground favour between-group contacts. Bovine tuberculosis in badgers is sometimes a rapidly progressive, lethal disease, the causal organism probably being excreted by many routes in large numbers^{1,2}. It has, therefore, ample opportunities for transfer and by any objective epidemiological standard the risk of spread to neighbouring badger populations must be considerable. The onus for showing that this is not a reasonable interpretation should rest on others.

The argument advanced by Dr Harris, that the recent decline in the incidence of tuberculosis in cattle in the South West is simply a reflection of a general downward trend in the figures for England as a whole, misses the point that the incidence of herd "breakdowns" in the South West, in the years 1974–80, has never been less than five times that for the rest of England (Report; Table 19).

Many anomalies in the data were highlighted in the Report, which pointed out

the need for continuing investigation and research. In this connection it is difficult to see how the "subtle factors" twice mentioned by Dr Harris as significant in the spread of the disease from infected badgers, could be elucidated without extensive sampling on a statistically significant scale. In particular, continuous monitoring of badgers on the perimeter of heavily infected areas would be necessary to prove that the disease is *not* spreading (Report; para. 137). It is to be hoped that consideration will be given by interested bodies as to how this could be organized, if the necessary funds were provided.

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1. Gallagher, J., Muirhead, R.H. & Burn, K.J. *Vet. Rec.* **98**, 9-14 (1976).
2. Gallagher, J. & Nelson, J. *Vet. Rec.* **105**, 546-551 (1979).

Museum policy

SIR—The broader implications of cladism are not my special concern, but the study of human evolution is. Cladistics is a useful research tool, but I support L.B. Halstead's¹ objections to the way it is used in the new exhibit *Man's place in evolution* at the Natural History Museum.

Cladistic analysis is still a relatively untried method for expressing relationships between groups, and technical details of its application are still being debated. It was devised as a conceptual framework within which one aspect of the relationships between modern and fossil forms could be analysed. It relates groups solely on the basis of shared features, and rejects the notion that any ancestral relationship can be inferred from this evidence; such relationships are considered "unknowable". Proponents claim that only by adhering to these principles can palaeontologists frame testable hypotheses. The strengths of the method are its simplistic rigour, but therein also lie its weaknesses. In order to provide an unambiguous framework, certain assumptions have to be made. For example, speciation is recognized only as a dichotomous branching event, and more complex adaptive radiations, anagenesis and convergent evolution of morphological features are discounted. Such assumptions are unrealistic. The concept of "relatedness" enshrined in cladistics is therefore fundamentally different from the general one which implies a lineal relationship, and as used in the exhibit, it is likely to confuse, if not actually mislead, the visitors.

The contents of the exhibit also give cause for disquiet. The authors seem particularly confused about what constitutes the "habiline" and "australopithecine" groups of early hominids. Two important specimens "1470" and KNM-ER 1813, have been seriously misassigned. KNM-ER 1813 is consistently cited as an example of a "habiline" and "1470" as an "australopithecine". Yet, "1470" has a cranial capacity of around 775 cm³ and is included in *Homo habilis* by many workers.

The cranium KNM-ER 1813 is smaller overall, and it has a cranial capacity of between 500 and 550 cm³. It has not yet been formally assigned to a taxon but its position is sufficiently enigmatic for it to have been compared, by some authors, to *Australopithecus africanus*, and by others, to early *Homo*. Howell² has attributed KNM-ER 1813 to *Homo habilis*, but he also includes "1470" in the same species.

The section which deals with *Homo erectus* also deserves comment. Recent, and well known, discoveries in Europe and Africa have demonstrated that a much wider range of cranial morphological features is subsumed into the "erectus" group than was previously believed. Several skulls shown an apparent mixture of classic "erectus" and archaic *Homo sapiens* features. These new finds strongly suggest a continuous gradation of morphology from "erectus" to "sapiens". The exhibit totally ignores this evidence.

To attribute important finds wrongly is careless; to ignore "uncomfortable" evidence is dubious academic practice, and to imply that cladism is an orthodox systematic approach is irresponsible. The contribution of the Natural History Museum to science and education is too important to be compromised in this way.

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1. Halstead, L.B. *Nature* **288**, 208 (1980).
2. Howell, F.C. in *Evolution of African Mammals* (eds Maglio, V.J. & Cooke, H.B.S.) 154-248 (Harvard University Press, Cambridge, 1978).

SIR — In a recent letter to *Nature* (288, 208; 1980) L. B. Halstead states his convictions that human evolution was gradual, that its gene pool of the past had certain knowable characteristics, and that the ancestry of a living species can be determined with deadly accuracy. Halstead's convictions arise from his discovery of a new form of *doubt*, such that he can contend that "there is not any serious doubts about *Homo erectus* being directly ancestral to *Homo sapiens*". This is certainly the news Biology has waited for, the moment when the Truth can at last be known so that all this difficult and extremely tiresome theory can be dispensed with. Until now my colleagues and I had always imagined that to doubt something was "to be uncertain as to a truth or fact" and the notion that distinguishes science from, say, politics is that in science uncertainty about the truth must remain or progress ends. Halstead's discovery can only mean that he has in hand a new form of truth — a kind of truth that can be known. Many of us puzzled over what kind it might be until it finally dawned on us that it emerges from false doubt and, to honour its discoverer, I call it *Halsteadian Truth*. If ever again we are troubled by some theoretical matter we need only consult the oracle in Reading. Now that Halsteadian Truth permits us to know at last the way in which evolution proceeds, what extinct gene pools were like, and exactly who was whose ancestor, I confess, with sadness for years misspent, that cladistics is indeed a waste of time.

This news about the emptiness of cladistics will have a profound effect on systematics, for

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From the Mammal Society:

In the letter from the Mammal Society on badgers and TB published on 11 December, the statements appear to be attributed to Stephen Harris. I wish to make it clear that the letter is a Mammal Society view and that Stephen Harris was only personally involved in the communication of the letter to the Editor.

— J. R. Flowerdew, Hon. Secretary.

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most of the students and younger zoological systematists (and now even some botanists) have adopted this strategy of dealing with the empirical world in the belief that they were coming to understand something new about the hierarchical structure of nature. Of course, we should have known all along that something was amiss since most of our older colleagues from an earlier and wiser generation had told us that the spread of cladistics will have an extremely deleterious effect on Mayrian (or Simpsonian, or Darwinian) taxonomy. Well, the damage is done: their taxonomy will never be the same again — wiped out, as it were, by an epidemic of cladistics brought on by thoughtless, unhygienic and scientifically irresponsible cladists.

Some of my colleagues doubtless (in the old sense) will consider it premature for me to write such congratulatory lines about Dr Halstead on the grounds that he knows almost nothing, and appears to be unfamiliar with the literature, of cladistics. But I remind them that with Halsteadian Truth, knowledge of cladistics is irrelevant for cladistics is a theoretical matter. So I say to Halstead that there can be no serious doubt (in the new sense, for I actually seem to be getting the hang of it!) about the importance of his discovery of false doubt to systematics and evolutionary biology. Application of Halsteadian Truth suddenly reveals why all those serious doubters have questioned the usefulness of gradualism as a doctrine. This is because, poor souls, they imagined that (1) the modern doctrine of gradualism as derived from population genetics has no known empirical relationship to the hierarchy of organisms, (2) that population genetics theory was designed to rescue Darwin's theory of natural selection when it faltered because of its untestability, and (3) that the applicability of gradualism as an explanation of taxic diversity is achieved by a simple extrapolation from artificial selection experiments and the changes in gene frequencies observed in nature. I am forced also to include among the serious doubters a variety of embryologists and developmental geneticists who, before the coming of Halsteadian Truth, were thought to be rather distinguished.

The innate wisdom Halstead shows about cladistics and evolutionary theory extends to his philosophy of science and politics. On the thinnest of pretexts he tries to convince us that his certain knowledge that evolution is gradual constitutes a perfect, and necessary, refutation of Marxist doctrine. Would Halstead, I wonder, imagine that the massive extinctions that have taken place in his gradualistic world also carry sinister implications for us? The class struggle? Or did Darwin only predict the end of his own theory? Halstead, this great seeker of truth, must really be rebuked for deliberately confounding science and politics and for attributing the troublesome notions of Social Darwinism to the entirely ethical and clear-thinking scientists and educators of the British Museum.

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SIR — Beverly Halstead has drawn attention¹ to the ideological basis of the present exhibition policy at the Natural History Museum, and its implications. Other aspects of present policy are scarcely less disturbing. They relate to the display of the museum's collections and to the use of the building in which they are housed.

The museum's original exhibits were systematically arranged and a great number of genera and species were shown. The building in which they were housed was designed by Alfred Waterhouse (1830–1905), one of our greatest Victorian architects. It is a superb building and is listed as a Grade I Historic Building by the Department of the Environment. Waterhouse himself took great pains over the exterior and interior detailing and the whole building has an impressive unity.

The original exhibition galleries, whose plan and detailing reflected the order and hierarchy of nature, as then perceived, remained in use until the outbreak of war in 1939. The war led to closure of some galleries, and after the war nothing much happened for a long time. Museums generally were reacting against the old cases and galleries stuffed with objects, and the authorities of the British Museum (Natural History) did not seem to know what to do about the old systematic displays.

Eventually a policy seemed to emerge, exemplified by the new Fossil Mammal exhibit opened in 1972 in what was the old Fossil Mammal Gallery. In place of the dramatic long vista past mounted skeletons of fossil elephants, the continuity of the gallery was broken up and its original architectural character subdued, though some of the detail is still visible. Scientifically the new exhibit is much better than the old one, with a great deal of information on the animals and on the rocks from which they came, attractively displayed. As Halstead has pointed out², everyone, from a specialist to a child, can get something out of it.

So far, the museum had pursued the policy of showing a representative series from the collections which are its *raison d'être*. In this connection it is interesting that the British Museum Act 1963 makes no reference to the public exhibition of the collections. The explanations must be that in the eighteenth century, when the original acts were framed which the 1963 Act replaces, it was self-evident that a museum existed for this purpose.

Those who hoped that Fossil Mammals heralded the modernization of other galleries were soon to be disappointed. There was an abrupt change of policy. The museum's report³ for 1972–74 said that "Unhappily, the existing public exhibition does not match the museum's purpose . . . it has lagged far behind this century's revolution in the natural sciences. . . it fails to give a clear picture of natural history as the study of the world in which we live with principles which lie at the groundwork of our technologies, and which are the first principles of rational living". The failure seems disputable—the original exhibits largely gave such a picture, and the rest of the sentence is gibberish. (What are the first principles of rational living?)

The report outlined a new exhibition "much larger than the old" designed to "reflect all aspects of modern biology". It

gave four themes which were to be the basis for the new exhibition: Ecology; Life processes and behaviour; Evolution and diversity; and Man. The Hall of Human Biology (opened in 1977; actually a warren rather than a hall), Ecology (1978) and the new exhibits on Man and Dinosaurs criticized by Halstead, are the first fruits of the stated policy.

It is debatable to what extent the museum should set itself up to be the fountainhead of the fashionable parts of modern biological education. In the case of the new Dinosaur exhibit, the sequence of arguments presented in the exhibit is repeated almost word for word in the accompanying booklet. This exemplifies the pointlessness of the new exhibition policy, which employs a great deal of expensive equipment to put over concepts which are better expounded on paper, and which are but distantly related to the collections and function of the museum. There is no coherent relationship between the new exhibits, the collections and the building. The recent exhibits make minimal use of actual specimens (none at all in Human Biology) and are unsuited to the exhibition space provided by Waterhouse's building. Human Biology was built inside former galleries, which it conceals completely, and Ecology was set up in the former gallery of living mammals. (It has recently been moved to the Fossil Reptile Gallery, soon to be demolished, where it looks even more out of place.)

Yet for all these unpromising omens, the museum apparently does intend to show its collections. The "Evolution and diversity" exhibits will, it is stated, "contain most of the material now on show in the Museum", exemplifying 5,000 to 10,000 species. They "will continue to provide . . . the most entertaining aspect of the Museum, halls of monsters and a stuffed zoo." Now this is exactly what the Waterhouse galleries were designed to do. It seems the height of perversity not to use them for the purpose. On the contrary, it is proposed to dismantle the 1972 Fossil Mammals exhibit (an admirable exposition of evolution and diversity) and to demolish a large range of the original galleries. Surely it would be better to do the obvious thing, use the original galleries for displaying the collections, and put exhibits such as Human Biology in a communicating building outside the original structure.

It is clear that the authorities of the museum are, at best, embarrassed by the splendid interiors they have inherited. They do not know what to do with them, or how to make use of them constructively. At worst they appear to regard them merely as an impediment to be concealed and, if possible, removed.

Worse than this, there appears never to have been any coherent plan for the development of the buildings and the site as a whole. Both the original building, admirably suited to its original purpose but quite inadequate for research and storage, and the site are large, but not inexhaustible. For example, the new Palaeontology building (opened 1977) is splendid and fairly well suited for its purpose of research and storage, but it has used up the last available building plot next to the main building. This piecemeal approach exactly parallels the present attitude to the use of the interiors — exhibits and other facilities being