CORRESPONDENCE

Wrong rodents

SIR — The discovery that several cell lines claimed to be derived from patients with Hodgkin's disease in fact come from an owl monkey [1] emphasizes the vital importance of quality control in biological research.

Several cases of genetic contamination of inbred strains of laboratory mice and rats have come to light recently, and although these are not associated with any known scientific fraud, they have led in some cases to a considerable disruption of research projects. In the United States, for example, one large commercial breeder appears to have been selling "inbred" Lewis (LEW) rats which are not histocompatible, leading to a flood of complaints from research workers and inevitably casting doubt on the validity of some published work. In Japan, a survey of over 100 colonies of inbred mice [2] has shown that about 10 per cent had been genetically contaminated either recently (with continued genetic segregation at some loci) or in the more distant past. Indeed many of the major sublines of common inbred strains such as C3H and C57BL have arisen as a result of genetic contamination in the past [3].

In the United Kingdom a voluntary genetic monitoring scheme for commercial breeders was started about five years ago by the Medical Research Council Laboratory Animals Centre and several cases of genetic contamination have been discovered since that time [4]. Both of the main UK suppliers of inbred mice and rats took these results so seriously that they have established their own in-house genetic monitoring programmes run under the direction of consultant geneticists. The International Committee on Laboratory Animal Science (ICLAS) is preparing a handbook on methods of genetic monitoring, and is considering the establishment of some international reference centres for genetic monitoring. In the meantime research workers are strongly urged not to take the authenticity of the strains that they use entirely on trust. If they purchase animals from commercial breeders they should demand to know what steps the breeder is taking to monitor the stock. Those who breed their own strains should remember that accidental contamination can occur at any time, and they should check their stock using skin grafting or some other suitable method (such as the study of biochemical [5] or immunological markers [6] or morphological features such as mandible shape [7]) as a matter of routine.

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French museums

SIR — Seen from this side of the Channel, Halstead's criticisms of the new exhibition policy of the British Museum (Natural History) appear extremely unjust (*Nature* **288**, 208; 1980), especially towards the staff who contributed to such clear and remarkable presentation. In France, there is no discussion of that sort, because most of our natural history museums simply have no exhibition policy at all, and are nothing but museums of museums!

Last November, when Halstead was writing his venomous letter, I visited the two new exhibits (on dinosaurs and on the evolution of man) at the British Museum, and was amazed to see how much the public liked them, in particular the children, who seemed very receptive to the logic of cladistic analysis. Halstead's long attack against that method of tracing the interrelationships of living beings deserves more comment. He claims that cladistic analysis, which is the basis of the British Museum exhibits, does not take into consideration the concept of gradualism, according to which evolution can be presented as uninterrupted series of species, older ones being ancestral to younger ones. Hence his accusation against cladism, which is, according to him, trying to make evolution fit Marxist views on the history of societies! Halstead presents gradualism as evidence, and cladism as a crime against evidence. But where is the evidence of gradualism? There is almost none, or rather it is everywhere one wants to see it.

Cladists do not deny that a species can give birth to daughter species, but they claim that in practice, it is impossible to determine the fossil ancestor of a species, since the ancestor is devoid of the derived characteristics of the daughter-species in question. Ancestor–descendant relationship is not, for cladists, a necessary statement, and the history of a group is better expressed by sister-group relationships, often illustrated by a cladogram. Many biostratigraphers are now convinced that successions of species based on stratigraphic distribution are nothing but illusions and that, in reality, each older species is extinguished by the younger one, whose centre of origin may be far from the locality studied. So, the subversive palaeontologists who are supposed to pervade the British Museum, knife between teeth, are simply those who consider that gradualism gives an illusory precision, which is unnecessary for tracing the course of evolution of a group. If these new views are congruent with vague ideological inferences on human societies, so what?. . . After all, the history of human societies often shows a succession of leaps, be they revolutions, wars, epidemics, or changes in climate. But major changes in human societies are seldom decided by the societies themselves. The societies and their chiefs want gradual change (compare the terms "changes in continuity" or "permanent revolution" used by right-wing and left-wing politicians respectively), but in the event, circumstances provoke sudden changes.

In France, some authors are now attacking Darwin (see P. P. Grassé, *L'homme en Accusation,* Albin Michel, Paris) because he is supposed to have inspired Hitler and still inspires the deeply anti-Marxist sociobiologists. Since cladism is also directly descended from Darwin's ideas on phylogeny and systematics, it can just as well be accused of supporting extreme right-wing ideologies.

In sum, even if some cladists claim that this method of analysis is more consistent with their political convictions, it is simply ridiculous to condemn it on the basis of such spurious arguments, or because Halstead's political opinions are different. The British educational system can be proud of the British Museum's exhibitions which will certainly teach future generations of biologists and palaeontologists the principles of phylogenetic reasoning. Perhaps this is just what Halstead is afraid of?

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More museums

SIR — Halstead's well publicized reactions[1–6] to the exhibition policies of the British Museum (Natural History), to non-gradualistic hypotheses about the evolutionary process, and to his own mistaken perceptions of cladistic analysis are unfortunate[7–9]. His notion that these views somehow abet non-scientific creationist metaphysics or logically support Marxist dialectics is preposterous. Whether Popper[10], Patterson[11], or Miles (cited by Halstead in ref.4) have said that the evolutionary concept is also metaphysically based and whether this is true are beside the point. Equating Marxist political theory either with cladistic analysis or with so-called macroevolutionary hypotheses is as wrong as social Darwinism was. It needs to be driven home, as Patterson has done[9], that cladistic analysis is about pattern, not about any particular hypothesized evolutionary process, although most phylogenetic systematists do hold that descent with modification is an economical explanation of the existence of biological pattern in general. These systematists also define "related" to mean "genealogically related." In all six publications Halstead confuses cladograms with evolutionary trees. For instance, a steady theme is, "there is no place for ancestral species in a cladogram"[2]. Again, "it is axiomatic, therefore, that no species in the fossil record can be ancestral to any other nor can one species evolve directly into another"[5]. Halstead has not understood that cladograms do not assume that species are ancestors, but neither do they deny possible ancestry when a taxon lacks known derived characters. Assumptions of ancestry are appropriate for trees but are not made in cladograms.

Cladistic analysis parsimoniously estimates relatedness and is therefore testable. The British Museum (Natural History) is to be congratulated for bringing epistemology into its exhibits and teaching visitors that science is a method, not a body of revealed knowledge. That the museum may also need to discuss various hypotheses of evolutionary process[12]

and to address other needs as well, such as the public's thirst for discussion of functional anatomy, ecology, behaviour, and so on[13], is also evident.

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SIR — I have read with diligence the continuing controversy sparked by Halstead. Thus far I have had difficulty achieving a precise understanding of it. Now, however, certain matters are clear (*Nature* 1/8 January, p.106.) To sum up: Halstead dislikes the new exhibits at the British Museum (Natural History), and he would convince other persons that they, too, should dislike the exhibits. The argument he gives, as far as my evaluation goes, passes not from the ridiculous to the sublime, but emanates entirely from the low end of that spectrum. Knowing Halstead to be usually of good cheer and judgement, I am led to suspect that not all is as it might seem — that the root of his dislike is not to be found brewing in a pot pourri of punctuated equilibria, Marxism, scholastic death, etc. Rather, his dislike may stem, as seems to me, from a sense of loss of "the fossil record" — the ultimate source of the truth of evolution as rendered by a professional class of fellow-specialists. To the dismay, sometimes acute, of the more clerically minded members of this profession, cladistics treats fossils in a secular fashion — not as revelation but as some among many other biological specimens subject to interpretation that is apt, indeed expected, to be diverse, especially with respect to details (for example, the true nature of the "Petralona skull"). As reasonable as this treatment might seem to the outsider, the emotional effect within such a palaeontologist involuntarily confronted with cladistics (as I have witnessed on more occasions than I care to remember) is not unlike that apt to be experienced by a fundamentalist minister who has forced upon him uninvited the notion that the Bible is just one book among many. Suffice it to say that more than one kind of church has been built upon rock.

So what now? Here in the States creationists dislike the museums' secular exhibits on evolution and the schools' secular treatment of that subject. In Britain a palaeontologist dislikes secular exhibits on the "fossil record". The forms are similar, but the substances at first glance seem utterly different. Palaeontology, after all, is nominally a science, and a rational mind can easily defend it as such. The problem I have with Halstead's defence, if I may term it that, is reconciling it with a standard of rationality.

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Genes and racism

SIR — Steven Rose notes in his recent letter (*Nature* 22 January, p.335) that a National Front journal *New Nation* has claimed to find support for racism in my writings on sociobiology, as well as in those of Dawkins and Maynard Smith. Rose calls on the latter two authors to dissociate themselves from such misuse, although curiously he does not extend the same invitation to me. To keep the record straight, I am happy to point out that no justification for racism is to be found in the truly scientific study of the biological basis of social behaviour. As I stated in *On Human Nature,* "I will go further and suggest that hope and pride and not despair are the ultimate legacy of genetic diversity, because we are a single species, not two or more, one great breeding system through which genes flow and mix in each generation. Because of that flux, mankind viewed over many generations shares a single human nature within which relatively minor hereditary influences recycle through ever changing patterns, between the sexes and across families and entire populations".

If there is a possible hereditary tendency to acquire xenophobia and nationalist feelings, it is a *non sequitur* to interpret such a hypothesis as an argument in favour of racist ideology. It is more reasonable to assume that a knowledge of such a hereditary basis can lead to the circumvention of destructive behaviour such as racism, just as a knowledge of the hereditary basis of haemoglobin chemistry and insulin production can lead to the amelioration of their pathological variants.

I now call on Professor Rose to consider these and similar arguments raised in my writings. It is my hope that he will not confine himself, as he has in the past, to arguments that link sociobiology to racism and thus to continue to abet the very misuse which he piously claims to deplore.

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Origin of cancer

SIR —John Cairns' article on "The origin of human cancer" (*Nature*, 29 January 1981, pages 353-357) dismisses the importance of chemical mutagens in human cancer aetiology on, we believe, very tenuous grounds. He argues that there must be a single underlying mechanism for tumour production and that this is not through point mutation. As your leading article (*Nature*, 5 February 1981) quite rightly points out, why should there be a single mechanism for cancer development? Indeed, can one say that cancer is even a single disease? Cairns' article appears to us to be a simplistic approach to a complex problem. We all would like to have the answer to how a cancer cell develops, but can one say that Cairns' article will lead us any nearer to the truth? Why should workers in the field of chemical carcinogenesis abandon a large body of work which may be getting somewhere near to establishing why populations differ in cancer incidence, and substitute a hypothesis of genetic transposition bearing in mind that, as Cairns writes, it is not yet clear whether transposition is important in vertebrate development. Should we abandon the somatic mutation theory of cancer for a hypothesis with no experimental evidence? Furthermore, why should mutagens/carcinogens only act through point mutations? It is well recognized, as Cairns states, that agents reacting with mammalian cell DNA can cause gross effects such as chromosome aberrations, sister chromatid exchanges, deletions, and so on. Just because one measures DNA reaction in bacteria with a point mutation system does not mean *a priori* that this is the mechanism of action of these chemicals in mammalian cells. After all, bacteria don't even get cancer. Bacteria are merely used for carcinogen screening because they are cheap and mutations are easy to score.

Where Cairns' article is mischievous is in suggesting that people are wasting their time looking for carcinogens in the environment and then, having identified them, seeking to reduce exposure. The fact is that if people would stop exposing themselves and their immediate families to mutagens/carcinogens in the form of cigarette smoke, a large proportion of cancers would be prevented (besides a large proportion of coronary heart disease, lung disorders, etc.). If Cairns accepts that smoking is bad for one, why should he not accept that other environmental insults might also be carcinogenic? To say that animal carcinogens induce mainly liver cancer, that humans don't normally get cancer of this organ, and therefore animal liver carcinogens have nothing to do with human cancer shows a complete unawareness of chemical carcinogenesis. Most chemicals which have been identified as human carcinogens do *not* give the same spectrum of tumours in animals as in humans, for example benzidine causes bladder cancer in man but liver cancer in animals.

Cairns' main argument for DNA reaction being unimportant in carcinogenesis is the finding that in xeroderma pigmentosum patients, few if any internal cancers have been seen. Why should one expect an increased incidence of lung cancer, for example, in these patients? Is the skin the same as the lung in its function, biochemistry, enzyme profile, etc.? Why should we expect the mechanism of skin cancer to be the same as for internal organs? Do xeroderma pigmentosum patients live in the same environment as the normal population? I don't think one has to indulge in "special pleading" to support the case of somatic mutation as being important in cancer production. The majority of facts available tend to support the somatic mutation hypothesis. When there are sufficient data available to overthrow this hypothesis, then is the time for it to be abandoned. In the meantime, those of us working in chemical carcinogenesis will carry on identifying carcinogens in the environment and recommending that exposure be reduced. Whether society (or Cairns) listens to us is entirely up to them.

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