

and phrenology make the grade as science — false science but nevertheless science.

Numerous other contributors to the philosophy of science such as Nagel, Hempel, Hanson, Toulmin, Kuhn, Shapere, Lakatos, McMullin and many others have shown serious conceptual flaws in Popper's defence of the falsifiability criterion for demarcating science from non-science⁵.

Since the issue is so badly mauled in the *Nature* editorial let us take up the matter of the falsifiability of Darwinism since it seems of such great concern to cladists, creationists, and correspondents to this journal. The issue is stated as the question, "Is Darwinism falsifiable?" admits of a simple answer — yes.

If we are talking about the version of evolutionary theory propounded by Darwin in the *Origin*, then that theory has been shown false many times over. It claimed that all organic variation could be accounted for by natural selection and a tendency to inherit among all creatures. This is false. Mutation and recombination at the level of genes refute the adequacy of Darwin's posited mechanisms. Darwin claimed that the bulk of speciation occurred through anagenesis — evolution within a lineage. This is false. Cladogenesis or the splitting of an interbreeding population into two groups via natural barriers or other means accounts for the bulk of speciation. Darwin claimed that social behaviour exists in insects as a consequence of group benefit. This is false. Social behaviour exists among many insects as a consequence of kin selection, reciprocal altruism, parental manipulation or some combination of these mechanisms. This list could go on. The point is that evolutionary theory, like all theories in science is constantly tested, refined, modified, adapted and re-written. The issue confronting defenders of the validity and utility of evolutionary theory in its early or modern forms is not whether it is science or not — Darwin himself certainly settled that matter with admirable skill in the empirical and analogical evidence he brought forward in his own writings. Rather the question is how does one go about invalidating scientific theories that constantly change and evolve — an issue that ought to attract the attention of the cladists if not the creationists.

How true is the theory of evolution? It is as true a theory as there is in science. Which is to say it depends on who is asking and when. Of course the theory of evolution is not a fact. Evolution is a fact and the theory of evolution tries to explain this fact. But the theory of evolution surely passes muster as science, even on the out-dated grounds cited by Professor Popper in his early work. (Even he no longer believes in the adequacy of his early analyses of evolutionary theory⁷.) The real question that should concern scientists is whether they know enough about current thinking in the history and philosophy of science to know a sound theory when it stares them in the face — a circumstance unlikely to occur at the British Museum unless this ignorance is alleviated.

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1. *Nature* 290, 75-76 (1981)
2. Popper, K. R. *The Poverty of Historicism* (Routledge & Kegan Paul, London, 1957).
3. David Hull, *The Philosophy of Biological Science*, (Prentice Hall, Englewood Cliffs, 1974).
4. Ruse, M. *Philosophy of Biology* (Hutchinson, London, 1973).
5. Suppe F. (ed.) *The Structure of Scientific Theories* 2nd edn (University of Illinois Press, Urbana, 1977).
6. Caplan, A. L. *Erkenntnis* 13, 261-278 (1978).
7. Ruse, M. *Philosophy Sci.* 44, 638-661 (1977).

Academia stagnant?

SIR — I see no reason for your concern (*Nature* 19 March, p.175) about the ability of British universities to recruit young scientists to academic posts. The present policy of recruiting young persons in their mid-twenties and sometimes without postdoctoral or international experience has so little merit that nothing would be lost if this recruitment of lecturers was totally ended. The best of our young scientists will wish to stay longer in full-time research and will continue to be available to the universities for years to come.

The rapid recruitment of academic staff in the 1960s expansion phase is unlikely to be the cause of stagnation in science departments. Where stagnation exists it is fairly certain that the department has defective leadership which might very well result from easy promotions gained in the years of expansion. Universities should, especially when resources are tight, recruit scientists into permanent teaching posts later in their careers than has been customary in Britain. A changed policy could provide continuing opportunities for the present generation of young scientists. Retirements and early retirements will improve some departments and at the same time allow new entrants or well justified internal promotions which ought to maintain the intellectual vitality of any department worth keeping.

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Freedom from NATO

SIR — With the mounting concern in your columns on measures which scientists should take to ensure academic freedom for colleagues deprived of their rights, it is surprising that you publicize the NATO Advanced Study Institutes, that scientists are happy to perform on the stage which NATO sets, and that no mention of this paradox has been made in your correspondence.

No doubt these conferences fulfil a useful scientific function in promoting exchange of information in very congenial surroundings. Under normal circumstances, the argument that funds (from a rather dubious source) were being channelled to a good cause would have been sufficient to rationalize the situation. In some cases there is also the hope that the opinions of the scientific community will filter back along this channel, and that contact with sensitive institutions will lead to a more extensive dialogue and to a greater opportunity for diplomatic manoeuvres. Often, such occasions bring us into contact with scientists who have difficulty in meeting foreign workers because of governmental restrictions.

These conditions do not apply to attendance at NATO-supported conferences. There is little evidence that the meetings provide a forum for discussion with NATO officials in any way which would allow scientists to exercise their social responsibility. Nor could it be claimed that the activities of the conferences could not easily take place under other auspices. But most seriously, at a time when nuclear installations in Europe, products of scientific ingenuity, threaten the peace of our own and other countries, this close relationship between scientists and NATO can only be seen from outside as complete approval for (if not connivance with) the increasingly military orientation of our society.

This association with NATO must also affect the scientists themselves. After participation in any symposium it is difficult to leave without feeling some debt of gratitude to the organizers who lavish money and attention on their charges. Qualms about the ethics of the institution running it are henceforth stifled. Thus the NATO scientific conference is a highly effective public relations exercise both for the participants, and those who look to scientists for a lead in problems created by technology. How can Soviet colleagues be expected to take our views seriously when they see the strength of our opinions so cunningly controlled by political agencies? If our protests are to carry conviction in any field, we should dissociate ourselves from the influence of organizations like NATO, and take every opportunity to make this known publicly. ROGER R.C. NEW
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Conservation sites

SIR — Many criticisms can be levelled at the system of Sites of Special Scientific Interest (SSSIs) but Muir's arguments (*Nature* 12 March p.82) are untenable.

Extinction may be "normal" but that is irrelevant to "value". To analogize, we will all die, but do not usually accept this as an excuse for murder. We are replacing complex ecosystems with much simpler landscapes, allowing for fewer species to exist or appear.

In a limited sense, rarity may support a value judgement of "biological deficiency", and the mere presence of a rarity may be poor grounds for conservation; but rare species can be good indicators of environmental quality when considered along with other evidence.

SSSI status may result in a "loss of freedom" of the owner. Losing such sites is a far greater "loss of freedom": it destroys our options and those of future generations, for we cannot replace them.

It is also misleading to allege just that protection of SSSIs is a cost to their owners: it prevents owners increasing their income by reclaiming the site, but that increase would itself be a cost to the taxpayer in terms of grants for reclamation, and later subsidies.

It is astonishing that someone from a university zoology department could contend that "Once . . . information is recorded and published . . . nothing new will be learned by preservation". No complete record of any site or habitat exists, and the same material can generate an infinite variety of "descriptions". Frequently we cannot turn to the literature for basic data to test new theories — we have to test them through fresh fieldwork designed specifically for that purpose. If we lose the field sites, ecology becomes metaphysics and not science, and to rely just on the environments we are creating today leaves us a severely restricted data base.

This necessity has fortunately been recognized in other fields. Once, excavators of bone caves destroyed vast storehouses of information by removing every vestige of material but failing to distinguish stratified deposits. Later workers recognized strata but again lost vital information by failing to extract pollen, rodent bones, and other small items. Now sections of deposit are left undisturbed for the future application of new ideas and techniques. Ecologists need similar opportunities. KEVIN A. ROBERTS
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