CORRESPONDENCE

Ethics for animals

SIR — As noted in your editorial (*Nature* 17 September, p.173), human research is already controlled by ethics committees, and so we have some experience that can predict the effects of introducing their non-human counterparts.

An ethics committee that takes itself seriously is a major obstacle to a research worker. By the nature of things, its members are senior, having their own productive research days well behind them. The supplicant is called upon to justify his proposition in advance of the evidence, so the more original "look-see" type of work is discouraged. In arguing a nice point of logic, the committee is both cross-examining counsel and judge. Research workers have many possible deadlines to meet, and delay imposed by waiting for one or maybe more meetings may give the lead to other workers or cause an intending financial sponsor to take his grant elsewhere.

But, having said all that, the ethics committee is a necessary evil, and the adversarial system sharpens the scientist's mind wonderfully. Since non-human animals cannot be consulted or give consent, their "rights" must be safeguarded even more carefully than those of humans. I have never met a vivisectionist who approached his work thoughtlessly, yet standards of what is acceptable vary enormously, and if nothing else we should each be exposed to other people's opinions when planning animal experiments.

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Survivalist hopes

SIR — Barrie Pearson (*Nature* 3 September, p.6) objects to Antony Flew's (*Nature* 16 July, p.192) "formulation of the principle of natural selection as non-random survival" on the grounds that it is an "empirically empty concept if the range of possibilities the non-random survivors were chosen from is unknown". Thus, he would have us believe that without such absolute knowledge biologists, as "creatures of reason", must accept the possibility of random survival. This argument, however, is simply another one of those "philosophical muddles" to which Dr Flew referred that continue to obscure the creationist debate.

The "range of possibilities" on which natural selection acts is ultimately dependent upon randomly occurring mutations. Many of these are deleterious, often with fatal consequences. It is not necessary to identify every possible mutation, its effects and its frequency of occurrence, therefore, in order to conclude that some allelic variants are much less likely to persist than others.

Of course, Darwin's theory of natural selection explains not only the elimination of deleterious mutations, but also the spread of favourable ones. The generality of this theory lies in the generality of certain necessary conditions. (1) Many more individuals are produced than can possibly survive; (2) among these individuals there is heritable variation in characteristics affecting reproductive success. If Dr Pearson wishes to dispute the general applicability of natural selection he should probably begin by attacking the ubiquity of these conditions in nature. I think that it is safe to say, however, that he will not meet with much success.

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Gold solution

SIR — Your editorial on returning to the gold standard (*Nature* 24 September, p.246) showed an amazing degree of understanding of the topic. Gold has been a monetary standard primarily because almost all that has been mined in the past several thousand years is still around in a readily available form. Mining at full tilt can scarcely make a ripple in this huge, widely distributed bulk and the major commercial uses of gold do not destroy its availability, so that the supply is essentially independent of human agency. Your suggestion of the establishment of a short-lived isotope as an alternative monetary standard would never have occurred to me.

Your observation that gold is unnecessary is correct. All that is needed is pure reason and self-restraint and the world economy will run quite smoothly. Better yet, there will be no wars.

It is, however, not strictly correct that the gold standard would force a country to balance its yearly budget. Borrowing of funds is still permitted under a gold standard. The difference is that such debts would eventually have to be repaid rather than being inflated out of existence.

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A good read

SIR — That science is more complex that it was fifty years ago is reflected in the fragmentation of subjects into more and more specialities, each requiring at least one journal of its own to lend an air of respectability. The hard-pressed scientist wishing to keep a tenuous grasp on the broad advance of science turns, as he always has done, to *Nature*, *Science* and the like. But does the writing in these non-specialist journals now also mirror the growing complexity of science? We have applied an objective method of testing readability to letters published in *Nature* over the past fifty years and we find a highly significant decrease between 1940 and 1950. The readability after this date appears to be stable.

The test, developed by Flesch[1], uses the number of syllables, words and sentences in a chosen passage to calculate the average word length (wl) and sentence length (sl). These are then used to compute the reading ease of the passage (RE) from the equation:

$$RE = 206.835 - 0.846wl - 1.015sl$$

Flesch produced a classification of RE which is summarized in Table 1. We have chosen every twentieth letter published in *Nature* from 1930, at ten-year intervals, to 1980 and calculated RE for the first hundred words in each. The first hundred were chosen in the belief that here the authors should be writing most clearly, attempting to explain the purpose of their work and its significance to the general reader. Choosing the beginning rather than any other part also reduced the chance of encountering complex formulae and equations which do not readily lend themselves to RE calculations. Our results are shown in Table 2.

It can be seen that there is a sharp drop in RE when 1950 is compared with 1940. The Student *t*-test shows this to be highly significant ($P<0.01$). The RE changed little over the period 1950–80. Thus, our subjective impression that *Nature* was much easier to understand during the 1960s than today does not appear to be true, at least as measured by reading ease. The significant drop in RE between 1940 and 1950 was accompanied by an increase in the number of letters published in *Nature* but, interestingly, no similar reduction took place in 1960 when the number of letters published was greater than ever before.

Table 1 Flesch classification of reading ease (RE)

RE	*Style*	*Typical example*
0–30	Very difficult	Scientific
30–50	Difficult	Academic
50–60	Fairly difficult	Quality
60–70	Standard	Digest
70–80	Fairly easy	Slick fiction
80–90	Easy	Pulp fiction
90–100	Very easy	Comics

Table 2 Reading ease (RE) of letters published in *Nature* over half a century

Year	*No. of letters*	*RE (s.e.)*
1930	8	32.1 (6.3)
1940	10	29.1 (2.6)
1950	29	16.5 (2.4)
1960	76	18.0 (1.5)
1970	59	15.0 (1.6)
1980	41	12.8 (2.1)

It has been suggested[2,3] that, for a paper to be impressive, it must be almost or, even better, completely unintelligible. For example it has been demonstrated[4] that, in the field of management studies, a journal's prestige increased as its readability decreased. Although we are at present unable to test this idea rigorously for science journals we have used *Current Contents* to identify a dozen letters published in *Nature* since 1950 which have been cited more than one hundred times. The mean RE of these letters was 25.0 with a standard error of 4.2 and perhaps the most celebrated of these, on the helical structure of DNA by Watson and Crick[5], had an RE of 51.9. These findings suggest at least that letters appearing in *Nature* need not necessarily be cloaked in obscure and difficult language to be accepted for publication or to make a significant impact on the scientific world.

●The RE of this letter is 40.0.

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