

## CORRESPONDENCE

## Stanford's patent

SIR — Nowhere in your discussion of the United States patent to Cohen and Boyer (*Nature* 13 August, p.571–574) do you give its number, which is 4,237,224.

In what you call the antecedents of the Cohen–Boyer patent the patentees included the work of Hershfield *et al.* at that time in the press but later in prosecution cited by the Examiner from *Proc. natn. Acad. Sci. U.S.A.* 71, 3455 (1974). The US Examiner also cited the earlier work of Chakrabarty to be found in his important US patent assigned to General Electric Company, No. 3,813,316.

It is essential in any discussion of possible infringement of 4,237,224 to look at the precise language of the 14 claims *per se* and not to rely on loose remarks on what commentators think they may or may not protect.

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## Unnecessary schism

SIR — I think that the Devil tries his best to drive the Church and scientists apart by splitting those who see truth in Darwin's theory of "Survival of the fittest" and the creation of the Universe by God. I believe the two go well together. If we think of "fitness" as being pleasing to God, who told the animals to "go forth and multiply", then this ties in well with "for the Lord watches over the way of the righteous, and the way of the wicked will perish" — Psalm Iv.6.

I believe that God is at work just as much in the Miller experiment as in the day of the primordial soup.

When the bible says that the Lord created the world in six days, we need not take this to mean 6 × 24 hours. In English we often use the word "day" to mean an indefinite period.

When Fox (*Nature* 6 August, p.490) suggests a theory of how nucleic acids are formed this reinforces my faith in God. Christians do not need to hide in the unexplained to see God at work but rather see him as all the more glorious when we see how he does it.

The gaps of knowledge suggest to me that the Creator is cleverer than all the world's scientists and all the world's computers put together.

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## Discovery unmasked

SIR — Examination with modern microscopes of van Leeuwenhoek's original late-seventeenth century specimens and sections preserved amongst the manuscripts at the Royal Society is a most exciting project. However, Brian Ford (*Nature* 30 July, p.407) is mistaken in claiming that they have only just been discovered by himself. Clifford Dobell (1886–1949), who carried out his pioneering study of the Leeuwenhoek collection at the Royal Society throughout the 1920s, clearly states in his scholarly and indispensable *Antony van Leeuwenhoek and his "Little Animals"* (p.333) published in 1932 (and reprinted in paperback by Dover Books in

1960) that "Leeuwenhoek was one of the first — if not the very first — to study the structure of solid opaque bodies by means of sections. Some which he cut with his own hand by means of a sharp shaving razor are still in existence. They were enclosed in a little packet affixed to an early letter (Letter 4, 1 June 1674. To Oldenburg. MS. Roy. Soc. . .), and have remained intact to the present day".

Professor F.J. Cole, who justly stated "No student of Leeuwenhoek can fail to be deeply impressed by Dobell's classic monograph", also refers briefly to the existence of the specimens at the Royal Society in a publication in 1937 on "Leeuwenhoek's Zoological Researches". No doubt because it was published after Dobell's book, Cole's study, which appeared in two parts (*Ann. Sci.* 2, 1–46, 185–235; 1937), seems to have been generally neglected. It can be recommended as a most useful guide to Leeuwenhoek's letters, as a thorough study of Leeuwenhoek's histology, and particularly valuable because Cole has compiled a 37-page analytical index of tissues and specimens studied by Leeuwenhoek. The way in which Leeuwenhoek prepared his razor is described by him in two letters of September and November 1709 (*Phil. Trans.* 26, 493–502; 1709). Cole sums up Leeuwenhoek's histology thus: "He studied rough hand sections . . . never evolved a technique which would have enabled him to prepare sections of soft tissues without previously drying the material. Only near the end, in 1714 does he mention any method of staining . . . He first mentions section technique in 1674 and some of the sections he then cut with a 'sharp shaving razor' still survive attached to Letter 4 [1 June 1674]".

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## but . . .

SIR — My contribution to your columns did not make the claim to which R.D. Wood objects. It actually began with the words: "The original specimens sent by the 'father of microscopy', Antony van Leeuwenhoek, are still in existence". The words "After not being seen since 1674 . . ." were added by *Nature* staff in processing the article.

Even so, this exciting discovery of Leeuwenhoek's fine specimens in pristine condition after three centuries or more is not dignified by citing occasional individuals half a century ago who noted the existence of the packets, but did not investigate what they contained. Contrary to what your correspondent implies, for the majority of the specimens there are no known historical records since Leeuwenhoek's time.

The value of these specimens as a source of information on Leeuwenhoek and his work lies in the remarkable fact that they have remained apparently undisturbed until I discovered them earlier this year<sup>1</sup>. The historical record substantiates my belief: in particular, Dobell's remarks about the packet of 1 June 1674 are a helpful corroboration that the specimens remained (in his words) "intact to the present day".

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1. Ford, B.J. *Not. Rec. R. Soc.* 36 (1), 37–59 (1981).

## Matter of principle

SIR — Anthony Flew in his formulation of the principle of natural selection as non-random survival (*Nature* 16 July, p.192) claims to have denied this principle the tautological status some have accorded it. It does not seem, however, that this formulation circumvents the main problem — namely lack of empirical content of the principle. Non-random survival is an empirically empty concept if the range of possibilities the non-random survivors were supposedly selected from is unknown. The range of possibilities is known in a few instances only, where polymorphisms determined by multiple alleles correlate with environmental variables, for example, as in the industrial mechanism of the moth *Biston betularia*.

As a consequence natural selection can never have a broadness of empirical base commensurate with its claims for generality — a point which ought not to be overlooked if biologists wish to lay claim to being creatures of reason rather than faith and thus maintain the distinction in modes of thought between the biologist and the creationist.

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## Thoughts on Popper

SIR — Halstead<sup>1</sup> has mentioned Popper's recantation<sup>2</sup> of some of his mistaken ideas on evolution. The unmasking of some of Popper's apparent errors occurred as follows. In May 1977 my paper<sup>3</sup> on "The testability of the role of natural selection within theories of population genetics and evolution" was widely circulated and received for publication. In that paper I explained at length why Popper's ideas on the testability of natural selection seem totally wrong. Popper's own recantation appeared first somewhat later in his Darwin lecture (Darwin College, Cambridge, November 1977). His brief arguments seemed to be as wrong as ever and were based on isolated instances of natural selection, instead of referring to some population-genetic theories that involve natural selection. The Darwin lecture also contained an apparent flaw as regards Popper's views on epiphenomenalism<sup>4</sup> (these views were based on evolutionary considerations). Ruse had earlier criticized Popper's apparent misconceptions on evolution<sup>5</sup>, but further scrutiny was called for. Hence, in a recent paper<sup>6</sup>, following the earlier critiques by Lewontin and Ruse of Popper's views on evolution, I analysed at great length the likely nature of the "theory of evolution". In that paper I noted that "it would be wrong to think that Popper has correctly assessed what matters in evolution", and I tried to exhibit in great detail the apparent fundamental errors of many of Popper's ideas on evolution, although my paper is mainly constructive.

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1. Halstead, I.B. *Nature* 292, 403–404 (1981).
2. Popper, K. *Dialectica* 32, 344 (1978).
3. Wassermann, G.D. *Br. J. Phil. Sci.* 29, 223–242 (1978).
4. Wassermann, G.D. *Mind* 88, 572–575 (1979).
5. Ruse, M. *Phil. Sci.* 44, 638–661 (1977).
6. Wassermann, G.D. *Phil. Sci.* (in the press).