Serious flaws in UK funding system

Sir — The article by Wennerås and Wold (*Nature* **387,** 341–343; 1997), containing clear proof of nepotism in a funding body, is fascinating.

I have had many discussions with contemporaries and colleagues about the way in which decisions are made by the major UK funding bodies, and there is general agreement that the system is seriously flawed.

Everyone has stories about autocratic committee chairmen with whom no one dares to disagree, referees with a study competitive to that of the applicant, committee members who take up ideas for their own groups a year or two after the originator's application was turned down, and committees where more than half the grants under discussion were awarded to members of the committee.

More recently, in Europe, I heard of two European Commission contracts awarded to a group that had been removed from its own national register because of poor performance and no publications. Of course, no one has had, until recently (see D. F. Horrobin, *The Lancet* **348,** 1293–1295; 1996), any good idea how the system might be changed to avoid lack of objectivity, bias and the unfair apportioning of huge sums of money, often repeatedly.

I have been involved in making grant applications to the major funding bodies since the mid-1960s. In all that time, it was until recently never necessary to object to fair or even relatively unfair criticism. However, I wrote last year to one of the major funding bodies about the careless way in which our application was both refereed and subsequently dealt with by the committee. It was not prompted by sour grapes but by the indisputably incorrect statements made in their letter of rejection.

My letter was designed, first, to explain how much more helpful it would be to those of us 'on the other side' to receive feedback before the committee meeting that decides our scientific futures. Grant applications are dealt with in this way by at least one major UK funding body. Secondly, I wished to point out the extent to which the outcome can be substantially affected by luck and by objectivity (or lack of it) from both referees and committee members.

The unsatisfactory reply that I received contained the comment: "You are free, of course, to submit a fresh application if that is substantially different from this one." Given that the reasons for turning down the application could not be discussed, are we to play some sort of guessing game as to how our "important study" (the committee's words) should be redesigned? If I am to go by the referees' reports, then very little needs to be altered. Am I therefore to take a stab at suggesting something completely different which is trendy and of academic interest but may not improve the welfare of patients to the extent that I believe our present protocol may do?

This is a difficult and dispiriting situation, which erodes research time, thinking time and confidence. It is not surprising that nothing will induce many graduate students (male or female) to take up scientific research as a career these days.

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Sir — The research that Wennerås and Wold have been able to do and therefore the issues they raise are crucially dependent on the Swedish commitment to freedom of information. The findings, however, are of relevance to the global research community.

In the British case, our pathological love of secrecy means that it is simply impossible to monitor equity of treatment, either in the procedures for the allocation of grants or in the recognition of scientific excellence. Bodies from the research councils to the Royal Society should find this study food for thought, as they, like the judiciary, are institutions where pale males have been for too long chosen to overrepresent themselves for the health of either science or society.

For that matter, *Nature*'s innovatory footnote drawing attention to the gender of the three referees was itself both smug and sexist. A more worthy reaction would have been for *Nature* to concentrate first on the possibility of a beam in its own eye. In the same issue, your account of the "Science Wars" (*Nature* **387**, 331–335) manages to erase the fact that Gross and Levitt had attacked, most vitriolically and extensively, the feminist critics of science. Indeed, as I recall their book, the single most attacked person was the distinguished US philosopher of science Sandra Harding. By focusing exclusively on the fights between the boys, *Nature* managed to make even the "Science Wars" another pale male story.

Is it any wonder that some of us think that the tide is rather unlikely to turn?

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Cannibalism and kuru

Sir — In his review of *Deadly Feasts: Tracking the Secrets of a Terrifying New Plague* (*Nature* **386,** 565; 1997), Robert Desowitz repeats a fundamental error with regard to the aetiology of kuru. He writes: "Gajdusek and his Australian colleagues, Michael Alpers and Vincent Zigas, showed that cannibalism was the cause of kuru. The Fore women and children, but not the men, ate — totally consumed — their dead relatives."

That cannibalism might be the route to kuru transmission was discovered by two anthropologists, Shirley Lindenbaum and Robert Glasse, five years after Gajdusek first arrived in the Eastern Highlands of Papua New Guinea, years in which every attempt to unravel the cause of kuru had failed. In 1961, they spent nine months with the Fore searching for a possible genetic cause for kuru. None existed.

Their second visit proved more fruitful. A New Zealand neurologist and epidemiologist, Dr R.W. Hornbrook, suggested that they try to answer the question "what is it that the adult women and the children of both sexes in the Fore tribe are doing that the adult men are not?".

They found that the women, who prepared the bodies for burial, occasionally ate part of the flesh and of the steamed brain tissue. But far from their "totally consuming their dead relatives", or cannibalism being a ritual in the Fore tribe, it was a casual practice that had infiltrated from tribes in the south of the region within living memory. The Fore themselves said that "it was after the first aeroplane flew over that we tried cannibalism for the first time".

Back at the US National Institutes of Health, Gajdusek and Joe Gibbs now tried to induce kuru in nonhuman primates by feeding them with kuru-infected human brain. They failed to induce the disease, even when they inserted the infected tissue directly into the stomach through a gastric tube. So they believe that direct inoculation into the bloodstream or through the mucous membrane was the route of infection. Women and children with cuts and sores on their hands would frequently rub their eyes and noses when handling dead bodies, so any infectious particles could enter the body quite easily.

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