

French blood contamination

SIR — I should like to respond to your leading article and to the News story by Declan Butler (*Nature* 359, 759 & 764; 1992) about human immunodeficiency virus (HIV) contamination of French haemophiliacs by blood products.

I should first like to clarify my responsibilities at the CNTS (the national blood transfusion centre): I was not a “government official”, I was, as a medical scientist, the head of research and development for purification of plasma proteins, not “head of research”. This position placed me in the third tier of the organization. Being in research and development I had no part in collection, manufacturing or distribution of CNTS products. I cannot understand how it can be claimed that I “sold or allowed to be sold” or “supplied” any product.

In December 1984, two critical pieces of information became known to the 29 members of the AIDS-Haemophilia French study group that I coordinated and these facts were subsequently communicated (January 1985) to my senior colleagues and heads of the French Haemophilia Association. These were:

(1) French Factor VIII and Factor IX concentrates were potentially contaminated by HIV as 35–40 per cent of haemophiliacs exclusively treated with these products had antibody to LAV as it was then termed¹. Results were obtained with a research screening assay that had not at that stage been subject to rigorous analysis.

(2) In a group of 18 patients receiving the Travenol Factor VIII preparation — which was heat treated — seroconversion was not observed².

The conclusion drawn from these two observations was that the still uninfected haemophiliacs were likely to be protected from HIV infection if treated with heat-inactivated concentrates. In early 1985 there were no French heated products and only imported concentrates were available. As part of my letter of 16 January 1985 I therefore emphasized that the CNTS had responsibility in the prevention of this fatal illness (AIDS) *vis-à-vis* haemophiliacs, their doctors and the Ministry of Health. I also drew attention to the probable consequences of not signing the contract for heat-treatment technology transfer including the need for huge imports of foreign heat-treated products.

The demand for heat-treated products was also expressed by me and six other haemophilia experts in February 1985 and again by me in April following the first AIDS conference in Atlanta. These occasions are clearly documented and additional verbal but undocumented attempts on my part were also ignored.

An important difficulty in implementing preventive measures was that, until March 1985, HIV antibody assays were still under development and, until July 1985, the Abbott test was not licensed in France.

I as much as anybody at that time was deeply concerned with the risks the haemophilia population were potentially incurring. However, it is important to recognize that by the earliest date at which heat-treated products would have been introduced, that is, if the advice of myself and others had been implemented forthwith, more than 90 per cent of those who were later established to be infected had in fact already seroconverted — that is, had already been infected.

In brief, the difficulty I face today is to get people to understand that my conclusions in early 1985 were not perceived then as self-evident facts beyond scientific criticism — hence the difficulty in getting my advice implemented. Uncertainties existed at that time that have now been resolved — uncertainties about the accuracy of serological tests, about their clinical significance, about the risk of seropositive patients developing AIDS and the efficacy of various heat-treatment processes. The information available at the time was shared by a large number of scientists, medical specialists and officials at the French Haemophilia Association through their medical advisers. The problem everywhere, not just in France, was to convert information into prompt action in organizations that were traditionally bureaucratic.

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1. AIDS-Hemophilia French Study Group. *Blood* 66, 891–901 (1985).
2. Rouzioux, C. et al. *Lancet* I, 271 (1985).

Cave of the ostrich

SIR — Irrespective of whether George William Stow produced a spurious Bushman painting of a man disguised as an ostrich approaching a flock of real ostriches, there is no doubt that he visited a cave which he designated ‘the cave of the ostrich’. He listed it in his list of important caves of the Bushmen¹. He also mentioned that it was near Oliphant’s Been.

Oliphant’s Been is, in fact, a pillar of rock standing some 15 metres high with weathered rocks and rockface around it, close to the border of Lesotho in the

southern Orange Free State. I visited the site in December 1982 and found the cave within about 10 metres of the rock pillar. I doubt whether many people had been there during the 100-year interval between Stow’s visit and mine. Apart from mentioning the site in his list of notable caves, Stow also mentioned it briefly as being near an ‘excellent ochreous’ earth supply². As it was an extraordinarily hot day, I did not look for the ochre deposit after photographing the paintings but regard this mention as definite evidence that Stow visited the site.

As far as I am aware, these paintings have never been reproduced and, in fact, Stow’s title should be in the plural as there are actually three ostriches in the cave and not much else. Two of the ostriches are painted together and the other, with an extremely attenuated neck, is separate. There are one or two rather crude red paintings, one of which might also represent an ostrich, and a very faded human figure, but the two ostriches in black with touches of white on the wings, obviously males, are the main attraction of the site.

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1. Stow, G. W. *Native Races of South Africa* (Struik, Cape Town, 1905, reprinted 1964).
2. Stow, G. W. *Report of the Geological Surveyor* (OFS Newspaper Company, Bloemfontein, 1878).

Misquoted

SIR — Sir Hermann Bondi (*Nature* 358, 363; 1992) noted that “Thus science is not something strange and odd but the most human of pursuits. Hoyle, in his novel *The Black Cloud* puts this so well when a nonscientist says of scientists: ‘I cannot understand what makes them tick. They are always wrong and they always go on.’ This very popperian sentiment inspires us all through our trials and tribulations. Popper has made it clear that we should be proud so to be described.”

Remarkably, there is no such statement in *The Black Cloud*. The nearest thing to it in the edition published in 1957 (Harper and Row, New York) is on page 166, where Francis Parkinson, the private secretary to the Prime Minister, said “‘It isn’t so much the volume of talk that surprises me’ he laughed. ‘We get plenty of that in politics. It’s the number of mistakes they’ve made, how often things have turned out differently to what they’ve expected.’”

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