clinically less appropriate than the addition of folate supplements the apportioning of blame for folate deficiency to the sulphasalazine treatment must remain tentative.

STEPHEN P KANE
M A BOOTS

Departments of Gastroenterology
and Haematology,
Charing Cross Hospital,
London W6

Anxieties and fears about plutonium

SIR,—Dr R H Mole's article (17 September, p 743) is an understandable reaction to criticism. Yet if the criticisms of official studies on plutonium toxicity are to be met we need to feel that they are being dealt with impartially and competently.

Two points in Dr Mole's article are disquieting. One is his comment on the interaction between smoking and uranium mining, where he quotes one paper¹ that suggests no interaction but not another paper² that suggests that there is one. Experts in a field are not usually so selective.

The second point is the quotation from Ide *et al*³ where Dr Mole harshly suggests that Gofman⁴ misrepresented the paper in saying that "Ide and co-workers showed 30% of ciliated cells to be denuded of their cilia among smokers." Dr Mole goes on, "If the reported evidence has to be misrepresented in this way to make a case, then the case is likely to be worthless." It so happens that at the Windscale Inquiry Dr Dolphin, an assistant director of the National Radiological Protection Board, was cross-examined on this point. In response to the question of what proportion of cilia are damaged by smoking (day 50, p 20, letter G of the official transcript) Dr Dolphin replied, "I have no first-hand knowledge of this obviously, but reports which have been quoted in the literature, for instance by Ide, indicate in his particular observations that about 30% of the cells in the bronchial epithelium had lost their cilia compared with non-smokers. That was a paper by Ide. It was published in 1959." Now I cannot imagine Dr Mole accusing Dr Dolphin of misrepresentation; presumably Gofman was thought unworthy of the normal courtesies of scientific debate because he has opposed the nuclear power industry in the past. In fact I have checked the paper by Ide *et al* and consider both Gofman and Dr Dolphin justified in their interpretations, though the paper is perhaps not very clearly written.

After three months at the Windscale Inquiry I came away more worried than when I went, because the monitoring authorities do not seem to be doing an adequate job on the Windscale effluents. The letter by Dr Mole, written apparently under the aegis of the MRC Radiobiology Unit, increases my concern still further.

ROBERT BLACKITH

Zoology Department,
Trinity College,
Dublin

¹ Sevc, J, Kunz, E, and Plaček, V, *Health Physics*, 1976, **30**, 433.

² Archer, V E, Gillam, J D, and Wagoner, J K, *Annals of the New York Academy of Sciences*, 1976, **271**, 280.

³ Ide, G, Sultzzeff, V, and Cowdrey, E V, *Cancer*, 1959, **12**, 473.

⁴ Crofman, J W, *Journal of the American Medical Association*, 1976, **236**, 284.

* * * We sent a copy of this letter to Dr Mole,

whose reply is printed below, together with a comment on a previous letter from Dr Jacqueline Orman (8 October, p 959).—Ed, BMJ.

SIR,—Dr Blackith quite wrongly assumes that my article was a reaction to criticism. Actually it was an attempt to explain some of the background to the Windscale Inquiry which might interest busy doctors. I haven't been near the inquiry or read any of the transcripts of evidence. In my article I quoted Ide *et al*'s own words and what Gofman made of them ("... the bronchi will have sizeable regions free of cilia") and must leave Dr Blackith with his own personal assessment. Has he read Gofman's articles? In these smokers are said to be 125 times more sensitive than non-smokers to lung cancer induction by inhaled radioactive particles because the virtual absence of cilia from (parts of) the respiratory epithelium in smokers prevents the normally rapid removal of particles from the airways. Neither Ide *et al* nor Auerbach *et al*¹ said they had observed sizeable regions free of cilia in smokers but not non-smokers. I would not regard it as misrepresentation for Dr Dolphin to describe what Ide *et al* or Gofman wrote as "in the literature."

Clearly my article was not, and could not have been, a substantial review of any scientific evidence. The USA monograph on lung cancer in uranium miners² showed that there was an association between cigarette smoking by the miners and an enhanced lung cancer frequency. Possible explanations were considered—for instance, that cigarette smoking potentiated the effect of uranium mining (against which serious arguments were adduced) or that it made mining-induced lung cancer appear earlier than otherwise. Within a few years the number of lung cancers trebled in Indian uranium miners³ (who rarely smoke⁴) and the overall frequency for a 14-year follow-up ($11/780 = 1.4\%$) was as high as one-third of that for White miners (most of whom smoked) after a 24-year follow-up ($144/3366 = 4.3\%$).⁴ What need disentangling are the differential effects of two age-related phenomena, the rapidly increasing age-specific lung cancer incidence in smokers, and the much less rapidly increasing radon exposure of the lungs with increasing duration of occupation as a miner. The Czech authors (Sevc *et al*) whose words I cited surveyed more miners, found a higher risk per rad than in the US, and concluded that their dosimetric errors were lower. Was it so wrong of me to limit myself to the one reference (which cited the US work) and its conclusion that smoking as a major causal agent could be excluded?

Much of Dr Orman's letter repeated what I wrote as if I had said the opposite. However, her emphasis on hereditary damage in relation to plutonium exposure is misplaced, as I had already explained.

R H MOLE

MRC Radiobiology Unit,
Harwell, Oxon

¹ Auerbach, D, *et al*, *New England Journal of Medicine*, 1961, **265**, 253.

² Lundin, F E, Wagoner, J K, and Archer, V E, *Radon Daughter Exposure and Respiratory Cancer: Quantitative and Temporal Aspects*. NIOSH and NIEHS Joint Monograph No 1. Washington, DC, US Department of Health, Education and Welfare, 1971.

³ Wagoner, J K, Archer, V E, and Gillam, J D, *Proceedings of the XIth International Cancer Congress*, 1975, **3**, 102.

⁴ Archer, V E, Gillam, J D, Wagoner, J K, *Annals of the New York Academy of Sciences*, 1976, **271**, 280.

Human tick infestation in Britain

SIR,—Dr J A Slater (29 October, p 1158) describes his experience of tick infestation while picnicking beside Coniston Water and would like to hear of other cases in Britain. Oddly enough, a friend recently described to me the removal of a tick from her scalp when, as a child, she lived on the shore of Coniston Water.

I have seen two cases of tick infestation in London caused by *Rhipicephalus sanguineus*, a parasite of dogs. One patient, a Scandinavian visitor, found the tick in her pubic hair. The other, an administrator at a London teaching hospital, had stayed at a cottage in Norfolk and his wife discovered the tick on his back when he was in the bath. In neither case did the tick draw attention to itself, nor was there any history of direct contact with dogs.

Ticks are common parasites of household pets, though human infestation in this country appears uncommon. The risk of transmission of serious microbial diseases, such as occurs in many other countries, must be remote. Some years ago you discussed in a leading article¹ the bites of parasites acquired from pets, but ticks were not mentioned; they deserve a place as an unusual cause of arthropod bites from this source.

M BARNHAM

Department of Bacteriology,
St Mary's Hospital (Harrow Road),
London W9

¹ *British Medical Journal*, 1971, **4**, 640.

SIR,—Dr J A Slater asks for information on human tick infestation in Britain (29 October, p 1158). Our experience in Shropshire suggests that this is uncommon but not rare.

We have seen two such incidents this year, the first for over six years. A 6-year-old boy who had been playing in the fields presented with a tick attached to the shaft of penis. An attempt to dislodge the tick by covering it with petroleum jelly to obstruct its airway was unsuccessful and traction together with the application of the hot end of a matchstick proved more satisfactory. The local lesion was then treated with magnesium sulphate dressings and a five-day course of oral penicillin was prescribed as a prophylactic measure. The second patient, seen by a colleague, was an adult with a tick on his arm; this was dislodged successfully by traction alone. Unfortunately the trauma of removal distorted the mouth parts of the parasites so that their detailed identification was impracticable but circumstantial evidence suggests that both were derived from sheep. During the last war one of us (GBR) had a tick attached to the chest wall; this was dislodged by applying a burning cigarette end to the back of the parasite.

C A MORRIS

Director,
Public Health Laboratory
G B ROSS
General practitioner

Shrewsbury, Salop

SIR,—In response to Dr J A Slater (29 October, p 1158), there are several hard ticks which may attach themselves to man in Britain. The most frequent is the sheep tick, *Ixodes ricinus*. In parts of Scotland bites by the sheep tick are accepted as a natural minor hazard of rural life and a doctor would not normally be consulted. After walking through bracken on the island of Mull I have picked, on one