It is understandable that the final paragraph of the paper by R Morgan and colleagues gave rise to concern.² One of the authors of the paper has confirmed that the medicolegal advice given in the case cited was given specifically in the context of facts about a particular patient with metastatic bronchial carcinoma who had expressed a particular wish about cardiopulmonary resuscitation. The society's and the union's advice in that particular case was then generalised and reported as a personal communication in the paper by Morgan and colleagues.

We agree with Dudley that it is not correct generally to assert that "legally if a patient requests cardiopulmonary resuscitation it should be provided." This is not the appropriate place to attempt an exposition of general guidance on the subject.

We question whether a major international journal should publish what is attributed as a personal communication without some attempt at verification. We commend for debate the proposition that personal communications should be substantiated by the authors when draft papers are submitted to a journal for publication. Additionally, there may be circumstances in which the journal would then seek to verify such personal communications before publication

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****Books and teaching on writing scientific papers** consistently tell authors that they should seek permission from the source before citing a personal communication, and we will alter our guidance to authors to make this clear. As with all statements made in papers and letters, the responsibility for accuracy must rest primarily with the author. It is not possible for us to check everything.—EDITOR

Tests on volunteers at Porton Down

EDITOR,—In his review of the *Network First* programme "The Secrets of Porton Down" Sean Spence rightly draws attention to the inadequacies of assessment of research programmes carried out at what is now the Chemical and Biological Defence Establishment, Porton Down, in which volunteers participated.¹ Perfect hindsight can also, however, be applied to many research protocols in civilian life during the first three decades after the second world war. The Ministry of Defence is not alone in having carried out work that now would seem unethical. I agree that, in retrospect, long term follow up studies might have been instigated, but should we blame our scientific predecessors for failing to recognise this need?

Ever since an independent ethics committee was established at Porton Down in 1988 all experiments of whatever nature involving volunteers done at the establishment have been subject to the most careful analysis for their hazards and their relevance to defence needs. The details of our approach have been described elsewhere,² and I challenge anyone to match the quality of evaluation that we have used. I doubt—though I do not have totally objective evidence—if most ethics committees in the NHS could meet out standards. We have a records system that allows us to trace every subject for follow up. In this regard we are conscious of the fact that the long term effects of

systemic exposure to nerve agents is not known but is, as far as is possible through retrospective surveys, being addressed; thus we do not sanction such exposure. We have no a priori scientific evidence that exposure to the levels that might be used in experiments would endanger volunteers in either the short or the long term, but we are aware of the need to be cautious.

Finally, I should emphasise that in the world beyond Britain's shores there is a chemical and biological warfare capability that requires us to continue research into methods of protecting our service personnel—and maybe our civilian population—against the hazards that could confront us. We should support an approach that tackles the problems but, at the same time, does so within the guidelines of the ethical attitudes to which we all subscribe.

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Reye's syndrome

EDITOR,—Susan M Hall has commented¹ on our statement that Reye's syndrome should be reassessed.² As we originally emphasised, the fact that Reye's syndrome is a heterogeneous condition implies that the North American and British epidemiological studies were done on a heterogeneous group of children with quite different—viral, metabolic, or toxic—diseases. This compromises the data suggesting a link between Reye's syndrome and aspirin, as the definition of a "case subject" must be specific for the case-control study to be valid and reliable.³

With regard to the aetiology of Reye's syndrome, it is surprising that Hall denies any evidence for an aetiological role for antiemetics despite accumulating evidence. In 1976 the Food and Drug Administration's warning concerning Reye's syndrome and antiemetics was based on convincing data (table). Moreover, why deny the results of the Ohio study part I and even the complete results of the British risk factor study itself?⁴ If Hall is convinced that vomiting is the start of Reye's syndrome, why did the British study record all drug treatment given to the patients before admission?⁵ Mowat's important commentary to this study stated: "Please note that 40% of the cases in the study were not thought to have taken aspirin before or during their illness."⁶

*Proportions of patients with Reye's syndrome who had taken phenothiazines or aspirin**

Study	No of cases	No for which drug history available	% Who had taken phenothiazines	% Who had taken aspirin
Goldfield	38	38	68	34
Corey	379	164	51	78
Partin	78	45	71	?

*Data quoted by Food and Drug Administration's panel on Reye's syndrome, 1976.

We do not think that our statement that the North American case-control studies were biased because they recorded only the drugs given before the onset of vomiting is weakened by the findings of Lichtenstein *et al*.⁷ rather it is strengthened. Lichtenstein *et al*'s patients had vomited for 1-2 days (range 12 to 66 hours) before being admitted to hospital, and all patients had taken drugs (average 2.5 drugs per patient) before being seen in the emergency room. Of the 19 patients in the study group, 15 had taken salicylates but seven

had taken decongestants, six antiemetics, and three diphenhydramine; all these drugs induce central effects, which may easily be confused with the central nervous system involvement in Reye's syndrome, as emphasised previously.

In conclusion, we hope that Reye's syndrome will be reassessed. Perhaps the best way would be for a group of experts—including clinicians—to reanalyse all available data on the syndrome.

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Treatment of doctors' wives

EDITOR,—The harrowing experiences of the anonymous author of a recent personal view are probably not exceptional.¹ Many years ago, when I was interested in how doctors treat each other, I looked at the obstetric experiences of doctors' wives.² It was clear that the essential ingredient to getting poor care was to be a doctor's wife. How these women's treatments became standard was relatively straightforward. The doctors marginalised their colleagues' wives by making them "special" patients. But what had to happen before a disaster occurred was for all the attending medical staff to ignore clinical clues that things were going wrong. This became clinically important when the wives' emotional care was being dealt with.

As the author of the personal view says in her last paragraph, it is important to treat everyone similarly. But it is also important for doctors to recognise the dangers of making some patients more special than others.

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Charging doctors for medical services

EDITOR,—Anthony Kenney's editorial seems to suggest that doctors are not or cannot or should not be treated as NHS patients but are or must or should always be treated as private patients. Is that true?

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