babies should thereafter be given either 25 µg daily or 1 mg weekly orally from 1 week until 3 months of age. All suspected cases of haemorrhagic disease of the newborn were collected by a surveillance system similar to that in Germany and Britain and based on active monthly reporting by all paediatric departments. From 1 October 1992 until 31 August 1994 no cases of serious bleeding or intracranial haemorrhage were reported. Only one case could be validated as idiopathic late haemorrhagic disease of the newborn.³ This was in a healthy 8 week old Turkish boy who was admitted with persistent nasal bleeding. He had been exclusively breast fed and had never received extra vitamin K.

Thus over 23 months only one case of idiopathic late haemorrhagic disease of the newborn was reported, in a boy who had not received prophylaxis. In the Netherlands an incidence of 7/100 000 was found before prophylaxis was introduced⁴; this is comparable to the incidence in Britain⁵ and Germany.⁶ As about 200 000 babies are born in the Netherlands each year and about 70% are breast fed it is striking that we did not encounter a single case in which prophylaxis failed; the surveillance system obtained a mean response rate of 90% (range 87-93%). In contrast, many cases of failure of oral prophylaxis were found in Germany, where a response rate of 77-88% was achieved.² We assume that this is because only two or three doses are given in Germany. In accordance with our data, the British Paediatric Surveillance Unit stated in 1993 that a considerable proportion of babies in Britain probably received repeated oral doses and that it had not been notified of any failure (unpublished report).

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- 1 Newburn M, Dodds R. Vitamin K for neonates. *BMJ* 1994;309:668-9. (10 September.)
- 2 Von Kries R, Göbel U. Oral vitamin K prophylaxis and late haemorrhagic disease of the newborn. *Lancet* 1994;343:352.
- 3 Von Kries R, Hanawa Y. Neonatal vitamin K prophylaxis. Report of the scientific and standardization subcommittee on perinatal haemostasis. *Thromb Haemost* 1993;69:293-5.
- 4 Widdershoven JAM. Vitamin K deficiency in infancy. Nijmegen: University of Nijmegen, 1991. (Medical thesis.)
- 5 McNinch A, Tripp JH. Haemorrhagic disease of the newborn in the British Isles: a two year prospective study. *BMJ* 1991;303:1105-9.
- 6 Von Kries R, Göbel U. Vitamin K prophylaxis and vitamin K deficiency bleeding (VKDB) in early infancy. *Acta Paediatr Scand* 1992;81:655-7.

Should obesity be treated?

Patients with sleep apnoea should be treated

EDITOR,—We believe that there are two further aspects to the problem of whether obesity should be treated.¹ Firstly, conditions in which obesity is life threatening and not just a marginal statistical risk must be considered. We frequently see patients with the obesity hypoventilation syndrome and respiratory failure. In these dangerously obese patients weight loss is critical if life expectancy is to be greater than just a few months. We take an aggressive approach to dieting in this condition and generally achieve a good response.

Secondly, obese patients who are a potential hazard to others should be treated. An appreciable number have the sleep apnoea syndrome, in which an excess of road traffic accidents occur because

patients fall asleep at the wheel. The driving authorities in Britain ban people with this condition from driving public service or heavy goods vehicles until they have received 12 months of effective treatment. We have seen more than 20 professional drivers (of buses, taxis, and heavy goods vehicles) who are unwilling to accept investigation or effective treatment for sleep apnoea because of the potential effect on their ability to drive and hence their standard of living. We have a major moral dilemma here. Should we inform the driving authorities about these patients or should patient confidentiality come first? It is difficult to resist the temptation to blackmail patients into losing weight with threats of driving bans.

We are faced daily with the problems of managing obesity in patients with disordered breathing during sleep. We feel duty bound to take an active approach to weight loss in view of the adverse effects of obesity on the patients and the community. We strongly support J S Garrow's proposal for community based slimming clinics, although, while women find these useful, our predominantly male patients with sleep apnoea decline to attend them.

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- 1 Garrow JS, Wooley SC, Garner DM. Should obesity be treated? *BMJ* 1994;309:654-6. (10 September.)

Amniotic band sequence in child of thalidomide victim

EDITOR,—Andrew P Read¹ and Richard W Smithells² dismiss W G McBride's hypothesis that thalidomide might be a mutagen.³ After this correspondence was published we saw a child with limb reduction defects born to a thalidomide victim.

The baby had the following isolated anomalies of the left hand: almost complete absence of the third and fourth fingers, with necrotic tissue attached to the tip, while the second finger had a deep constriction ring at the proximal phalanx with a hypertrophic distal segment and hypoplastic nail. Annular constriction was also present between the first and second phalanges of the thumb and was associated with a fibrous remnant; the fifth finger was normal (figure).

Pregnancy had been uneventful; ultrasound examinations did not show any limb defect, but a probable amniotic band was noted at the 22nd week of gestation. The mother, who was 34, had first been seen in 1986 when she received genetic counselling; her history showed maternal intake of thalidomide during the first trimester of pregnancy in 1959. Physical examination showed defects typical of thalidomide victims: bilateral symmetrical reduction defects of the arms (complete absence of the left forearm and hand with severe hypoplasia of the distal segment of the left

Limb reduction defect in baby born to thalidomide victim

humerus; absence of the right forearm with fusion of the hypoplastic humerus with a bony stump, possibly the radius, and the presence of only one finger) and shortening of the left femur, the rest of the legs being normal.

The birth prevalence of limb reduction defects in our region (north east Italy malformation registry, a EUROCAT local registry—EUROCAT is a European network for epidemiological surveillance of congenital anomalies) was 5.3/10 000 newborn babies during 1981-93, when 570 872 live births and stillbirths occurred. Three of the 304 cases were familial: two babies had a split hand or split foot malformation and one had a defect not due to thalidomide. In the same period only 10 cases due to amniotic bands, all sporadic, were registered. These data are not significantly different from those reported by other registries.^{4,5}

This is the third report of limb reduction defects in children of thalidomide victims; to our knowledge no anomalies other than limb reduction defects have been noted. These three reports raise the possibility that thalidomide could be involved in the cases. Read pointed out that thalidomide is unlikely to be a mutagen because a mutagen is not specific and thus malformations other than limb reduction defects should also be expected. Limb reduction defects could, however, be more easily reported than other defects because they are evident to the doctor, especially when a parent has a limb reduction defect. Furthermore, the hypothesised split hand or split foot deformity and Holt-Oram syndrome in the two cases reported by McBride could be the result of a new mutation.

Finally, although these three cases might have arisen by chance, a causal relation between limb reduction defects in the offspring of thalidomide victims and parental exposure to thalidomide cannot be excluded because the three cases are statistically unexpected on the basis of available epidemiological data and because the offspring of thalidomide victims are few (owing to the limited number of the victims and their presumed reduced fitness).

Thus retrospective and prospective studies should be carried out, especially because thalidomide is still used to treat leprosy.

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- 1 Read AP. Thalidomide may be a mutagen. *BMJ* 1994;308:1636. (18 June.)
- 2 Smithells RW. Thalidomide may be a mutagen. *BMJ* 1994;309:477. (13 August.)
- 3 McBride WG. Thalidomide may be a mutagen. *BMJ* 1994;308:1635-6. (18 June.)
- 4 EUROCAT Working Group. *Surveillance of congenital anomalies 1980-90*. Brussels: EUROCAT Central Registry, 1993. (EUROCAT report No 5.)
- 5 Froster UG, Baird PA. Amniotic band sequence and limb defects: data from a population-based study. *Am J Med Genet* 1993;46:497-500.

Resuscitation and patients' views

EDITOR,—N J Dudley suggests that the Medical Defence Union and the Medical Protection Society should re-evaluate their advice on cardiopulmonary resuscitation and expresses the view that our advice is "both ill considered and . . . incorrect."¹ If our advice had been quoted correctly we would accept his objection, but unfortunately it was not.

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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- 1 Dudley NJ. Resuscitation and patients' views. *BMJ* 1994;309:409. (6 August.)
- 2 Morgan R, King D, Prajapati C, Rowe J. Views of elderly people and their relatives on cardiopulmonary resuscitation. *BMJ* 1994;308:1677-8. (25 June.)

****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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- 1 Spence S. Listening to the silence. *BMJ* 1994;309:1024. (15 October.)
- 2 Dudley HAF. Research ethics committees and military defence. *J R Coll Physicians London* 1994;28:237-9.

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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- 1 Hall SM. Reye's syndrome. *BMJ* 1994;309:411. (6 August.)
- 2 Casteels-Van Daele M, Eggermont E. Reye's syndrome. *BMJ* 1994;308:919-20. (2 April.)
- 3 Moyer VA. Confusing conclusions and the clinician: an approach to evaluating case-control studies. *J Pediatr* 1994;124:671-4.
- 4 Hall SM, Plaster PA, Glasgow JFT, Hancock P. Preadmission antipyretics in Reye's syndrome. *Arch Dis Child* 1988;63:857-66.
- 5 Lichtenstein PK, Heubi JE, Daugherty CC, Farell MK, Sokol RJ, Rothbaum RJ, *et al*. Grade I Reye's syndrome: a frequent cause of vomiting and liver dysfunction after varicella and upper respiratory tract infection. *N Engl J Med* 1983;309:133-9.

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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- 1 A catalogue of disasters. *BMJ* 1994;309:814. (24 September.)
- 2 Dearlove J, Newman P, Bate T, Dearlove B. Ignoring the obvious: doctors' wives as patients. *BMJ* 1982;285:187-9.

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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- 1 Kenney A. Should doctors charge doctors for medical services? *BMJ* 1994;309:1318. (19 November.)