disease. Whatever the extent of the risk (and it is not clear what the committee considers to be a "low" risk), in my view there is a very strong case for compensating the parents of those children who are handicapped by such a reaction.

Although evidence has been presented on behalf of the Association of Parents of Vaccine Damaged Children to the Royal Commission on Civil Liability and Compensation for Personal Injury, it is unlikely that legislation will ensue for some time, and at best it is unlikely to be retroactive. The Joint Committee on Vaccination would do well to back its benign reassurance about pertussis vaccine with strong support for the speedy introduction of a State compensation scheme.

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1 Lambert, H J, *Public Health Reports*, 1965, **90**, 365.

Volunteers and the aftermath of stroke

SIR,—The setting up of volunteer schemes for home visitors for dysphasic patients, as described in your leading article (13 September, p 606) by Miss Valerie Eaton Griffith (p 633), is surely to be applauded in principle, but why do Miss Griffith and your correspondents (27 September, p 763) urge that such schemes should develop outside the existing speech therapy services? We see three dangers in setting up a separate autonomous system.

(1) Despite the present declarations from Miss Griffith and the Chest and Heart Association that volunteer schemes and professional speech therapy services can co-exist in parallel, economy-minded area health authorities may be tempted to jump to the conclusion that volunteer schemes can provide a cheap substitute for professional services for dysphasic patients. The glowing account that 21 out of 31 patients improved in speech after a few months of visiting by untrained and unpaid volunteers calls into question, if read uncritically, the value of the existing speech therapy services. It is true that, on several counts, the report does not stand up to critical examination—the patients were selected, the assessment procedures were not standardised, there was no untreated control group, some of the patients were also receiving professional speech therapy, and the evaluations of progress were largely made by people who had undertaken the therapy and people who were firmly committed to the scheme's success. But such reservations tend to get lost from view when schemes seem to offer solutions to difficult problems at little cost.

(2) Dysphasia is a complex subject about which continuing research is constantly providing new insights, particularly at the present time when a new discipline of "neurolinguistics" is developing; and surely it is not unreasonable to believe that the management of language therapy for dysphasic patients requires an awareness of this major area of study—requires, in fact, the professional training of a speech pathologist and therapist. Yet Miss Griffith suggests that the secret of the success of the volunteers is their very ignorance of this study and insight. One thing of which the professional speech therapist is aware is that the type of general stimulation therapy which volunteer schemes are offering may well be quite unsuitable for people with some kinds of dysphasic problems and could even be harmful. Speech therapists know all too well that they do not possess all the answers; in the present state of knowledge about dysphasia patients should be carefully assessed by modern techniques in order that therapeutic regimens can be individually designed for each patient, and reassessed, so that these regimens can be revised as therapy proceeds.

(3) There is a continuing need for the systematic collection and collation of data so that patterns of impairment can be compared with patterns of treatment and recovery. There is also an urgent practical need for scientifically based research into the effectiveness of all ways of coping with the rehabilitation of dysphasic patients. No one, surely, would advocate that this research should be undertaken by the untrained rather than by qualified professionals, but isn't this what has been happening?

These seem to us to be compelling reasons why volunteer schemes should be fostered within the aegis of the professional speech therapy service rather than outside. If all those good intentions and hours of devoted work from volunteers are to be well directed, and if such schemes are to realise their potential usefulness, doctors who wish to refer patients to voluntary organisations should do so through a speech therapist, who can decide on whether this approach is suitable for the individual patient, plan a therapeutic programme, and supervise its implementation. If there is any magic in being untrained and uninformed, as Miss Griffith suggests (and we suspect there isn't), it needs to be complemented by informed guidance.

These views are representative of those expressed by senior colleagues in speech therapy and provide some indication of the general concern which is felt in relation to the article.

MARGARET EDWARDS

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Sudden fetal deaths

SIR,—With the increasing use of continuous fetal heart monitoring there have been a number of anecdotal reports of sudden fetal death without warning. It is extremely important that the histories and fetal heart traces of these cases should be studied in detail if we are to make labour safe for the fetus.

I would be most grateful if any of your readers who have experience of such a case would write to me and, if possible, send a short clinical summary and the fetal heart trace. Hopefully, if sufficient cases are collected it will prove possible to make recommendations that will help to avoid these distressing unexpected intrapartum stillbirths.

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Epilepsy

SIR,—I read with interest Dr F B Gibberd's article (1 November, p 270) and would like to make the following comments.

(1) It is surprising in November 1975 to find an article on the treatment of epilepsy which does not mention sodium valproate. (2) In young children presenting with convulsions it is important to consider the possibility of non-accidental injury, especially when fundal haemorrhages are seen. (3) The minor motor epilepsies, dismissed in two lines, are more common than petit mal and their treatment is different; it may include clonazepam, nitrazepam, sodium valproate, steroids, and, in resistant and severe cases, a ketogenic diet. (4) With the availability of sodium valproate in addition to ethosuximide there must now be little place for troxidone in the treatment of petit mal. (5) The prophylaxis of febrile convulsions is an open subject; the evidence as regards regular anticonvulsant prophylaxis is contradictory and there is little evidence that intermittent phenobarbitone is useful.

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SIR,—Although not mentioned in Dr F B Gibberd's article on the treatment of epilepsy (1 November, p 270), a neonatal withdrawal syndrome has been associated with barbiturate use in pregnancy.[1-3] The prevention of this syndrome might be another valid reason for stopping treatment before pregnancy in attack-free patients.

Because the age of onset of symptoms ranges from 30 min to 14 days[1] this condition should be borne in mind both during hospital stay and after discharge in any infant of an epileptic mother.

I BLUMENTHAL

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1 Desmond, M M, *et al*, *Journal of Pediatrics*, 1972, **80**, 190.
2 Bleyer, W A, and Marshall, R E, *Journal of the American Medical Association*, 1972, **221**, 185.
3 Martinez, G, and Synder, R D, *Neurology*, 1973, **23**, 381.

Infective agent in infantile gastroenteritis

SIR,—Dr B Rowe and Mr R J Gross (18 October, p 162) are right to take up the cudgels in defence of some strains of *Escherichia coli* as causes of infantile enteritis and to remind us of the difficulties and limitations of routine bacteriological methods. Yet much of the evidence for the enteropathogenicity of *E coli* strains for man rests on similar observations of association with disease as have been reported for some of the viruses. A combined bacteriological and virological approach is required to define and distinguish the aetiological roles of these bacteria and viruses, together or separately, with due attention to the possibility of spurious associations. Difficulties of proof, particularly in neonates, may necessitate ultimately accepting such circumstantial evidence as the basis of guilt, however.

If alteration of the intestinal contents by viral diarrhoea favours selection of certain *E coli* types rather than others these types will show a secondary, not causal, association with the diarrhoea. If some of the viruses are coliphages they likewise may show spurious association with diarrhoea, unless by analogy with diphtheria the phages confer pathogenicity on the bacteria. If either bacterium or virus causes diarrhoea in a host carrying the other type of organism the diarrhoeal condition will favour simultaneous dissemination of both agents so that both may show association with diarrhoea in a particular outbreak. Similarly both agents, or whichever of them the investigator was equipped to detect, might show association with diarrhoea in an outbreak due to neither but to a third and undetected agent.

A lively dialectic between proponents of bacterial and viral aetiologies may generate useful hypotheses to be criticised and tested during the next exciting years of research into diarrhoeal diseases.

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Multicentre trial of prednisolone in the Guillain-Barré syndrome

SIR,—The Guillain-Barré syndrome, discussed in your leading article (26 July, p 190), has a misleading reputation as a benign condition. Severe weakness lasts for at least three months in most patients and respiratory failure necessitates artificial ventilation in about 20%. Despite intensive care mortality rates of 5 to 10% are still found in most modern series and recovery is incomplete in a further 5 to 10%.[1-3] Accordingly the potential benefits of an agent such as prednisolone are worth investigating.

Sadly, after quarter of a century of uncontrolled trials the role of corticosteroids in the treatment of Guillain-Barré syndrome remains controversial.[3-5] We are therefore engaged in a multicentre controlled trial in which the results of randomly allocated treatment with or without a course of prednisolone are being assessed by "blind" observers. The trial is now in its second year and 20 patients have entered. Preliminary statistical analysis of our results by Professor P Armitage does not reveal an advantage to either group. We estimate that 50 patients will be required to show a clinically worthwhile change due to treatment. Patients from any hospital in London or its immediate neighbourhood are eligible for entry to the trial, and further details can be obtained from us.

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1 Masucci, E F, and Kurtzke, J F, *Journal of Neurological Sciences*, 1971, **13**, 483.
2 Marshall, J, *Brain*, 1963, **86**, 55.
3 Goodall, J A D, Kosmidis, J C, and Geddes, A M, *Lancet*, 1974, **1**, 524.
4 Prineas, J, *Acta Neurologica Scandinavica*, 1970, **46**, Supplement 44, 1.
5 Bammer, H, and Schaltenbrand, G, *Münchener medizinische Wochenschrift*, 1965, **107**, 1629.

SI units

SIR,—Since my letter (19 July, p 159) on the subject of SI units, I noted that there was no further correspondence from any clinician who could point to any advantage resulting from their use.

At the insistence of my medical colleagues, I carried out a survey of all the medical staff in our health district, and the results indicate that, out of a possible 150 ballot papers, there were 90 signed objections to the introduction of SI. There was one dissenting colleague.

As I mentioned in my previous letter, I would be extremely foolish to force a system of clinical reporting on my colleagues who did not desire it, and I must say that the dilemma remains unresolved, especially in the light of Ministry "advice."

I find none of the arguments advanced in favour of SI as being convincing, and I know that medical staff and others make frequent use of literature derived from America and other foreign sources, which so far will continue to report in "proper metricated" units. It is easy to say that "everybody else is out of step but me," but in this instance everybody else—that is, the world—will be in step while the United Kingdom, by virtue of SI adoption, will be talking a curious scientific jargon almost singular in world clinical medicine.

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SIR,—The King's Lynn District Hospital Medical Staff Committee approved on 23 October the following motion: "That we do not intend to introduce SI units on 31 December 1975."

The staff suspected that support for the change is rather less strong than advocates of the new régime would have us believe.

MICHAEL J BRINDLE
Chairman,
King's Lynn District
Hospital Medical Staff Committee

King's Lynn, Norfolk

Influenza vaccination

SIR,—I read with great interest your leading article on influenza vaccination (18 October, p 125) and would like to make some comments supported by evidence which may not have been in your possession.

It is stated that field evidence of the protective effect of a live attenuated influenza vaccine given intranasally is still lacking. As this is a product very recently introduced into the UK this is so of this country, but a considerable amount of such work has been done on the Continent and in the USA.

To the best of our knowledge there is no field evidence of the protective effect of any updated inactivated vaccines against the A/Scotland strain. The killed vaccine has been shown to produce a better antibody response in general, but there is often a poor correlation between serum antibody response and the protection afforded an individual against influenza following vaccination. Protection of between 70 and 80% with reasonable certainty is claimed in your article for inactivated vaccine. However, a recent study has shown over 80% protection against natural challenge with A/Port Chalmers using a live vaccine.[1] Response to the A/Scotland virus is stated to be unlikely to be as good in the case of a live vaccine as that provoked by a killed vaccine, but again recent work has shown a serum conversion rate of 84% to this virus using a live vaccine.[2] Postvaccination titres were as high to A/Scotland as to A/Port Chalmers and the homologous strain.

A small point of correction is that the work by Lauteria *et al* referred to in your article involved an early strain of live attenuated virus "Ann," not the one currently available ("Alice").

The slight adverse effect on small-airway function observed in healthy volunteers[3] has not been confirmed in a further study in the USA in which changes in pulmonary function (using flow volume curves with air and helium mixture) have been used in both asthmatics and a control group. No changes in pulmonary function were demonstrated, no significant symptoms were reported, and a fourfold rise in antibody titre was found in [illegible]sons with low titres.[4]

In conclusion, it is submitted that live attenuated influenza vaccine has been well tolerated by over 10 000 people during its development, subsequently by well over a quarter of a million in clinical usage, and significantly by 381 patients who took part in clinical trials and were suffering from bronchopulmonary disease—that is, the high-risk groups.

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1 Douglas, G, as reported by A Prinzie at a symposium at the Royal Society of Medicine. London, April 1975.
2 Kuwert, E, in Symposium on Viral Diseases, Vienna, September 1975.
3 Rosenzweig, D Y, *et al*, *American Review of Respiratory Diseases*, 1975, **111**, 399.
4 Storms, W W, *et al*. Submitted for publication.

*** We are familiar with published, and much unpublished, work on live attenuated influenza vaccine from both the Continent and the USA, including that presented at the London symposium to which Dr Jackson-Moore refers. We reaffirm our view that although "there are expectations that live vaccines will stimulate a more solid immunity than killed," so far there have been no unequivocal reports that the live A/England vaccine protects against clinical infection with homologous or related viruses.

We agree that there is no field evidence so far available of the protective effect of up-to-date inactivated vaccine against the A/Scotland strain of influenza A virus. At the same time most workers in the field, including manufacturers, would expect that inactivated A/Scotland vaccine should be as protective against the homologous virus as previous inactivated vaccines against their homologous viruses. This expectation underlies the regular updating of inactivated vaccines, a policy that has not yet been doubted.

It is encouraging that "Alice" live A/England vaccine may give a serum conversion rate of 84% against the A/Scotland virus. Nevertheless, the serum conversion rate is only one criterion of the antibody response, and in the study to which Dr Jackson-Moore refers Professor Kuwert also reported that the serum HI antibody titres were lower after live virus vaccine than after inactivated vaccine. He further reported that the local HI antibody response was predominantly strain-specific.

That live vaccine may have an adverse effect on small-airway function is a possibility that it would be unwise entirely to disregard on the basis of the two negative reports. It is greatly hoped that live attenuated influenza vaccines will prove to be