Correspondence

Archives of Disease in Childhood, 1970, **45**, 731.

Treatment of the Poisoned Child

Sir,

There may well be a place for emetics in the treatment of children who have ingested poison, as advocated by David Reid (1970). However, on the evidence at present available, it is quite unjustifiable to conclude that emesis is superior to gastric lavage in terms of safety or effectiveness.

The risks of properly performed lavage are frequently exaggerated. Bleeding does occur in approximately half of those washed out, but this has never, in my experience, required treatment. Reid states that cardiac arrest has been reported as a complication of lavage by Lee and Ames (1965). In fact, these authors merely suggest lavage might cause arrest but presented no evidence to support this. Serious complications of lavage are rare (Matthew *et al.*, 1966; Burke, 1968).

In the individual case neither emesis nor lavage can be relied upon to empty the stomach. Corby *et al.* (1968) gave children a known quantity of magnesium hydroxide before inducing emesis using ipecac and apomorphine. The average recovery of stomach contents with ipecac was 28% and apomorphine 31.3%. They concluded that neither drug was highly efficient in emptying the stomach. During the past four years I have been carrying out similar studies in a group of 362 adults using gastric lavage. The average return of magnesium sulphate was 63.5%. During the course of this study it was realized that the position of the patient influenced the efficiency of the procedure. With the patient in a modified left-sided Simm's position, and the table tilted head down, it is possible to obtain average returns of 84% magnesium sulphate. No attention appears to have been paid to the technique of lavage when comparing it with emesis in the studies that Reid referred to. It is therefore doubtful whether any valid conclusions can be drawn from such work. There are two situations where I feel emetics would be valuable. The first is in conscious, co-operative patients (apart from those who have taken paraffin) immediately the ambulance reaches them. The second is in children who have taken seeds, berries, or enteric-coated tablets, for these are unlikely to be recovered by lavage. At the moment, emesis should be considered as an additional aid to emptying the stomach, but should not replace lavage. Finally, there are good reasons for preferring copper sulphate solution to ipecac as an emetic (Karlson and Noren, 1965).

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We showed Dr. Burke's letter to Dr. Reid, whose comments were as follows:

In my article I mentioned that there are fundamental differences between poisoning in adult patients and poisoning in children.

Dr. Burke, on the basis of his experience with 'a group of over 362 adults', seeks to pass judgement on the special problems of paediatric poisoning. Such evidence as he presents tends to support rather than detract from the use of syrup of ipecac. His finding of a 50% incidence of bleeding (site not stated) with gastric lavage is higher than the 11% incidence of severe gastric haemorrhage with lavage that I referred to, and suggests that stomach lavage is even more hazardous than was previously recognized.

His report of improved results with lavage with the patient in a modified left-sided Simm's position, and the table tilted head down, is not entirely relevant or meaningful as children were not studied, comparison with ipecac was not made, and the decisive time factors were apparently disregarded. Dr. Burke does nothing to demolish the overwhelming mass of evidence that ipecac-induced emesis is the more efficient method of initial therapy of paediatric poisoning. In particular, he does not comment on the careful, crucial, and conclusive studies of Boxer and his colleagues (Boxer, Anderson, and Rowe, 1969).

Dr. Burke believes that copper sulphate is preferable to ipecac as an emetic. As stated in my study, the toxicity of copper sulphate precludes it from serious clinical consideration (Arnold, Hodges, and Barta, 1959; Beckman, 1961; Deichmann and Gerarde, 1964; Gosselin and Smith, 1966; Shirkey, 1966).

In his last paragraph Dr. Burke agrees that emetics would be valuable in conscious, co-operative patients. In contrast to the situation in adults (Burke, 1969) poisoned children are almost invariably conscious and co-operative when first seen in the casualty department—to the extent that they are able and willing to take syrup of ipecac without difficulty or demur.

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Transient Gluten Intolerance

Sir,

I have read with interest the Case Report by Dr. Walker-Smith in your previous issue (p. 523), which describes a patient with presumed transient gluten intolerance. The author has argued his case closely and correctly makes it clear that the patient does not suffer from coeliac disease which, by definition, means permanent gluten intolerance. However the evidence for transient intolerance rests solely on the child's clinical improvement coincident with the removal of gluten from the diet. It seems to me that an alternative explanation could be considered. It is noted that just before the institution of a gluten-free diet the child's serum proteins were returned to a more normal level. Might this not have stimulated growth recommencement, as it may do in a condition such as kwashiorkor? Could perhaps this patient have been one who originally suffered a gastro-enteric type of illness which resulted in prolonged small intestinal mucosal abnormality, a recognized phenomenon? This may have been associated with protein leak and the child had protein-losing enteropathy which eventually resolved as the mucosa healed.

On the other hand, Dr. Walker-Smith's explanation may be the correct one. However, my purpose in

writing is to point out that, though in experienced and critical hands the diagnosis of transient gluten intolerance means that follow-up and retesting will be carried out, if this diagnosis is used freely by those less experienced there will may be more patients who are treated with a gluten-free diet and are uncertain how long the diet is to be continued. Those of us who are involved in investigating paediatric patients with gastro-enterological problems have many such patients referred to us with the query, does this patient really suffer from coeliac disease and how long should he be on a gluten-free diet? As I have said, present evidence indicates that the 'true coeliac' should remain on a gluten-free diet for life. When retesting the validity of a previous diagnosis of coeliac disease or wheat gluten intolerance, there is no diagnostic evidence available while the patient is on a gluten-free diet and, as Dr. Walker-Smith points out, relapse on resuming consumption of gluten-containing foods may be slow and retesting may have to be carried out periodically over several years. This is naturally very arduous for both patient and doctor. Hence accurate initial diagnosis is important.

Before a diagnosis of transient gluten intolerance is contemplated I feel that all other explanations for the patient's symptoms and signs should be carefully contemplated. If one does decide to exclude gluten, then careful follow-up is certainly mandatory.

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Attention may be drawn to the observation of McNeish in this journal (1968, **43**, 362) that if in true coeliac disease gluten is reintroduced into the diet, not more than 2 months is needed before the jejunal biopsy pattern becomes clearly abnormal. This could be relevant to the problem mentioned by Professor Anderson, of deciding whether a gluten-free diet should be continued in cases where the validity of the original diagnosis of coeliac disease has been questionable.—Editor.