should add AIDS to this list but suggested no, not because AIDS is not transmitted through casual social contact and that therefore such a procedure would be unnecessary but because previous legislation covering sexually transmitted diseases had proved ineffective. I fear Mr Porter is missing the point.

By discussing AIDS in the same article as bubonic plague, leprosy, cholera, typhoid, and diphtheria without pointing out the important differences between these conditions and AIDS Dr Porter did little to dismiss exaggerated fears that AIDS is a deadly plague of high infectivity that only severe measures can hope to control. He then said that "one should at least consider draconian measures . . . to protect others"; however, he decided not to consider them but instead dismissed these very issues by saying "experience suggests that this would be unwise." I wonder what experience Dr Porter is referring to? I am sure he is well aware that the media have aired many drastic measures to combat AIDS, and here was an excellent opportunity to answer, discuss, and hopefully dismiss them, but the opportunity was passed by.

The implications behind his concluding comment that "it takes two consenting partners to spread AIDS" are offensive and insensitive to those people who have suffered and died from this dreadful condition. People may consent to sex but I am sure they do not consent to spread AIDS. And what of the haemophiliacs and other recipients of blood products for whom this concept of consent has virtually no meaning? Many people who now have AIDS may well have been infected with HIV five or more years ago—long before the public health campaigns and even before HIV was discovered.

Important points which Mr Porter failed to emphasise are that infection with HIV and AIDS itself are not the same thing, that HIV is of low infectivity and is transmitted through the exchange of blood and other body fluids and not through casual social contact, and therefore that measures such as quarantine and compulsory admission to hospital are unnecessary and unhelpful in the management of this condition.

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HIV and sexual lifestyle

SIR,—In view of the near hysteria that seems to characterise most of the media's reaction to the AIDS epidemic, Dr Caroline Bradbeer's leading article (3 January, p 5) must surely be welcome and it is to be hoped that medical journalists and politicians will heed it and bear it in mind when they produce their copy. I feel that we could with advantage see clinical parallels between two other worldwide epidemics in modern times.

The huge pandemic of syphilis in the early part of this century started to decline before chemotherapy finally made syphilis a rarity, probably because of the popularity of condom use and local prophylaxis, which improved sex hygiene for those at risk. The popularity of male circumcision in the 1920s and 1930s may also have contributed to this happier state of affairs. As the human immunodeficiency virus (HIV) seems to be a relatively delicate creature surely to ignore the use of local (and cheap) antiviral preparations and pessaries as prophylactics in any anti-AIDS campaign borders on stupidity.

The second disease of the first half of our century to be "tamed" by man is, of course, tuberculosis.

That too started to decline well before chemotherapy finally once again made the disease uncommon. The hygienic principle in this case was better nutrition. Some five years ago, wandering through the streets of San Francisco, I was struck by the facies of many of the gay community. They reminded me very much of those of the often doomed young pulmonary tuberculosis victims that I saw haunting the medical outpatients of the Westminster Hospital in the 1940s. Those who were to die also had an immunity deficiency. Perhaps we should encourage groups at risk to look to their general health as well as their sexual lifestyle and hygiene.

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AIDS: a doctor's duty

SIR,—While agreeing with much that Dr Tony Smith (3 January, p 6) has to say about a doctor's duty in relation to AIDS, I would question his statement on antibody screening before surgery.

He claims that "those doctors who are calling for patients to have antibody tests before they undergo surgical procedures . . . are contributing to current hysteria about the disease." A surgeon is negligent if he does not perform a hepatitis screen on a patient with a history of undiagnosed jaundice. If a patient is found to be a carrier then precautions can be taken to protect medical and nursing staff during the course of the operation and aftercare.

Similarly, precautions have to be taken when operating on patients infected with the human immunodeficiency virus (HIV). As there are no clinical signs to determine the HIV carrier state the screening of people at high risk is the only method of detecting the infected patient. A careful sexual history should now become a routine part of clerking.

The "hysteria" can be prevented by education. If all patients are carefully and knowledgeably counselled at the time of venesection most will be reassured and containment of the disease may be assisted. Provision of leaflets such as that produced by the Terrence Higgins Trust¹ and the work of the support group Bodypositive are useful adjuncts.

Although I agree with Dr Smith that HIV carriage does not represent a substantial health risk for doctors, the use of screening of high risk groups before surgery not only protects against infection but also provides an opportunity to increase understanding of the disease, its transmission, and prevention among those at high risk.

Only by increased public knowledge of the disease can this hysteria, often helped by an ill informed media, be converted into a constructive appreciation of AIDS. Failure to screen patients and allow nosocomial AIDS to occur will only induce more hysterical and counterproductive reporting in the press.

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1 Anonymous. *AIDS and HIV. To test or not to test*. London: Terrence Higgins Trust, 1986.

Haematology, ethnography, and thrombosis

SIR,—Dr S Heptinstall's leading article (3 January, p 3) makes no mention of the well established association between thrombosis and particular blood groups.

I and my colleagues have analysed data published by numerous workers¹ and have shown that persons of groups A and B have a significantly higher

incidence of coronary thrombosis and other clotting diseases than those of group O, while haemorrhagic conditions are commoner in those of group O. It has also been shown that clotting factor VIII content is higher in blood of group A than in group O.² The incidence of myocardial infarction is 29% higher in persons of group A than in those of group O on the basis of 37 studies comprising 7124 cases. However, in women taking oral contraceptives the incidence of thromboembolism is nearly three times as high in those of group A as in those of group O. This result is based on only four series, comprising 236 cases. Though this is statistically very highly significant, it is desirable that larger numbers of cases should be studied in view of the possible importance of avoiding the use of oral contraception in women of group A (as well as B and AB). I have tried to persuade colleagues, particularly in family planning centres, to carry out the necessary observations, so far without success.

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1 Mourant AE, Kopeć AC, Domaniewska-Sobczak K. *Blood groups and diseases*. Oxford: Oxford University Press, 1978: 34-5, 259.

2 Bartlett A, Dormandy KM, Hawkey CM, Stableforth P, Voller P. Factor-VIII-related antigen: measurement by enzyme immunoassay. *Br Med J* 1976;i:994-6.

Dialysis arthropathy: amyloid or iron?

SIR,—Dr N R B Cary and coworkers (29 November, p 1392) suggested that iron deposits in synovial tissue may have been the cause of arthropathy in five patients undergoing long term haemodialysis.

The role of iron in the physiopathogenesis of various arthropathies, most notably rheumatoid arthritis, was invoked several years ago. Further studies, however, showed that such deposits are commonly found in cases of chronic inflammatory rheumatism and many other joint diseases and thus appear to be non-specific.¹ Based on a study we conducted on patients undergoing long term haemodialysis² we are at a loss to find any evidence in support of the hypothesis proposed by Dr Cary and his coworkers.

We conducted synovial biopsies on 19 patients. Even though iron was present in 12, usually the deposits of iron were minute. Only in one case was the iron deposit substantial, and in that case we found considerable amyloidosis.

We found iron deposits in non-symptomatic joints after synovial biopsies performed with a needle. Thus, these deposits did not seem to be any more specific to diseased joints than did the amyloid deposits in one of the authors' patients. Furthermore, it is useful to recall the similarity of bone cysts observed in these patients and in patients with secondary amyloidosis. The frequency and abundance of these amyloid deposits within the cysts, as well as the nature of amyloid, which is made up mainly of β_2 microglobulin, points to a relation between bone destruction and amyloidosis.

Among 22 patients who were dialysed for over 10 years with cuprophane membranes we found no correlation between the ferritin concentrations and severity of joint pain as was reported by Brown *et al.*³ We also could not show any association between ferritin concentrations and the extent of abnormal radiological findings (bone cysts, spondylarthropathy).

Finally, we noted a decrease of joint "algias" after a switch of dialysis membranes (unpublished results). This amelioration, which followed the

replacement of cuprophane membranes by more highly permeable material better able to filter β_2 microglobulin, supports the notion of a physiopathological mechanism linked to this molecule.

In summary, we believe that iron deposits cannot be considered to be the cause of dialysis arthropathy. That amyloidosis and β_2 microglobulin are contributing factors to this condition is undeniable, even though we cannot elucidate precisely their role in its physiopathology and explain why the clinical picture improves so quickly after changing dialysis membranes.

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- 1 d'Eshougues JR, Delcambre B, Leonardelli J, Defrance D, Rammaert-Guillot M, Rammaert-Guillot B. Secondes vues sur la présence de fer dans la synoviale articulaire. *Rev Rheum* 1976;43:469-73.
- 2 Foissac-Geloux Ph, Flipo RM, Hardouin P, et al. Aspects actuels de la pathologie ostéoarticulaire des hémodialysés. Résultats et discussion d'une enquête rhumatologique. *Néphrologie* 1986;4: 157-63.
- 3 Brown EA, Arnold IR, Gower PE. Dialysis arthropathy: complication of long term treatment with haemodialysis. *Br Med J* 1985;292:163-6.

Endocarditis prophylaxis: do parents remember advice?

SIR,—I recently undertook a similar study to that of Dr David W Pitcher and others (13 December, p 1539) while working at the paediatric cardiac unit in Liverpool.

The parents of 58 children with congenital heart disease were asked as part of the routine admission procedure what they knew about the importance of dental care and antibiotic prophylaxis for their children. Only 25 said that they were aware of the need for antibiotics or special care for their children's teeth. Eight stated that their information came from sources other than the hospital.

These results show, like those of Dr Pitcher and colleagues, that many parents are ignorant of the importance of dental care for their children. This is despite the fact that all parents are given advice, both written and oral, at the time of diagnosis and later. I agree with the authors' recommendations to try to improve this situation. With children it is probably even more important to repeat advice as the diagnosis of heart disease is often made in the first few days of life and can be completely devastating for the parents. It is not surprising that information apart from the diagnosis is forgotten. Also at diagnosis many children will not have started teething and so information on dental care may seem unimportant.

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Syrup of ipecacuanha: is it really useful?

SIR,—While the leading article by Drs J A Vale, T J Meredith, and A T Proudfoot (22 November, p 1321) provides an important historical perspective on syrup of ipecacuanha and its use, its conclusion

about usefulness seems seriously flawed. Conceivably the cause lies with their "perspective of the elephant"—that is, looking at the issue from an adult inpatient point of view.

Admittedly, until recently evidence of "clinical effectiveness" had been only inferential; but ipecacuanha had always compared favourably with gastric lavage in yields of markers—assuming, of course, that gastric lavage itself works.

In recent years, however, concrete evidence of clinical effectiveness has begun to appear—witness Tenenbein's studies in Canada¹ and Lovejoy's studies in Boston² with regard to acetaminophen. In truth, however, I must admit to a bias; I am convinced that doing anything to keep children out of emergency rooms is in itself helpful. Ipecacuanha's home use has certainly done that³; conceivably activated charcoal might prove equally effective—if only I could be assured that most children would consume the charcoal. None the less, the authors' plea for "reconsideration" seems appropriate. The past 30 years have seen elimination of many myths about syrup of ipecacuanha—its purported slowness of action, the "demand" for precise dosage, the "ineffectiveness" of "out of date preparations," the "need" for concomitant fluid administration to produce vomiting, etc, etc. While conceivably its "usefulness" is really yet another myth, let's avoid jumping on some other "bandwagon" until support of non-effectiveness is far better established.

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- 1 Tenenbein M, Cohen S, Sitar DS. Efficacy of ipecac induced emesis, orogastric lavage and activated charcoal for acute drug overdose. *Vet Hum Toxicol* 1985;27:321.
- 2 Amit Y, Lovejoy FH, Mitchell A. Effect of early induction of emesis in reducing acetaminophen serum levels in acute overdose in children. *Vet Hum Toxicol* 1986;28:499-500.
- 3 McIntyre S, Parish RA, Bobbink S, Robertson WO. Pediatric poisonings and Seattle's ERs. *Vet Hum Toxicol* 1986;28:577.

SIR,—Drs J A Vale, T J Meredith, and A T Proudfoot stated that "the low morbidity and mortality of children admitted to hospital suggest that many incidents are scares rather than true poisonings; the use of syrup of ipecacuanha in almost every case is therefore inappropriate." Might not at least a small contribution to these low rates be due to the prompt administration of ipecacuanha in the first place? Their conclusion, while reassuring, can be made only in retrospect, and we feel that even with a figure of 12% of admitted children who develop serious complications efforts must be made initially to reduce absorption of toxic substances whenever possible.

Boxer *et al* showed that the ratio of aspirin recovered via ipecacuanha induced emesis followed by gastric lavage was 2.02 whereas when lavage was performed first, followed by emesis, the lavage: emesis ratio was only 0.16, thus supporting the theory that ipecacuanha induced emesis was superior to gastric lavage for the recovery of ingested salicylate from the stomach. In comparison with the obvious hazards of gastric lavage the complications after ipecacuanha administration are rare. Indeed the death referred to was a case in which ipecacuanha may have been a contributory factor in the death of a 19 month old child due to herniation of the stomach into the chest through a congenital defect in the diaphragm.² It is safe to say that more patients undergo gastric lavage and emesis than is really necessary, but the need to empty the stomach by whatever method is mandatory in certain situations—for example, when highly toxic substances

such as iron, salicylate, or paracetamol have been taken. Yet further research is required to give us clearer guidelines on which method to use in doing so.

Syrup of ipecacuanha is undoubtedly an efficient emetic and its ease and speed of administration compared with that of lavage, allied to its safety,³ will ensure that it maintains a high profile in the initial management of overdose, especially in the younger child.

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- 1 Boxer L, Anderson TP, Rowe DS. Comparison of ipecac induced emesis with gastric lavage in the treatment of acute salicylate ingestion. *J Pediatr* 1969;74:800-3.
- 2 Robertson WO. Syrup of ipecac associated fatality: a case report. *Vet Hum Toxicol* 1979;21:87-9.
- 3 Gordon G. Ipecacuanha induced emesis in the treatment of self poisoning in adults. *Archives of Emergency Medicine* 1985;2: 203-8.

SIR,—In answering the question posed in their leading article (22 November, p 1321) Drs J A Vale, T J Meredith, and A T Proudfoot do not reach any very firm conclusion.

Ipecacuanha is a very effective emetic, and, although there have been isolated reports of complications occurring with its use, it does have quite a remarkable safety record. The question remains as to whether vomiting is actually useful, and I would take issue with the suggestion that "there is no evidence that it prevents drug absorption or systemic toxicity." This is a curious conclusion to draw after referring to papers which show that ipecacuanha induced emesis resulted in a reduction in the absorption of a therapeutic dose of tetracycline,¹ a 28% recovery of magnesium hydroxide marker,² and a 30% recovery of salicylates.³

This level of recovery may not influence the outcome in most patients, children in particular, but, like gastric lavage, the basis for promoting emesis rests more on the occasional recovery of large quantities of drug than on verification of its efficacy in routine use.

The possibility of retrieving a substantial quantity of poison and of otherwise being seen to have done nothing by parents, relatives, and ultimately the coroner are in practice difficult to resist.

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- 1 Neuvonen PJ, Vartiainen M, Tokola O. Comparison of activated charcoal and ipecac syrup in prevention of drug absorption. *Pharmacology* 1983;24:557-61.
- 2 Corby DG, Decker WJ, Moran MJ, Payne CE. Clinical comparison of pharmacologic emetics in children. *Pediatrics* 1968; 4:361-4.
- 3 Kulig K, Bar-Orr D, Cantrill SV, Rosen P, Rumack BH. Management of acutely poisoned patients without gastric emptying. *Ann Emerg Med* 1985;14:562-7.

Empirical evidence and authoritarian ethicists

SIR,—How refreshing it was to read an account of an attempt (25 October, p 1065) to obtain some evidence on which to compare different ways of obtaining informed consent to treatment. How sad it was that the response of a professional ethicist (13 December, p 1567) was to try, unsuccessfully, to discredit the attempt. How welcome it would have been if, instead, Dr Nicholson had acknowledged more explicitly that the study conducted by Dr Simes and his colleagues is one of a vanishingly small number of credible attempts to obtain empirical evidence to leaven the many