more detail, but I feel that a reassurance to patients who have had this most successful procedure carried out is necessary at this stage.

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1 McKenzie, A W, Aitken, C V E, and Ridsdill-Smith, R, *British Medical Journal*, 1967, **4**, 36.

SIR,—We read with interest the papers of Mr M K D Benson and others and Dr M W Elves and others (15 November, pp 374 and 376 respectively). We can confirm that since our paper on the subject of cobalt toxicity in relation to McKee hip arthroplasty[1] was published we also have found that approximately one-third of patients with a metal to metal (cobalt-chrome-molybdenum alloy) prostheses in situ will give a positive reaction to one of the metals of skin testing.

Of 20 patients so far reviewed, four are cobalt-positive, two nickel-positive, and one cobalt- and nickel-positive. The significance of a positive patch test remains unproved, however, and neither of the articles you have published have provided sufficient histological, immunological, or radiological evidence to permit any firm conclusion. Of the patients reviewed by us, there have been several with a positive skin patch test and yet perfect hip function. This is also the experience in at least one other centre.[2]

There are at least two points arising which merit further discussion. Firstly, metallic cobalt and cobalt salts are not only irritants but are directly toxic and this in itself may be significant in the production of any necrotic reaction around the joint. Also, polymethacrylate cement is not beyond suspicion as a possible cause of necrotic reaction. It is important that conclusions such as that of Mr Benson and his colleagues that "it therefore seems advisable to use Charnley or other forms of metal-to-plastic prostheses in preference to metal-to-metal ones" should not be made on the basis of a statistical relationship between skin sensitivity and loosening of the prostheses, particularly when only two cases of loosening are mentioned, neither of which is described nor is the orthopaedic evidence for loosening presented. We are more in agreement with Dr Elves and his colleagues and the guarded conclusion in their paper.

Secondly, we feel it is of great importance that authors on this subject state clearly which metal is used in the metal-to-plastic joints. Thus the femoral component of the Charnley prostheses is produced in stainless steel (for example, Thackray) and cobalt chrome alloy (for example, Down Bros). It may be that it is the use of stainless steel articulating with high-density polyethylene that leads to a low rate of increased metal sensitivity rather than the fact that it is a metal-to-plastic joint. We would therefore ask all orthopaedic surgeons who use the metal-to-plastic prostheses to document clearly in their case notes the particular material used in the femoral component as this may be of great significance at a later date. In this centre the information is being documented in the theatre register.

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1 Jones, D A, *et al*, *Journal of Bone and Joint Surgery*, 1975, **57B**, 294.
2 Ring, P A, personal communication, 1974.

Use of clonazepam in epilepsy

SIR,—In his article on epilepsy (1 November, p 270) Dr F B Gibberd did not mention the use of the newer benzodiazepine anticonvulsant clonazepam, which has been shown to be effective both in the control of seizures in childhood[1] and in intractable epilepsy.[2] This drug may also be useful in controlling status epilepticus without resort to the use of anaesthesia, muscle relaxants, and positive pressure ventilation, as the following brief clinical summary illustrates.

A 6-year-old boy with epilepsy and mental handicap following encephalitis at 4 months of age was admitted in status epilepticus in February 1975. Seizures were well controlled with intravenous diazepam and intramuscular paraldehyde for four days. Thereafter they recurred despite the continuation of this therapy, and treatment with intravenous clonazepam was started. An initial dose of 3 mg in a 500-ml infusion of 5% dextrose was ineffective, but by gradually increasing the dose over the next 48 hours the seizures were controlled. A total of 43 mg of clonazepam was infused. Despite this high dose there was no evidence of respiratory depression, hypotonia, or excessive bronchial secretion. Subsequently the dose was reduced and good control was maintained with 3 mg daily by mouth in combination with phenobarbitone.

Although further experience with clonazepam is required, it may have a useful place in the control of status epilepticus, as previous reports have suggested.[3 4]

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1 Martin, D, and Hirt, H R, *Neuropädiatrie*, 1973, **4**, 245.
2 Hooshmand, H, *Archives of Neurology*, 1972, **27**, 205.
3 Gastaut, H, *et al*, *Epilepsia*, 1971, **12**, 197.
4 Ketz, E, *et al*, *Acta Neurologica Scandinavica*, 1973, suppl 53, p 47.

"Syrup of ipecacuanha"

SIR,—Your leading article on childhood poisoning (29 November, p 483) deserves wide support for its recommendation that doctors, nurses, and ambulancemen should keep syrup of ipecacuanha at hand. It stores well and works well, but care must be taken to obtain the correct syrup. The American original is ipecac syrup *USP*. This can readily be made to order, but some pharmacists are not familiar with it. To ask for an unfamiliar preparation is to risk being supplied with ipecacuanha liquid extract *BP*, 15 ml of which would be a massive overdose. Deaths have been caused by this mistake.[1 2] I have heard of a recent occasion when the liquid extract was supplied but was not administered.

The British syrup carries the cumbersome title "ipecacuanha emetic draught, paediatric *BPC*." This is clearly unsuitable for everyday verbal use and a simple name should be made official.

M S Fraser

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1 Smith, R P, and Smith, D M, *New England Journal of Medicine*, 1961, **265**, 523.
2 Bates, T, and Grunwaldt, E, *American Journal of Diseases of Children*, 1962, **103**, 169.

*** Dr Fraser sent a copy of this letter to the secretary of the *BPC* Revision Committee, whose reply is printed below.—ED, *BMJ*.

SIR,—Ipecac syrup *USP*, on which the original work was based, is not readily available in this country, but ipecacuanha emetic draught, paediatric *BPC* is almost identical and is easily prepared from readily available ingredients using the formula given in the *BPC*. At the time when this preparation was introduced the Codex Revision Committee was aware that misunderstandings had arisen over the meaning of "ipecacuanha syrup" and considered that the use of the term "syrup" for such medicines should be discouraged, as medicated syrups are commonly syrupy stock solutions of drugs for use in extemporaneous preparations. It decided that the title should provide an exact description of the product, the use of which would avoid any possibility of misinterpretation.

At the time of compilation of the *BPC 1973* it was considered that the draught should be given only under medical supervision and not included in first-aid kits, and a statement to that effect appears on p 662. The addition of the preparation to first-aid kits would reinforce the need for a precise title to appear on every bottle so as to avoid any misunderstanding as to the purpose of the contents. However, I agree that there are many occasions when a short title would be useful and I shall see that this is considered when the monograph is revised.

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Codex Revision Committee

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Streptococcus mutans and dental caries

SIR,—I read your leading article "Immunisation against dental caries" (22 November, p 424) with personal interest, but I would like to take you up on one small point.

In mentioning the fact that evidence is accumulating that *Streptococcus mutans* may be associated with dental caries in man you give three references. You then go on to say, "Such a role had been suggested originally in 1924." It was my late father who first described this organism in 1924 when working in the laboratories of Sir Almroth Wright at St Mary's Hospital with a grant from the Medical Research Council.[1] He was attempting to determine the role played by *Bacillus acidophilus* (one of the lactobacilli thought to be implicated) in the aetiology of dental caries. It was found that *B acidophilus* could be isolated only from teeth in which cavities were already formed or in which the foci of caries were shallow and contamination from the surface could not be excluded. During the experiments he grew from culture an organism not previously described to which he gave the name *Str mutans*. This, he found, grew best in a medium with a reaction approximating to that of saliva; it was found in the earliest stages of decay and could be isolated in pure culture from carious dentine which is more or less effectually protected from secondary infection or surface contamination.

Since it is hardly my father's fault that