months. Three of them had had unsuccessful kidney transplants before CAPD and the fourth, a 3-year old, had Wilms's tumours. Although our experience is still too short to allow us more generalised conclusions, we are convinced that the availability of CAPD increases the therapeutic armamentarium in the management of children with end-stage renal disease.

D G OREOPOULOS
A KATIRTZOGLU

Toronto Western Hospital,
Toronto, Ontario M5T 2S8, Canada

G ARBUS

Hospital for Sick Children,
Toronto, Ontario M5G 1X8

P CORDY

St Joseph's Hospital,
London, Ontario N6A 4V2

¹ Oreopoulos, D G, *Dialysis and Transplantation*, 1979, 8, 460.

History of an improvement

SIR,—I was interested to read Dr C J Levy's letter "History of an improvement" (26 May, p 1426).

In 1976 the Coventry anaesthetic department decided that all operating theatres in the Coventry area should be equipped with anaesthetic gas scavenging facilities, and this has now been done. When the work started there were not many other such systems from whose designers we could gain experience. Not surprisingly, we had several problems initially; but thanks to a very helpful area works department, some fine work by the hospital engineers, and useful constructive criticism by members of the anaesthetic department we overcame them. With the systems now in daily use the improvement in working conditions is commented on by all who have to work in the operating suites. We hope that there may also be more successful pregnancies among female operating theatre staff than there were before the scavenging systems were introduced.

In summary, I fully agree with Dr Levy that "complicated physics" demands the services of experts, and that engineering works that can affect patients must not be undertaken unilaterally. I strongly disagree with his Hutter's Law stating that "improvement means deterioration."

R S C HOWELL

Anaesthetic Department,
Walsgrave Hospital,
Coventry CV2 2DX

Doctors and children's teeth

SIR,—We are much obliged to Dr A J S Waterston (2 June, p 1487) for drawing attention to fluoride tablets, but we think he may be giving a wrong impression by suggesting that a child should start on tablets at 2 years.

In accordance with Dr Benjamin Spock's suggestion, we advise that an expectant mother should take them in the last six months of pregnancy and the child from birth up to the early teens. An American dentist has demonstrated that dental caries can be almost entirely prevented by this method.¹

Taking tablets does not restrict their use to the most dedicated parents. For some 20 years Austria has had a nationwide fluoride tablet distribution scheme, which has resulted in the children in the 6-14 age group having just half as many teeth in need of treatment as children

in Britain. In this country Birmingham has achieved the same success through having a very efficient water fluoridation scheme since 1964.

P STOCKER

Dental Health Promotion Limited,
London NW3

¹ Glenn, F B, *Journal of Dentistry for Children*, 1979, 46, 17.

Cimetidine for hypertrophic prostate?

SIR,—The antiandrogenic effect of cimetidine has been demonstrated recently in both animal and human studies.^{1,2} In the former animal and human studies has also been shown. One wonders if there is any place for the use of the product in benign hypertrophy of the prostate in man. The age at which it would be used would give little concern about reduction in sperm count.

We do not as yet know enough about the long-term use of cimetidine, or the optimal dosage for this type of effect, but it would be interesting to know if anyone had considered the feasibility of such a study.

MAURICE COHEN

Highcliffe, Dorset

¹ Winters, S J, *et al*, *Gastroenterology*, 1979, 76, 504.

² Van Thiel, D H, *et al*, *New England Journal of Medicine*, 1979, 300, 1012.

The dilated upper urinary tract

SIR,—Your leading article on this subject (26 May, p 1382) was a timely focus on a clinical problem of increasing importance to urologists. It was unfortunate therefore that it contained several inaccuracies; we hope you will afford us the courtesy to correct these and update your account.

Firstly, it was stated that "the Manchester group believe that obstructed and non-obstructed dilated systems can be distinguished by comparing renograms in the dehydrated and hydrated states." Nothing could be further from the truth. What the diuresis renogram compares is renography in the normally hydrated patient with that performed during a brisk frusemide-induced diuresis, which increases the urine flow rate across the site of suspected obstruction to 10 ml/min or more—a situation similar to that artificially induced during urodynamic perfusion studies but without the coincidental invasion. We ourselves have stressed the pitfalls associated with dehydration renography, which can produce an obstructive pattern even in normal subjects.¹

Secondly, it is stated that "others who have compared the Manchester method with dynamic studies have found that even renography on hydrated patients can overdiagnose obstruction in those with greatly dilated upper tracts and underdiagnose it in those with early obstruction but minimal dilatation." A 1975 reference is given in support of this statement² yet our technique was first reported in 1978.³ To our knowledge no one has yet compared the technique with perfusion studies (although our own current work in this context shows good correlation). Your comments surely refer to standard, unmodified renography and describe the very reason for the development of our technique, which overcomes these deficiencies.

Thirdly, you state that the accuracy of the procedure has been compared only with operative findings. Twelve months ago we reported the results of a further study correlating the diuresis renogram with objective morphological changes in the renal pelvis of patients with idiopathic hydronephrosis removed at Anderson-Hynes pyeloplasty and examined by light and electron microscopy.⁴ Of 26 examinations, there was accurate correlation between our functional dynamic isotope procedure and the morphological changes in 24 and equivocal results in two, providing further evidence for the accuracy of the technique.

I trust you will record these important details so that any misconceptions may be corrected for the benefit of those clinicians wishing to continue using a simple, non-invasive, and accurate technique for the assessment of the dilated upper urinary tract while reserving more complex and invasive procedures for the small group of patients in whom the findings remain equivocal.

P H O'REILLY
E W LUPTON

Department of Urology,

H J TESTA

Department of Nuclear Medicine,
Manchester Royal Infirmary,
Manchester M13 9WL

¹ O'Reilly, P H, Shields, R A, and Testa, H J, *Nuclear Medicine in Urology and Nephrology*. London, Butterworths, 1979.

² Whitaker, R H, *British Journal of Urology*, 1975, 47, 377.

³ O'Reilly, P H, *et al*, *British Journal of Urology*, 1978, 50, 76.

⁴ Lupton, E W, *et al*, *British Journal of Urology*, 1979, 51, 10.

Biochemical testing for acute medical emergencies

SIR,—To do as Drs B T Williams and R A Dixon (19 May, p 1313) suggest and have an external advisory service in clinical biochemistry would, as I see it, take away from consultant chemical pathologists their raison d'être. But my chemical colleagues need have no fear, for Drs Williams and Dixon are quite naive when they talk of the "pooled experience of acknowledged experts."

Firstly, this would mean that the two groups of acknowledged experts, clinicians and biochemists, would physically have to come together, and this they simply do not do. Over the past three years I can think of only one scientific meeting to which both groups have been invited. And on this occasion they were as different as chalk and cheese, with little in common. Secondly, it assumes that you can get experts to agree. For many years now international experts in clinical enzymology have been wrangling over what is the best temperature to assay enzymes and have still not come up with an agreed figure. What hope is there for agreement on what tests are best in a given clinical situation?

So as not to appear totally destructive, may I propose a solution to some of the problems highlighted in this paper and many others? It is formulated on the premise that, just as clinicians consider themselves experts in clinical medicine, biochemists should also consider themselves experts in clinical biochemistry. My proposal is that clinicians, when requiring the assistance of clinical biochemistry for their diagnosis, simply submit a request stating the clinical details or the differential