

Ammonia in the Eye

SIR,—I was interested in the paper of Dr. V. N. Highman (8 February, p. 359) concerning ammonia burns and the eye.

Recently a patient was seen two hours after a jewellery robbery, having had ammonia thrown in her eyes. One eye showed loss of corneal epithelium and conjunctival injection. The other showed gross conjunctival injection, loss of corneal epithelium, stromal haze, and folds in Descemet's membrane. There was much fibrous material in the anterior chamber, iris atrophy, a vertical oval-shaped pupil, and subcapsular lens opacities. The intraocular pressure was normal in both eyes and has never subsequently been raised. The lens opacities later became similar to those of glaukomflecken.

At present, five months after the accident, the patient has developed band-shaped corneal degeneration in the more severely affected eye and heterochromia of the iris, but the lens opacities have not progressed.—I am, etc.,

R. MCGUINNESS.

Moorfields Eye Hospital,
London E.C.1.

Acute Abdomen in Children

SIR,—Why does Mr. P. F. Jones (1 February, p. 284) think it is wiser to do a laparotomy when the diagnosis of acute mesenteric adenitis has been attached to the child in pain, than when any other label has been attached? I admit that in the absence of a laparotomy the diagnosis remains unproved, but to attach a diagnostic label to a condition and then to say that a laparotomy must be carried out in order to prove it is surely wrong. Surely the decision to carry out a laparotomy rests not on a diagnostic label but purely and simply on the physical findings in the child's abdomen—with repeated and careful examination if necessary?

I agree that a "syndrome of acute non-specific abdominal pain of childhood can be identified," but I am sure that Mr. Jones would agree that we should not rest content with this and assume that all these cases have a single aetiology. It is a fair assumption that many cases of mesenteric adenitis in childhood are due to virus infections, and I would hope that further virological research coupled with careful detailed clinical observation of the type so well carried out by Mr. Jones might in the future improve the precision with which we can diagnose this disease. In the meantime it surely cannot be wrong to make a tentative diagnosis of this disease even if it remains unproved; it does not preclude the correct management of the condition, which is by laparotomy if the physical signs warrant it. Mr. Jones has done so much to throw light on a very difficult subject that I think it is a pity that he should say "thus far and no further." Far from agreeing with his plea to abandon attempts to diagnose acute mesenteric adenitis at the bedside, I would plead for much greater attempts to diagnose the condition accurately, with or without a laparotomy.—I am, etc.,

R. H. JACKSON.

Children's Department,
Royal Victoria Infirmary,
Newcastle upon Tyne.

SIR,—In Mr. Peter F. Jones's interesting paper on acute abdominal pain in childhood (1 February, p. 284) there is no mention of rectal examination. I am wondering whether this clinical examination is now falling into disrepute; either because it is an unpleasant experience for a child, or because it is of little value?

Having been brought up on the "finger/foot" dictum, I am surprised at its omission, and would like clarification of its usefulness.—I am, etc.,

D. J. MURRAY BRUCE.

Annandale,
Wentworth, Surrey.

Acute Urography

SIR,—Your correspondents, Dr. J. Roylance and others (15 February, p. 442) do a grave disservice to the practice of radiology in deploring "any attempt to introduce the concept of routine urography." The article of Mr. R. D. W. McLean and others (18 January, p. 142) is interesting, stimulating, and valuable and was of particular interest

to me. I have just completed a large series (over 250 cases) of investigations which they term "acute urography" in patients referred from outpatient clinics quite unprepared and normally hydrated, with excellent results (submitted for publication). It should be the aim of all radiologists to devise methods to enable the clinician to be presented with accurate and therefore valuable diagnostic evidence as rapidly as possible—we must indeed tailor each examination to the particular problem, and I would submit that is just what Mr. R. D. W. McLean and his colleagues have done.

Finally, since Dr. Roylance and his colleagues have brought in the simile of tailoring, may I point out that Burton suits for all from a single well-tryed pattern are far more acceptable to the population as a whole than solitary Savile Row masterpieces. Surely the greatest good for the greatest number using an already well-tryed or tailored technique is what we should as radiologists be aiming to supply.—I am, etc.,

W. M. C. ALLEN.

Department of Radiodiagnosis,
Royal Infirmary,
Aberdeen.

Tetracycline and Nystatin

SIR,—The comments on our recent report (16 November, p. 411) from Dr. G. Holti (28 December, p. 829), Dr. D. D. Adams (11 January, p. 122), and Professor H. I. Winner (18 January, p. 186) include some curious criticisms of our paper which might persuade an uncritical reader that the trial on which our report was based was so badly designed and analysed that no conclusion, however limited, could be valid.

Dr. Holti says, in effect, that we could not "arrive at any valid conclusions" because we (i) did not "clear" patients of "excess" *Candida* (whatever that means) before treatment, (ii) accepted a denial of having a sore mouth as evidence of not having a sore mouth, (iii) did not measure flatulent distension, frequency of defaecation, and volume of faeces, (iv) did not take stool cultures daily for several days before, during treatment, and for at least seven days afterwards, and, at the same time, culture scrapings from the corners of the mouth, perineum, and vagina—that is, did not culture more than 80 specimens from each female and more than 60 from each male, (v) did not measure "reactions to *Candida*" by an unspecified number and nature of "established immunological techniques."

Dr. Holti's argument that because not everything that might have been done was done, no valid conclusions can be made from what was done, is in our opinion erroneous. Certainly if we had elaborated the mycology, as Dr. Holti suggests, by taking more than 30 times as many swabs, we would have gleaned more information, but not information that was essential either to the purpose of the trial or for making useful valid inferences, and we did not think the cost of such elaboration could be justified by the relatively small additional yield expected. Of course some patients who have an infected mouth, as judged by objective criteria (inspection, microscopy, etc.) will not complain or admit to having a sore mouth, and conversely some with no apparent disease will complain of a sore mouth or say they have a sore mouth when questioned directly. But the presence in a double-blind trial of such

well-known ambiguities in symptomatology no more invalidates the results than does a difference in any other attribute.

In his letter Dr. Adams expresses the view that the inclusion in this trial of patients "already suffering from gastrointestinal symptoms before receiving the test drugs" invalidates the conclusions. He argues that "Table III shows that many of the subjects had signs of an alimentary disorder, probably due to infection by micro-organisms such as the currently ubiquitous adeno-, Coxsackie, or E.C.H.O. viruses." This seems to us illogical. Table III is of symptoms, not signs. The symptoms recorded on entry to the trial were in answer to questions beginning, "Have you had . . . ?" This method of interrogation, rather than virus infection, accounts in our view for the rather high incidence of positive answers. But even if many of the patients had been infected with a ubiquitous virus our failure to find a significant difference between symptoms at ten days in the two treatment groups would still be valid. In making a choice of therapy the physician must in general consider the results to be expected in the patients he actually has to treat, and if viruses are "ubiquitous" should also allow for these.

Moreover, separation of the patients into those with and without symptoms on admission gives no support to Dr. Adams's ideas. Taking those without symptoms on admission to the study (38 tetracycline and 29 nystatin) the two treatment groups were still reasonably balanced for age (60 years or more: 26/38; 14/29), proportion with bronchitis (25/38; 20/29), and history of antibiotics (14/38; 10/29), although less so for sex (females: 5/38; 8/29). Analysis of these shows that:

(i) The incidence of symptoms at 10 days was not significantly different in the two treatment groups (10/38 tetracycline, 10/29 nystatin; $\chi^2_c = 0.206$, $P > 0.5$).

(ii) In those without symptoms on admission there was also no association between *Candida* in rectal swabs and symptoms at 10 days ($\chi^2_c = 0.158$, $P > 0.7$).