Correspondence

Letters to the Editor should not exceed 500 words.

"Five-day Courses" and Respiratory Infections

SIR,—Last week one of the leading pharmaceutical companies sent a postal advertisement to the medical profession announcing that their range of penicillins was being marketed in liquid form. The heading reads, "One bottle stays the prescribed course," and the leaflet explains that "this development reflects current medical thinking that, for maximum benefit, a course of antibiotic therapy should be of at least five days' duration." This statement, coming from what may be regarded as an authoritative source, cannot be allowed to pass unchallenged. Notwithstanding the sophistic insertion of the words "at least," the general purport of the advertisement is designed to reinforce the widespread and dangerous misconception that antibiotics should be prescribed in limited courses.

I have never been able to discover how this idea of a "five-day course" originated, but when applied to the treatment of respiratory infections it is responsible for a great deal of unnecessary illness and avoidable hospital treatment. Time and again a patient suffering from acute bronchitis or upper respiratory infection is given five days' supply of antibiotic tablets and is told by his doctor that this is a "five-day course." Consequently, when the tablets run out the patient ceases to take the antibiotic, regardless of his condition at the time, and a few days later he has to be admitted to hospital with pneumonia. Also the dosage given in such "courses" is often inadequate. Usually 250 mg. of tetracycline or ampicillin four or even three times daily is prescribed, when 500 mg. q.d.s. may well be necessary—at least for the first day or two—depending upon the severity of the infection.

An additional danger lies in the fact that when a patient is receiving an antibiotic his sputum is almost always free from pathogens, whether the antibiotic is controlling the infection or not, and this effect often persists for some days after the withdrawal of the drug. Consequently when he is admitted to hospital it is usually impossible to identify the causal organism and study its antibiotic sensitivity.

The conception of an antibiotic "course," whether of five or any other number of days, is completely illogical. Once started, an antibiotic should only be stopped when there is good evidence that the infection has been eliminated. When an antibiotic is prescribed for a respiratory infection the doctor should always arrange to see the patient again before the tablets run out. He should then decide by the usual criteria (control of symptoms, presence or absence of purulent sputum, fever, or leucocytosis) whether or not to continue the treatment. The risk of continuing an antibiotic, if necessary for several weeks, is negligible compared with the risk of a serious recrudescence of the infection if it is stopped too soon.—I am, etc.,

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Subdural Haematoma and Effusion in Infancy

SIR,—Mr. Kenneth Till, in reporting his observations in 116 infants, claims that treatment by subdural pleural shunt is preferable to the older method of removal of the subdural membrane by craniotomy (17 August, p. 400). However, in presenting his data has he really shown this? I submit that he has not, and that the matter is still *sub judice*, largely because his observations lack proper controls.

In his Table IV he compares two nearly equal groups of children treated in these ways, and writes that in the group treated by a shunt operation "a higher proportion were found to develop with normal intelligence, although the follow-up period for these patients was shorter." Yet the two groups are not comparable, for they have not been matched case for case for controls. Presumably the average age of the infants in both groups at the time of operation was 5 to 7 months, and yet the group of 42 infants treated by craniotomy alone were followed up for an average of 5 years and 4 months, indicating that their average age at the time of assessment was 5 years 10 months. In contrast, the group treated by subdural pleural shunt contained 34 infants, who were followed up for an average period of only 1 year 6 months. This would make the average age of these infants at the time of assessment at just 2 years.

How can one therefore make a valid comparison between two groups of children, one assessed at an average age of just 2 years (the shunt group) and the other assessed at an average age of 5 years 10 months (the craniotomy group)? This, however, is what the author has done, and he infers that in the shunt group assessed at an average age of just 2 years 86% were of normal intelligence, none was educationally subnormal, and 11% were ineducable; whereas in the craniotomy group assessed at an average age of 5 years 11 months 66% were of normal intelligence, 21% were educationally subnormal, and 7% were ineducable. I submit that these are not valid statistical comparisons.—I am, etc.,

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Infection Risks of Haemodialysis

SIR,—I have read with interest the report of the working party to the Public Health Service on prevention of spread of infection in haemodialysis units (24 August, p. 454). Although I have had seven years' experience of maintenance haemodialysis and have been responsible for the treatment of over 70 patients, I do not feel sufficiently qualified to comment on the remarks on hepatitis, as I have seen this occur only once in a patient in over 100 patient-years with this form of treatment. In spite of this, it may be of some interest for you to note that I have never employed any of the precautionary methods recommended by the committee to prevent the spread of hepatitis, but instead have avoided the use of blood transfusions,[1 2]

and have steadfastly refused to mix dialysis equipment used for acute and chronic dialysis patients. In addition, for the past four years I have used home dialysis exclusively.[3]

I do, however, feel qualified to speak on the subject of shunt sepsis. In the past two years at the National Kidney Centre 42 patients have been treated, employing a technique involving no specific precautions when handling cannulas—that is to say, no masks were worn, no gloves were worn, and the skin around the shunt was only cleaned if debris or discharge was present. Five significant shunt infections have occurred in 17 years of patient treatment using this technique, which compares more than favourably with the figures reported by the working party, referring to patients treated at centres where the care of cannulas has become so fastidious as to make the expansion of this form of treatment extremely difficult. It is my personal belief that the fundamental health of the patient, and particularly the health of his skin, is more important in resistance to infection than any preventive measures taken at the time of handling the cannulas. Perhaps the increase in dialysis frequency of three to four times a week that we practise at the National Kidney Centre has contributed to the success of our results. In addition, of course, the domestic environment in which the patients are trained, together with the accent on the patients handling their own cannulas, may again be contributory factors.

Finally, I would like to confirm the observation made by Brescia *et al.*,[4] reported in their communication, that there has been no sepsis associated with the use of the arteriovenous fistula as an alternative to the external shunt. In five patients using the internal fistula in their own homes for a period of between 1 to 11 months (total patient experience 27 months) there has not been a single episode of skin infection, nor the requirement for the use of a single antibiotic in any patient during this period of time. I believe this last observation may be of the utmost clinical importance in the future management of patients on maintenance haemodialysis.—I am, etc.,

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Hepatitis-free Plasma Protein Solution

SIR,—Some of the conclusions reached by Drs. A. J. Salsbury and M. Brozovich (10 August, p. 352) are inadequately supported by the evidence they give. Although clinical assessment of the response to plasma protein solution may be reassuring, the efficacy of the solution in comparison with other colloids can be judged only on measurement of plasma volume. Few would have thought the effect of fresh homologous plasma so unpredictable before Hutchison[1 2] measured it. Ultraviolet irradiated plasma stored for six months at 30–32° C. gives predictable expansion on measurement.[2] Measurement of the effect of the plasma protein solution used by Drs. Salsbury and Brozovich should also be made.

Further, it is claimed that plasma protein solution carries no risk of transmitting serum hepatitis. The work quoted as evidence that heating plasma to 60° C. for 10 hours inactivates the agent of serum hepatitis does not support that conclusion, which seems to be based on the failure to produce serum hepatitis after the administration of 2 ml. icterogenic plasma to five volunteers.[4] Plasma protein solution may well be safe in this respect, but its safety can only be judged on the basis of a properly conducted prospective trial. Redeker and others[5] have recently published the results of such a trial using plasma, ultraviolet irradiated and stored for six months at 30–32° C. (a procedure hitherto thought to be virucidal). Redeker confirmed chemically and histologically that 12 of 120 patients given this plasma developed hepatitis. Only four of these patients were clinically icteric. Drs. Salsbury and Brozovich do not give details of the methods they used six months after transfusion to assess whether hepatitis had developed, and they do not raise the question of anicteric hepatitis. It is clear that rigorous methods are necessary to establish true incidence of serum hepatitis, and since there is no indication that such methods were used the safety of plasma protein solution is still in question.

The particular advantage claimed for plasma protein as a colloidal infusion solution is freedom from interference with blood grouping and cross matching as a result of rouleaux formation. This may be a disadvantage of some dextran fractions of higher mean molecular weight than dextran 70 (Macrodex), but Dr. Salsbury himself has earlier shown (14 October 1967, p. 88) that Macrodex does not lead to difficulty in blood serology. Thus, I feel, for a number of reasons, that a strong case for the use of plasma protein solution as a source of colloid has not been made. Furthermore, its cost is 12 times that of Macrodex, which is a safe and effective colloidal infusion solution. If plasma protein solution can be shown to be safe and effective it might be preferred in some patients in whom albumin infusion is particularly desired, until such time as human albumin becomes more freely available in this country.—I am, etc.,

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Waiting for Doctor

SIR,—You are to be congratulated on drawing attention (24 August, p. 447) to the time that patients must wait to go to hospital. You refer to the patient's fear that his condition may deteriorate during this wait. Recent evidence[1] suggests that this fear is shared by some doctors and may be well founded. It is not wholly accurate to state that with this in mind the medical man decides on the urgency of his patient's need for hospital treatment. Outpatient waiting times of several months severely constrain general practitioners and limit their ability to take meaningful decisions regarding the timing of investigations.

If urgent appointments are requested, patients may still wait, as did one with postmenopausal bleeding, for four weeks. Even after waiting to see the consultant as an outpatient, another patient has been reported as waiting eight more weeks before having an "actively proliferating and much less well differentiated than usual" adenocarcinoma removed.

It has already been suggested that general practitioners may not in some cases always be able to identify, at the point of referral, those patients who need urgent appointments. They may only be able to identify patients who need investigation.

Thus, a 40-year-old man wes referred for urological investigation because he had had two attacks of urinary infection. Eleven weeks later the consultant surgeon was also unable to find any abnormal physical signs, but agreed that investigation was indicated. Three weeks later still, that is, 14 weeks after referral by the general practitioner, carcinoma of the bladder was detected by cystoscopy. It is difficult to see how an urgent appointment could reasonably have been requested, and difficult to deny that this patient's condition must have deteriorated during the delay of more than a quarter of a year. Indeed, in your own earlier leading article on bladder cancer[2] you stressed the importance of early recognition in relation to prognosis and wrote, "Delay can in fact be disastrous."

Appointing a senior member of the hospital staff to review waiting times periodically, although a step in the right direction, is far from an adequate remedy. First of all, the times that patients must wait for appointments and admissions, and hence often for basic investigation, should be published with analysis by region and specialty. It is remarkable that such figures are not already available. Secondly, the retrospective analysis of the records of patients proved to have progressive disease, with reference to the referral date by the general practitioner, as has already been suggested,[1] might well reveal the extent of this problem. Thirdly, the traditional system of using outpatient departments to screen admissions may need to be reviewed. Is it strictly necessary for patients requiring, for example, a diagnostic dilatation and curettage to be made to wait for outpatient appointments, or could they be admitted direct, as suggested by the Operational Research Unit at Oxford ?[3]

Our executive council sends regular circulars to local general practitioners, and at the time of writing the delays before the first available health service outpatient appointment in this area are: general surgery, 13 weeks; E.N.T., 14 weeks or 17 weeks (two different consultants); and gynaecology, 14 weeks or 17 weeks (two different consultants). I believe these delays may constitute a serious hazard to some of the patients concerned.—I am, etc.,

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