and they use their nebulizers every hour or so . . . they become tolerant very quickly. . . . My standard practice is to regard this excessive use of the nebulizer as evidence of deteriorating asthma and therefore to increase the corticosteroid maintenance dose as soon as the number of inhalations exceeds 8-10 in 24 hours."

It would therefore be interesting to compare the increase in asthma mortality observed in some countries with the frequency and practice of using corticosteroids in asthmatic patients, since the reluctance on the part of many doctors to use steroids is well known and one-half of the asthmatics who died in Great Britain (1961-6) were found *not* to have been treated with steroids in their final illness.⁸ In support of the possibility that a decreased use of steroids was related to the increase in mortality rate is the finding of Gandevia⁹ that steroid prescriptions were decreased in one Australian state where the asthma mortality doubled and were two to three times greater in another state where the asthma mortality increased by only 50%. After corticosteroids were introduced into England and Wales in 1952 the number of deaths due to asthma decreased progressively, from 1,879 in 1952 to 1,507 in 1957.¹⁰ Thus one wonders whether or not the use of steroids decreased in England and Wales from 1959 to 1966, when deaths due to asthma increased from 1,214 to 2,040?

Dollery stated¹¹ about the situation in Britain that "there was roughly a doubling of hospital admissions from asthma . . . after the warnings . . . because physicians . . . were recognizing that deteriorating asthma was a more serious condition than they had realized." These sentiments have also been expressed by Gandevia¹² and Harris.⁷—I am, etc.,

BERT SPILKER

Department of Pharmacology,
Sterling-Winthrop Research Institute,
Rensselaer, New York

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Alcoholism Research

SIR,—Government research policy in regard to alcoholism was brought up in the last paragraph of your leading article (22 September, p. 602) reviewing recent studies of outcome. At a time when the Department of Health and Social Security is devoting more attention to alcoholism than ever previously, I find your remarks a little unkind. Why should not a systematic follow-up of the research reviewed in your editorial find financial support from the D.H.S.S.? I guess that it would, as would "a systematic follow-through of . . . any other research" be able to attract support from the D.H.S.S. or the Medical Research Council. There is

a dearth of proposals for research in the field rather than a lack of support.

Government research policy seems to be no more "haphazard" with regard to alcoholism than could be said of any other field where many different avenues need pursuing, from enzyme studies in rats to social studies in Camberwell. Co-ordination between such disparate approaches must necessarily remain loose without deserving to be called "little integrated" and "haphazard." The D.H.S.S. is certainly very concerned with developing systems for monitoring alcoholism and is now jointly with the M.R.C. looking at potential research over the whole field. The addiction research unit at the Maudsley Hospital and the Institute of Psychiatry is largely funded by these two bodies and we continue to hope for knowledge from this source. It would be a pity, therefore, if you were to give readers the impression that the appropriate Government bodies were unconcerned with the problem of alcoholism research.—I am, etc.,

NEIL KESSEL

Department of Psychiatry,
University Hospital of South Manchester,
Manchester

Loud Knocking on the Back Door

SIR,—I was somewhat taken aback to read in the *B.M.J.* (6 October, p. liii) a large and expensive advertisement in the personal columns headed "Consultant Surgeon." In this the advertiser openly solicited a medical school place for his son, whose "A" level grades were below the usual entry requirements of most medical schools.

That a reputable journal such as the *B.M.J.* accepted and published this advertisement, the nature of which must cause offence to many people, surprised me. Indeed, my colleagues and I found the wording of this piece so outrageous that we seriously doubted its authenticity.

I do not pretend that entrance to medical schools via the "back door" does not exist, but never before have I seen it exhibited so blatantly. As the *B.M.J.* is regularly scanned by the general press, I should have thought that advertisements of this kind were inviting public criticism of both this journal and the medical profession as a whole.—I am, etc.,

JOHN SQUIRE
Medical Student

London N.3

Intermediate Coronary Syndrome

SIR,—I read with interest your leading article on this subject (22 September, p. 601). Referring to the use of anticoagulants in this condition it is stated: "The rationale of using these drugs is now doubtful and conflicts with modern observations on the low incidence of coronary thrombosis in myocardial infarction."

I have no argument with the first part of this statement but I must disagree with the last part. It is not my experience, or that of my colleagues, to find a low incidence of coronary thrombosis in fatal cases of myocardial ischaemia; in fact, the opposite is true. In 1970 Professor G. L. Montgomery and I published a paper¹ on a two-year study of the cardiac lesions in fatal cases of myocardial ischaemia from the coronary care

unit of the Edinburgh Royal Infirmary. Of 100 cases examined, coronary artery occlusions were found in 88, with thrombosis forming at least part of the occluding lesion in 71. Examination of a large number of similar cases from many sources over the subsequent years indicates no significant change in these figures.—I am, etc.,

D. C. BOUCH

University Medical School,
Edinburgh

¹ Bouch, D. C., and Montgomery, G. L., *British Heart Journal*, 1970, **32**, 795.

Delayed Attacks of Malaria in Visitors to the Tropics

SIR,—I have a high regard for Professor L. J. Bruce-Chwatt, but I am puzzled by some of his comments (22 September, p. 637) on the article by Dr. Paul Horstmann (25 August, p. 440). In particular, I do not understand his references to insufficient dosage or his reasons for saying that the suppressive dose given to Dr. Horstmann's subjects "was manifestly insufficient." The suppressive dose, 300 mg of chloroquine base once weekly, was adequate to prevent any manifestation of malaria in 90 persons so long as it was being taken, and it seems to have been continued for about four weeks after the last probable exposure to infection. It is true that six of the 90 persons then developed attacks of vivax malaria after latent periods ranging from 7½ to 10 months, but surely this is, in Professor Bruce-Chwatt's own words, merely "a salutary reminder of the well-known fact that chemoprophylaxis with chloroquine must not be mistaken for causal prophylaxis (especially when it comes to *Plasmodium vivax*)."

My own point of view on this subject can be stated briefly.

(1) The suppression of malaria with chloroquine alone is incapable of eradicating all *P. vivax* infections, whatever the suppressive dose used. Provided that the parasites are not drug-resistant, the continuation of dosage for four weeks after the last exposure to infection should eliminate the risk of an attack of falciparum malaria, but it will not prevent the possibility of an attack of vivax malaria.

(2) If an attack of vivax malaria does occur, the time interval between its onset and the cessation of suppressive dosage will be determined largely by the characteristics of the infecting strain of *P. vivax*. The strains typified by the New Guinea Chesson strain have a pattern of short latent intervals, and with one of these the attack is likely to occur within a few weeks; with Korean and other—mainly temperature zone—strains long latent periods are common, and the attack may not occur for 6 to 14 months, though many will occur at about 10 months. Between these two extremes there are other strains mainly in tropical areas, with latency patterns that are irregular and less well defined.

(3) In practice, therefore, vivax malaria must be considered as a possible cause of fever in anyone who has visited a malarious area during the previous 14 months or so, especially if chloroquine or another 4-aminoquinoline was used as the sole suppressive drug.

(4) Though not so relevant to the present discussion, it is worth noting that a radical

cure with primaquine is usually more difficult to obtain with the New Guinea-type strains of *P. vivax*.—I am etc.,

T. WILSON

Portstewart,
Co. Londonderry,
N.Ireland

Edrophonium in Diagnosis of Cholinergic Crisis

SIR,—While I agree with Dr. D. L. McLellan (22 September, p. 634) that the giving of 10 mg of edrophonium intravenously to a patient in cholinergic crisis may lead to further grave complications, what I would like to stress is that once the stage is reached where the clinician is not certain whether the patient is in myasthenic or cholinergic crisis he is so desperately ill, and anoxic, only emergency measures may save him.

Frequently when this stage is reached only junior staff are available and in this department we therefore teach that when the question of adequate oxygenation is in doubt, further drug treatment should be abandoned and the patient immediately intubated and positive pressure ventilation instituted. This measure is undoubtedly life-saving and gives the clinician a chance to sit back and review the situation. Our practice is to ventilate these patients for 48 hours, withholding all cholinesterase inhibitors. The drugs are then gradually restarted, and as a rule within 24-48 hours the patient may be weaned off the ventilator. Needless to say trained staff and the appropriate facilities should be available at all times in units undertaking the treatment of these patients.

In my opinion the appreciation of this principle of intubation, aspiration, and ventilation in these desperately ill patients has been the most significant advance in the management of these unfortunate sufferers in recent years.—I am, etc.,

M. J. LANGE

Department of Endocrinology,
New End Hospital,
London N.W.3

Amyloidosis in Rheumatoid Disease

SIR,—I was interested in your leading article (29 September, p. 651) concerning extra-articular changes in rheumatoid disease, including generalized vasculitis, but I felt that the amyloid changes found at times in the arteries of those with severe or protracted disease merited further discussion. This occurrence has been recognized for many years in rheumatoid^{1,2} and related collagen disease. Heptinstall *et al*³ have reported its presence in granulomatous arteritis and my colleagues and I⁴ are now reporting a remarkable case of collagen disorder supervening after many years in a case of chronic granulocytopenia. In this case widespread amyloid change followed well-controlled treatment of the vasculitis with corticosteroids.

Widespread vasculitis may be regarded as the feature common to all collagen disorders and it is possible that the development of amyloid change in the vessels in any of them is chiefly due to protracted and severe disease. Of course it is in such cases that steroid therapy is now most likely to be given, and by prolonging life to give time for amyloid change to occur. Certainly it developed in my reported cases of rheuma-

toid disease in 1948, before steroid treatment was available.²—I am, etc.,

G. H. JENNINGS

Northchapel,
near Petworth, Sussex

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Estimation of Body Surface Area

SIR,—One can understand Dr. C. E. Blogg's (28 July, p. 235) difficulty in estimating body surface area (S.A.) in children from the formulae of Du Bois and Meeh. However, Costeff¹ has proposed a simple empirical formula for calculating approximate surface area in children based on a knowledge of body weight only. This formula is $S.A. (m^2) = (4W + 7) \div (W + 90)$ where W = weight (kg). Costeff claims that this formula is accurate over the range of weights between 1.5 and 100 kg. My clinical experience has shown this to be the case except in the presence of gross oedema, as in the nephrotic syndrome. The formula is extremely useful for estimating fluid requirements in the dehydrated child whose mother knows a recent weight.

The ideal method of estimating dosage in paediatrics is a moot point. Wood² states that there is general agreement that calculations of dosage based on body surface area provide the most reliable estimate of therapeutic dose. Nelson³ concurs in stating that many physiological phenomena are more closely related to body surface area than to age, height, or weight.—I am, etc.,

DENIS GILL

Department of Child Health,
Royal Children's Hospital,
Brisbane, Australia

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Psychiatric Nurse as Therapist

SIR,—We are glad that Dr. M. T. Haslam (1 September, p. 500) raised his misgivings about our article (21 July, p. 156) as they are probably shared by many people and it is useful to bring the issues out into the open.

We do not think that doctors need worry about ever being short of work as a result of nurses developing a more therapeutic role. The rise of physiotherapists, occupational therapists, social workers, and others has improved the quality of patient care but has hardly made doctors redundant. There is more than enough work to swamp everybody in the health professions for the foreseeable future. The demand for treatment far exceeds the supply, and at a time when many patients are being denied effective therapy for lack of therapists there is an urgent need to make treatment available to more people who can benefit from it.

Nurse therapists are not being trained to replace doctors—naturally, many years of medical training cannot be compressed into 18 months. The nurse-therapists have been trained to exercise specialized therapeutic skills for which prolonged medical training

would be largely redundant. Nor do we consider that a person with an academic training would wish to spend a substantial portion of his time on skills which do not represent a wide range of the potential for which his training fits him. The doctor would deploy his time more effectively as consultant to a team. Nurses have as strict an ethical code as doctors but require much less training than medical consultants or clinical psychologists.

Far more patients can be offered effective treatment by teams of this kind, the majority of whom are nurses, together with a psychiatrist and/or psychologist. The cost of treatment per patient is far less with such teams than where treatment is given mainly by the doctor or psychologist. The lower cost results from shorter training periods for nurses as much as from their lower pay. If Britain is to offer any hope of alleviating most of those neurotic disorders which can respond to newer psychological methods there is a pressing requirement not only for many more nurse-therapists, but also for more consultant psychiatrists who are qualified to supervise teams which include such therapists. It would be unfortunate and counterproductive if the ability of nurses to undertake therapy is taken to mean a need for fewer doctors. In fact the opposite is the case—if treatment services are to be extended, more doctors are needed to lead treatment teams, which cannot function without them, but of course proportionately more nurses than doctors will be required, as always. Adequate delivery of services requires careful development of workers of varying skills, some with shorter and others with longer training, requiring less or more supervision—but supervision, as Dr. M. J. C. Thompson (8 September, p. 545) rightly insists, that must be informed and interested. The nurse-therapists whose work we reported are shortly to carry their skills to other settings in which this point will be investigated.

Psychiatric nurse-therapists can play a valuable therapeutic role with psychotic as well as neurotic patients (as Dr. T. Lear points out (29 September, p. 69), but this field was outside the scope of our research, which was directed to those adult neurotic problems which can usually be treated successfully in fewer than 30 sessions. Such include many marital and family problems, which the nurse-therapists have dealt with skillfully after a year's training.

The numbers required to service the whole of Britain cannot be projected until detailed epidemiological and cost-effectiveness studies have been completed. With adequate personnel available, treatment of neurotic disorders in their acute phase might well reduce the prevalence of chronic problems, which form such a burden on the social services at present. Preventive psychiatry might then be more realistically attainable.—We are, etc.,

ISAAC M. MARKS
J. CONNOLLY
R. S. HALLAM

Maudsley Hospital,
London S.E.5

Suffocation in Epilepsy

SIR,—A year or two ago Dr. H. J. A. Longmore and I reported¹ the deaths of two of