service is not being fully utilized elsewhere, and the purpose of my letter is to draw the attention of your readers to this most valuable scheme, and to encourage fellow-surgeons to make full use of it.—I am, etc.,

M. F. BUTLER.

Isle of Thanet District Hospital,
Margate Wing,
St. Peter's Road,
Margate.

Hazards of Slimming Exercises

SIR,—A 21-year-old female student was concerned about her fat thighs, when she read about thigh-reduction exercises in a woman's magazine. These exercises, alleged to reduce bulk, consisted of repeated forcible rolling movements on to her flexed thighs.

After performing these she complained of a pain over the upper end of the right fibula and of altered sensation (which she described as numbness) on the lateral side of the same leg. This numbness persisted for three months, and when she consulted me after this period of time there was anaesthesia along the lateral side of her right leg, extending to the toes in a peroneal-nerve pattern. There was no motor loss. Sensation returned gradually without treatment, and when she was seen two months later was almost back to normal.

Perhaps would-be Twiggies should be aware of this hazard from a remedy of dubious value.—I am, etc.,

London W.C.1.

ERIC BLOOMFIELD.

Propranolol in Hypertension

SIR,—Dr. S. N. Tewari (3 August, p. 313) is kind enough to comment on our recent double-blind cross-over trial (8 June, p. 601) of propranolol in hypertensive Jamaicans, in which we found no difference between the effect of the drug and that of an inert placebo. Dr. Tewari's results form an addition to the lengthening list of uncontrolled trials which have appeared to demonstrate varying degrees of hypotensive effect. Indeed, our own uncontrolled run-in period, as we have stated, was associated with a small overall fall in blood pressure. Our work, however, remains as the only properly controlled trial of the drug in hypertension, and until another is published we cannot agree with the view that propranolol is a hypotensive agent of potency comparable to that of methyl dopa, guanethidine, or bethanidine.¹

Dr. Tewari suggests two reasons for the discrepancy. One is dosage; he has been using up to 400 mg. daily, and in most cases we used 360 mg. daily. At this very high dosage I cannot believe that a matter of 40 mg. can make the difference between success and failure. 360 mg. is in fact considerably in excess of what had been used in some apparently successful uncontrolled trials in the past. It is several times the therapeutic dose for other purposes, and at the time of planning the trial it was, to the best of our knowledge, the highest dose being given to ambulant hypertensives anywhere. In any case, after our trial was over we gave a number of the patients up to 480 mg. daily, and, failing to obtain any evidence of response, we abandoned the drug as a hypotensive agent.

His second reason is duration of therapy. Most, but not all, of the published uncontrolled trials have implied that propranolol takes considerable time to build up its effect, and Dr. Tewari takes this point of view. He may perfectly well be right. Certainly, in the light of present knowledge, we have suggested that future trials should employ longer cross-over periods than the two months each used by ourselves.

However, it seems to us more likely that the discrepancy is due to one or both of two other factors: firstly, that of race. It is obvious that people of different races may respond differently to therapy. We have suggested one possible mechanism (that is, that a mildly tranquillizing drug might be expected to have more effect on the blood pressure of a stress-ridden British businessman than on that of a happy-go-lucky Jamaican coconut gatherer), but there are many other possible ones, including, of course, as we have noted, that Jamaicans are not always punctilious about taking tablets on time. Secondly, controlled versus uncontrolled trials. This seems to us to be the crux of the matter at the moment. Until someone carries out a double-blind trial of the drug in European hypertensives I am afraid we must regard the verdict as "Not proven."—I am, etc.,

London E.C.4.

DAVID G. DELVIN.

REFERENCE

- ¹ Prichard, B. N. C., and Gillam, P. M. S., *Lancet*, 1966, 2, 1317.

Immediate Coronary Care

SIR,—There are several points in your leading article on immediate coronary care (20 July, p. 134) which require comment. You state that 60% of the deaths in the survey carried out in Belfast by Drs. R. H. McNeilly and J. Pemberton (20 July, p. 139) occurred within the first hour of infarction. In fact, they found that 60% of all the deaths from coronary artery disease occurred outside hospital. Of the deaths outside hospital 182 (18%) lived longer than two hours. It is for this group of patients that mobile intensive coronary care provides facilities for the early institution of intensive care. In these patients it might have resulted in the survival of 90 patients. This would have lessened the mortality by 9%.

Furthermore, you suggest that early admissions to hospital may overload existing hospital facilities. This is a policy of defeat. As Drs. McNeilly and Pemberton state, at present those admitted to hospital represent the survivors of a storm which has already taken its main toll. The function of intensive coronary care is to reduce this toll, and at present hospital intensive coronary care units are unable to come to grips with this problem. The expenditure of £19 for an ambulance call would seem a much more sensible and effective use of the available financial resources than some other expenses in the Health Service. Also, the cost of a mobile intensive care service organized on a large teaching hospital would be much less than £19 per call.

You also emphasize resuscitation facilities on the spot, including training of first-aid staff. The rationale of mobile intensive care is to provide early intensive care conditions

for prevention of primary ventricular fibrillation and thus negate the need for resuscitation in many cases. Resuscitation by first-aid personnel without provision of mobile coronary intensive care would not materially lessen the mortality of acute coronary thrombosis.—I am, etc.,

Cardiac Department,
Waveney Hospital,
Ballymena,
N. Ireland.

R. J. KERNOHAN.

** We regret the error pointed out in the first paragraph above.—Ed., *B.M.J.*

Ultrasound in Diagnosis

SIR,—The excellent article on this subject by Drs. R. B. Pridie and T. A. Turnbull (10 August, p. 356) will, it is hoped, bring this valuable diagnostic method to the notice of British cardiologists. Though Edler contributed a chapter to my book which fully described the procedure¹ and I have used it for an occasional case at this hospital for some years, I have been astonished at the lack of interest. Nearly always it is because the chest x-ray suggests effusion that I ask to be allowed to examine the case with ultrasound. This is the situation of echocardiography when I first described it in 1955, and eight years had to pass before the clinicians began to ask me instead of vice versa.

In the U.S.A. and Canada the method is already widely used, but in a recent lecture tour I was surprised to find that there it is usual to examine the space between the left atrial wall and the posterior pericardium instead of the anterior space. I always look in both places, but in my view the anterior is to be preferred. This may be due to the differences of equipment. The accurate calibration of pulses which is generally accepted in ophthalmic ultrasonography has not become routine in other fields. The authors state the pulse length is one microsecond, which at 2.25 megahertz would mean only 2½ cycles. I have never seen a pulse as short as this at any frequency. Four cycles is generally considered good, and many transducers produce pulses of eight or ten cycles.—I am, etc.,

DOUGLAS GORDON.

Willesden General Hospital,
London N.W.10.

REFERENCE

- ¹ *Ultrasound as a Diagnostic and Surgical Tool*, 1964, ed. D. Gordon. Edinburgh.

E.C.G. Changes in Amitriptyline Poisoning

SIR,—The recent paper by Dr. R. J. Barnes and others (27 July, p. 222) and letter from Dr. R. G. Brackenridge (10 August, p. 377) both suggest that there are specific E.C.G. changes to be found in patients suffering from amitriptyline poisoning. Dr. Brackenridge further suggests that there is a place for the routine estimation of aminotransferases following poisoning with this substance.

In our experience at the Poisoning Treatment Centre at the Royal Infirmary, Sunderland, Co. Durham, we have found both these investigations to be abnormal in patients suffering from many types of self-poisoning,

particularly Mandrax, and feel that it would be unwise to rely too heavily on results obtained from these tests to postulate a specific drug as having been used in the self-poisoning attempt.—I am, etc.,

Ipswich and East Suffolk Hospital,
Ipswich, Suffolk. G. R. BURSTON.

Voluntary Sterilization in the Male

SIR,—Formidable and remarkable is the assortment of disorders which your correspondent Dr. H. J. Roberts, of Palm Beach, Florida, attributes (17 August, p. 434) to the voluntary sterilization of young men.

The assortment includes grave diseases such as "severe narcolepsy" and "multiple sclerosis"; it includes common conditions such as "prolonged fever" and "recurrent infection"; and it includes such imprecisely defined disturbances as "various skin eruptions" and a "biological false-positive serologic reaction." Apart from hypoglycaemia, "the only common denominator was vasectomy." "By questioning every individual with such unexplained features," declares your correspondent, "a history of vasectomy prior to their onset has been uncovered." How many men, I wonder, suffering from "unexplained thrombophlebitis" and "increased circulatory gammaglobulin," to quote two more of the 13 conditions attributed by Dr. Roberts to a past vasectomy, did he see who had *not* been vasectomized? Do we not want to know how many before we incriminate the surgery?

Dr. Roberts's argument recalls those of many sincere opponents of new practices, such as vaccination and birth control. You believe them to be harmful. You may also believe them to be evil—it being easy to hold that if a practice is evil it must also be harmful. Your patients come to you with miscellaneous ailments. You question them about the suspected evil practice, and when acknowledgement is forthcoming you are easily convinced that the practice is the cause of the ailment. You then conclude, perhaps with more fervour than logic, that "the only common denominator" was . . . the evil practice (vasectomy, for example).

If, after vasectomy, seminal products are absorbed into the system, and if (as suggested by your correspondent) they cause adverse reactions, could not these reactions be identified with minimum risk of misinterpretation by carefully following up a sample of vasectomized men? Could not Dr. Roberts persuade the (American) Association for Voluntary Sterilization Inc. to undertake a follow-up? This they may have already done. The association has mediated the vasectomy of many men and has kept good records.—I am, etc.,

Shamley Green,
Surrey.

C. P. BLACKER.

SIR,—For Dr. H. J. Roberts (17 August, p. 434) to attribute the manifold ills he lists to vasectomy will strike many as quite fantastic.

He states that, before vasectomy, all these men had enjoyed "apparent" good health. How does he know that they were, in fact, in good health? Were all these elaborate investigations carried out before vasectomy (as well as after), and, if so, did they all prove negative? How can Dr. Roberts possibly assert that "the only common denominator" was vasectomy? And has he carried out a control series of similar investigations on men who have not undergone this operation? Such wild statements should not be made without scientific backing.

I may add that among the thousands of men known to have undergone vasectomy who have been in touch with us not one of these dire consequences has been reported. I suspect that Dr. Roberts has fallen into the vulgar error of confounding *post hoc* with *propter hoc*.—I am, etc.,

L. N. JACKSON,
Honorary Director,
Simon Population Trust
Voluntary Sterilization Project.
London S.W.1.

Wordsworth's Illness

SIR,—Anyone who studies the life of Wordsworth is aware of the disorders which oppressed him so grievously during the composition of his poems. In their fullest manifestation, between 1794 and 1807, they consisted of severe headaches followed by retrograde amnesia, pains in the stomach, the chest, the left side, and the arms. In 1805 there was, in addition, a symptom which gave rise to an obsessive dread of blindness. One cannot fail to notice the discrepancy between the physical robustness of Wordsworth and the symptoms which caused him so much direful suffering when he was writing his poetry.

Dr. A. J. Johnston, of Guildford, tells me that in his view the symptoms are almost certainly explicable as those of the particular type of migraine which is associated with intellectual stress or misgiving. No posthumous or contemporary diagnosis of the syndrome, so far as I am aware, has ever been published.—I am, etc.,

Bramley, Surrey.

C. E. VULLIAMY.

Records System for General Practice

SIR,—Dr. E. V. Kuenssberg's reply (27 July, p. 254) to our comments (15 June, p. 699) on his article (18 May, p. 420) shows so much misunderstanding that we are tempted to think either he did not read our letter, or at least he did not have it to hand when he wrote his.

He states that there is only $\frac{3}{4}$ in. (2 cm.) difference between his wallet and our proposed folder, yet the respective measurements are $7\frac{1}{2}$ in. $\times$ 5 in. (20 $\times$ 12.5 cm.), the wallet he describes, against $12\frac{1}{4}$ in. $\times$ $8\frac{3}{4}$ in. (31 $\times$ 22 cm.), the folder we describe. We chose the latter size because it allows the filing of international paper size A4, $11\frac{3}{4}$ in. $\times$ $8\frac{1}{4}$ in. (30 $\times$ 21 cm.) without folding. He insists that his wallet is suitable for international paper sizes, although size A4 cannot be filed in his wallet without folding or trimming. Dr. Kuenssberg suggests that if

we saw and handled one of his wallets we would find it was what we were looking for. He has, in fact, demonstrated the envelope to one of us personally, and we are not convinced. The last paragraph of his letter is very obscure. The letters and reports we intend to file in our folder are (obviously) those now crammed into the medical record envelope, and we certainly never suggested that we want to get rid of records that "contain the past of our patients." On the contrary our main purpose is to make the past history easily available.

We have taken a close interest in mechanical data handling experiments, and we are hoping to be associated with a record linkage study, but this does not alter our view that a folder of some design will be necessary for the foreseeable future. Nowhere did we say that a change in records should occur overnight; indeed, we think a new folder might be introduced gradually, starting with the fat record envelopes where orderly arrangement of the contents is of paramount importance, and Dr. Kuenssberg admits that the Scottish wallet is inadequate for this purpose. We were not criticizing the durability of the Scottish folder, merely the implication that it was a stopgap between the medical record envelope and mechanical methods of handling general practice records. Dr. Kuenssberg fears that the "perfect" record system could be expensive and useless; the word perfect did not appear in our letter.

Dr. Kuenssberg's first sentence, "Factual experiments never count for much with general practitioner theoreticians," is so silly that he probably regretted it when he saw it in print. The second sentence, "The doctors from Wantage are no exception," is astonishing, as he knows neither us nor our ideas except through our letter, which he failed to understand. We are putting a lot of thought and experiment into the planning of a practical trial of several folders, and we certainly welcome thoughtful discussion and criticism of our ideas, but Dr. Kuenssberg has provided none in his letter of 27 July.—We are, etc.,

I. S. L. LOUDON.
G. P. GREENHALGH.
J. K. HAWKEY.
G. T. BUNGAY.

Wantage, Berks.

Hypnotherapy

SIR,—It has been brought to the notice of this Society that courses in hypnotherapy are being offered to medical and dental practitioners by persons who hold no recognized professional qualifications. May we, through your columns, dissociate ourselves entirely with any lay organization or member of the public offering instruction to doctors or dentists?

The British Society of Medical and Dental Hypnosis holds a regular series of fully integrated courses in hypnotherapy in several major cities, and full details may be obtained by writing to the secretary, Mrs. B. Walton, 91 Booth Road, London N.W.9.—I am, etc.,

D. ZIMMERMAN,
Chairman of Council,
British Society of Medical and
Dental Hypnosis.

London W.1.