your attention to a press denial by the trustees that the centre is to close. This is a decision which still has to be discussed by the trustees in the light of the resignation of Dr. Shaldon, which is to take effect from 1 July 1968. Obviously, if a suitable replacement for Dr. Shaldon could be found within a reasonable period of time the centre could continue to function, but if perchance this is not possible inevitably and regretfully it would have to close.

On 9 May last a meeting took place with representatives of the Minister of Health to discuss a Ministry of Health circular, No. H/A 190/14, where the Minister advised chairmen of medical committees of teaching hospitals and senior administrative officers that, while regional hospital boards have the power to refer patients to the National Kidney Centre and pay for treatment outside the N.H.S., it was the unanimous recommendation of the meeting that patients should not be referred, as this form of treatment should be based only on hospitals with full supporting facilities. At this meeting a plan to provide chronic dialysis in the United Kingdom based on the use of home haemodialysis, which would integrate the National Kidney Centre within the National Health Service, thereby making all the medical equipment and financial resources available to the National Health Service, was submitted for consideration, and to date no reply to this plan has been received.

The attitude of the trustees of the National Kidney Centre and its medical director has always been an unselfish one, and the result of Dr. Shaldon's research into home dialysis was quite staggering in its potential in the terms of numbers which could be treated. We therefore feel that by integration with the N.H.S. the maximum number of people suffering from this disease in the United Kingdom could receive benefit.

Regretfully, it is the complete lack of interest and approach to this matter which has developed cumulative frustration in our medical director which now results in his resignation. I would have thought that from a national point of view there must be a strong case for the retention of the National Kidney Centre until such time as a better form of development could be substituted in its place. Should the train of events now result in the eventual closure of the National Kidney Centre it would be a national tragedy.

The result during the last seven weeks has already proved disastrous, as the programme of the centre was to accept 60 patients by the end of the year. During the past three weeks any patients referred have not been accepted. By the end of the year, if the situation continues, some 30 prospective patients will die needlessly.

The National Kidney Centre has been established as a charitable, non-profit-making organization to provide artificial kidney treatment to as many patients as possible. The centre is concerned with providing treatment under ideal circumstances—one unit, one patient, at home and freed from the need for repeated hospital attendance, involving travel and pressure on bed space and nursing staff in the already overburdened Health Service hospitals. Though the effectiveness of artificial kidney treatment for those with incurably diseased or damaged kidneys is medically accepted beyond dispute, the National Health Service will be unable to provide this form of treatment for more than

5 to 10% of those who could benefit in the next few years. This means that 5,000 to 10,000 sufferers in this country will die unnecessarily in that time.

The National Kidney Centre was set up by a group of individuals who were dissatisfied with the Health Service plan and who wished to demonstrate an alternative approach—home dialysis—which would, through private funds, provide hope for the majority who are doomed to die and for whom no Government help is foreseeable.—I am, etc.,

MURRAY MEDWAY.

London N.3.

. See leading article at p. 631.—Ed., *B.M.J.*

Amphetamine

SIR,—I have been most pleased to see in "Today's Drugs" on the prevention of motion sickness (12 August, p. 422) that you and your expert contributors say that any drug combination containing amphetamine is of itself open to objection. On turning overleaf (p. xii) I was dismayed to see an advertisement for an anti-obesity drug containing just such a combination.—I am, etc.,

Harrogate.

T. G. REAH.

The "Pill" and Deep-venous Thrombosis

SIR,—Mr. K. Hæger and colleagues (19 August, p. 493) query the reliability of diagnosis of venous thrombosis in the Royal College of General Practitioners series of non-fatal cases discussed in the Medical Research Council report (6 May, p. 355) and other reported non-fatal cases. Their remarks do not apply to any of the reported deaths, all of which have come to necropsy, as will have most of those studied by the Dunlop Committee. Even in the Royal College of General Practitioners cases, as the same practitioners were caring for both patients and controls, an increased incidence of *something* (pseudothrombosis?) has been found. Conversely, about 66% of deep-venous thromboses are not diagnosed in life¹ (my own observations agree) and it is as likely that the incidence is understated as overstated.

Interpreting broadly the term "coagulation patterns" in their last sentence, many studies have revealed a variety of changes in oral contraceptive users. To take one line of thought, your leading article (19 August, p. 449) says: "Platelet adhesiveness . . . has been shown to be increased in . . . venous thromboembolism." Puerperal venous thromboses are commonest about the tenth day, when both platelet count and platelet adhesiveness are maximal.² Platelet adhesiveness in the presence of cephalotogenic factor is increased in users.³

The platelet count in users is not increased.^{4,5} This was claimed as evidence against any thrombotic risk, but Dr. J. L. Burton (22 July, p. 214) suggests that agglomerins (notably fibrinogen) cause rouleaux formation and raised erythrocyte sedimentation rate in users. Very likely they play a part in thrombosis. If, as your leading article suggests, platelet adhesiveness is an

index of other blood changes it may in users connote a liability to thrombosis.—I am, etc.,

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Surgery in Acquired Cardiac Disease

SIR,—Perhaps I may draw attention to an error in the report on my communication on the place of surgery in acquired cardiac disease at the Annual Meeting in Bristol in July (22 July, p. 233). I am reported as saying that: "Patients with mitral incompetence tended to run a more rapid downhill course after the arrest of symptoms . . ." This should of course read "after the onset of symptoms . . ." The change in the word of course completely alters the sense of the message.

I was anxious to point out that, since operations for mitral incompetence are more complex and hazardous than mitral valvotomy, patients should not be advised to have open operations unless the symptoms due to mitral incompetence were demanding. Thus I stated that many patients with mitral incompetence were capable of managing satisfactorily without surgery for a number of years and in them operation could often be postponed.—I am, etc.,

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J. F. GOODWIN.

Protective Helmets and Traumatic Epilepsy

SIR,—I am very grateful for Mr. W. Bryan Jennett's helpful comments and information on traumatic epilepsy (29 July, p. 308). The following information may be of interest to him regarding his request for data on the incidence of head injuries in men wearing helmets.¹

During a period of six months in a coal mining area where 16,732 were employed 324 men voluntarily reported to the colliery medical treatment centres injuries which had occurred to that area of the head normally covered by the helmet. Of these, 299 accidents occurred underground and 34 on the surface. The wearing of helmets was virtually 100% in underground workers; it was the exception rather than the rule for those working on the surface. The distribution of accidents which occurred at the underground working place, according to the site of the head injury, was 60% frontal, 17% side, 8% back, and 16% top of the head. Surface workers had a different distribution—namely, 24% frontal, 9% side, and 68% top.

These figures demonstrate the efficiency of the helmet in protecting the top of the head. Unfortunately, the design and wearing fashion of the helmet did not give the same degree of protection to the front of the head in

underground workers. The helmet will only protect the head from injury if it is worn correctly and is designed to prevent those injuries which are foreseen. It must always be regarded as a second line of defence; accident prevention must be the primary aim.

Mr. Jennett is in error when he states that V. S. Caviness's conclusions² were based solely on a study of first world war gunshot wounds. In the statement I quoted from reference was made to the work of W. F. Caveness,³ who investigated 356 young adult males who sustained head injuries in the Korean war campaign. Of these, 159 had non-missile head injuries. The report deals with the occurrence of epileptic attacks in the first 8 to 11 years following head injuries. W. F. Caveness stated, "While mode of injury is an important factor in the onset of fits, there is no significant difference between missile- and non-missile-injured men as regards the cessation of fits. A common clinical impression has been that the earlier the fits take place the less likely are they to persist. This has been held especially for those fits that occur within a few hours or days after injury. Conversely, it has been thought that those attacks that begin after one year seldom cease. In the current series neither is borne out."

Because Mr. Jennett's investigations⁴ were of civilian head injury cases, his views are of great importance to those assessing prognosis, and especially his statement, "Early epilepsy (within a week of injury) has a 75% chance of permanent remission, but epilepsy developing later (whether or not there has been early epilepsy) persisted in more than 80% of 200 patients followed up for more than two years after their first fit."

Knowledge that seizures may cease can be an important factor when resettling patients. Unfortunately, to many managers the word "fit" is synonymous with unemployability. The possibility that seizures can cease may prevail on them to find suitable work, especially if the injury was the result of a works accident. Reduction of status and earnings are a deterrent to a proud craftsman from accepting the usual only available employment—for example, labouring. Acceptance of this work is made more likely when management undertake to upgrade a patient to his old skills if seizures cease permanently.—I am, etc.,

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Recognizing Early Myxoedema

SIR,—The facial appearances of patients with well-marked myxoedema have long been familiar to clinicians, but the earliest manifestations of this are not easy to recognize.

Considering this problem recently, I realized that a personal method of assessment that I have long used might be of value to young general practitioners. (It is of no value to those seeing such patients for the first time.) It is, quite simply, the fact that on seeing such a patient my first thought has been: "Mrs. So-and-so is losing her looks."

This is most obvious in the 45–55 age group.

The woman whose face is getting plumper does not provide this thought, nor do the tragic sufferers from depression.—I am, etc.,

Leeds.

A. U. MACKINNON.

Investigating Hypertension

SIR,—My letter (29 July, p. 306) was intended primarily to offer critical comment on your leading article (1 July, p. 4) which I had found not particularly helpful or informative. In that article you chose to quote Chamberlain and Gleeson's paper,¹ and I offered certain criticisms of their paper to which they have now replied (19 August, p. 495). Although they state that they "would not wish to advocate diagnostic nihilism but merely recognition of the limitations and potential dangers of a particular diagnostic technique," their comments are apt to be taken up and used as evidence against the value of aortography—as in your leading article and in recent correspondence elsewhere.² In my view their paper had certain limitations which should be recognized.

I share their view that the outcome of surgical treatment of hypertension may be unpredictable or disappointing—but I repeat the comment that the value of aortography should not be judged on this alone. If the procedure adds to the diagnostic data and enables one to exclude certain disorders (for example, fibromuscular dysplasia) and assists in obtaining a likely explanation for the hypertension (for example, "essential hypertension" with renal arterial anomalies) then there has been a useful gain. Further, I believe that medical management may be influenced in a number of ways by the aortographic findings—if only in providing a once-in-a-lifetime survey of the renal and intra-abdominal vascular tree and relieving the physician of the misgiving that he may be overlooking some important disorder. I have seen the sad consequences of inadequate investigation too often to feel confident in advising my patients after incomplete assessment.

Drs. Chamberlain and Gleeson chose to quote only our first and smaller series³ in an implication that our series was somewhat unrepresentative (with a 9-to-1 male to female ratio, and two-thirds of the patients being Service personnel). The second paper which they mentioned⁴ but did not quote in this context studies almost three times as many patients (341 as against 121), and 1 in 4 of the patients was female. As a matter of fact over the eight years which our unit has been working the proportion of Service to civilian patients is almost exactly 50%.

Their own work presented an interesting analysis of a retrospective study on a relatively small number of aortograms. We were not told of the period of time from which these results were collected, nor from how many hypertensive patients their 107 cases were selected—nor indeed how they were selected. Presumably since their text states "records of all aortograms were available and through these a retrospective study . . . was possible," the figure of 107 represents all patients subjected to angiography for hypertension at the Westminster Hospital since angiography was first introduced. It would be unusual if there had not been many hundreds, if not thousands, of hypertensive patients attending for advice, investigation, and

treatment over this period—so my comment that their series was highly selected must stand.

Whether our own papers described a selected series of patients is not relevant to the discussion on angiography, though it might be to any discussion on hypertension. Any series is in fact initially selected by geographical, environmental, and other features—but we did describe the findings from a large number of unselected angiograms (since all patients under investigation for hypertension were angiographed) in a prospective study. This seems to me a much more acceptable method of assessing the value of the procedure.

If Dr. Chamberlain and Dr. Gleeson would care to visit our unit at any time they will be most welcome, and we would hope to show them that our work is not founded on "uncritical . . . aortography in the hope that this will provide a diagnostic answer that it is incapable of doing." Further, they would see that a large amount of evidence has accumulated over the past seven or eight years in support of our contention that renal dysplasia, often familial, is associated with much clinical disorder, including hypertension. Papers on these and other aspects of the work are in an advanced state of preparation.—I am, etc.,

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Progestin Therapy of Breast Cancer

SIR,—Dr. B. A. Stoll (5 August, p. 338) has to be congratulated for his very worthwhile attempt at screening the efficacy of various progestational preparations in the treatment of advanced breast cancer. The paucity of satisfactory responses in his series of seven compounds tested should not discourage the search for more effective agents. The smallest change in the chemical structure of these steroids can markedly alter their clinical efficacy. For instance, the negative impression gained in the earliest trial in this country with norethisterone oenanthate was dispelled by a very similar compound having a shorter side chain and giving improved clinical results. Similarly the addition of 5 mg. oestradiol benzoate to progesterone 50 mg. by daily injection proved successful in 7 of the 15 cases with breast cancer treated by Landau *et al.*¹

In this department further experience was gained from the combination of progesterone (19-nor-17 α -hydroxyprogesterone caproate) 100 mg., with oestradiol valerate, 30 mg. Twenty-five patients (four premenopausal) with advanced cancer of the breast in whom previous surgery, radiotherapy, oestrogens, and androgens have either failed to arrest the progress of the disease or ceased to be effective were treated with this compound for periods of one to 14 months, receiving intramuscular injections weekly, later increased to twice weekly. In 8 cases (32%) a moderate to good response was noted with regression of visible skin and glandular neoplastic infiltration, disappearance of oedema, and amelioration of general conditions. In