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CORRESPONDENCE

Correspondents are urged to write briefly so that readers may be offered as wide a selection of letters as possible. So many are being received that the omission of some is inevitable. Letters should be signed personally by all their authors.

Oral anticoagulants

SIR,—I enjoyed reading Professor A Breckenridge's authoritative article (21 February, p 419) on the above subject in which he underlines the need for a pharmacological basis in drug therapy. He stresses that a therapeutic trial should be properly designed and notes that in the case of oral anticoagulants used in arterial disease the "better" the trial—that is, the nearer it fulfilled the seven criteria considered necessary—the worse oral anticoagulants fared. It is also interesting that none of the 59 studies of these drugs in arterial disease was considered perfectly designed. All scientists and statisticians and most clinicians will have no quarrel with this, but the implication that therefore oral anticoagulants are useless in arterial disease is, I think, fallacious.

Firstly, the necessity that successful therapy must have a rational basis is in question. Carbamazepine for trigeminal neuralgia, amantadine for Parkinsonism, and electric convulsion therapy for severe depression are a few examples accepted (even if occasionally misused) by most experienced clinicians as useful forms of treatment based on serendipity rather than scientific deduction.

Secondly, the premise that the failure of a drug to show significant advantages in a perfectly designed clinical trial indicates its uselessness is also unacceptable to many experienced and open-minded doctors. Often the drug will have shown some statistically insignificant advantages in the nearly perfect trials but significant advantages in the less perfect, and the question must be asked what is the reason for this difference. I think that the imperfections of some trials (and I include my own in these) stems from an author's intuitive selection of patients to enter the trial. This is, of course, anathema to the scientist but is the cornerstone in deciding treatment in the individual patient, as indications and contraindications can be worked out not, as the statistician would say, blindly but on the basis of experience and a careful weighing-up of many factors. My own particular interest has been in occlusive cerebrovascular disease, and although I am more selective than formerly I still think oral anticoagulants are useful in some patients, mostly those with transient cerebral ischaemic attacks or with progressing hindbrain ischaemic infarction.

Thirdly, the scientific premise that if a drug is unable to alter the function of the main causative agent of a disease it is therefore ineffective in that disease is also sometimes questionable. Professor Breckenridge rightly points out that platelets play a fundamental role in the genesis of arterial thrombosis and are unaffected in vitro by oral anticoagulants. However, the secondary clot which is the "red tail" is often seen in the carotid artery at operation attached to the platelet aggregation or "white head" of the thrombus and consists of clotted blood in fibrin. Many of us believe that this clot is associated with some manifestations of occlusive cerebrovascular disease, and we know that its appearance can be modified, as in venous thrombosis, by oral anticoagulants. Further evidence of this lies in the disappearance of the carotid murmur in some patients with transient cerebral ischaemic attacks when they are treated with oral anticoagulants.

It is for these reasons that I think it a mistake to suggest that oral anticoagulants are entirely useless in arterial disease, although I have modified my own original enthusiasm considerably, especially as I found that my original success with these drugs in cerebral infarction was not apparently due to any modification of the arterial thrombosis but to their influence on the occurrence of pulmonary embolism associated with venous thrombosis in the legs. This is certainly a point for the scientist, but fortunately the clinician can sometimes be right for the wrong reasons.

A BARHAM CARTER

Weybridge, Surrey

Assessment of preoperative cases

SIR,—I should like to comment on the article by Dr T W Ogg (10 January, p 82). Ideally, every patient on whom routine surgery is contemplated should be fully assessed from the point of view of fitness for anaesthesia well before his or her proposed date of admission. As this is prevented by a shortage of resources the problem is one of selection of patients for day-case surgery. In this connection I was somewhat disturbed that the ages of Dr Ogg's patients ranged up to 80 years and disappointed that he gave no details of age-related incidence of significant positive responses.

In my experience the prevalence of medical