per se a benign condition, Dressler himself described the main complication of post-myocardial infarction syndrome as being cardiac tamponade in patients who had been prescribed anticoagulants, this being the cause of death in one member of his original series.[5] It also appears that prior to Dressler's original paper there were numerous reports in the literature describing haemopericardium (often fatal) in postinfarction patients on anticoagulants. Many of these appear in retrospect to have had features of the then unrecognised postmyocardial infarction syndrome.[6]

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Assessment of fitness for surgical procedures

SIR,—Despite Dr Alex Paton's re-writing (23 February, p 529) of the paper by Dr M E Wilson and others (p 509), it is still difficult to understand. Perhaps the message is not so simple—and if it is it could be stated more succinctly in the discussion. Great ideas have been expressed in the form and in particular the length of a letter, as did Watson and Crick when putting forward their theory of DNA. "Writing makyth the exact man." I suggest that the paper should have been written by one anaesthetist and read by sceptical surgeons: should they understand it, all well and good. Many, however, would consider that pertinent variables have been overlooked. The skill of the surgeon, the morale of the patient, and the experience and technique of the anaesthetist are all very relevant, as is the presence in the ward of adequately trained nurses during the pre-operative and postoperative phases.

Surgeons select the more prolonged and difficult operations for the experienced anaesthetist, with whom they may have worked for many years, while the more straightforward procedures may be delegated to trainees. Anaesthetists learn the peccadillos and varying skills of surgeons, but if they are genuinely convinced that the operation is really necessary then they will anaesthetise the gravely ill: the girl deeply shocked and unconscious from her ruptured ectopic pregnancy needs concomitant resuscitation and laparotomy, when the skilled anaesthetist welcomes his responsibility.

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SIR,—The excellent and original paper on this subject by Mr M E Wilson and others (23 February, p 509), so ably subedited by Dr A Paton, is an important contribution to medical cost-effectiveness. They show that a simple questionnaire can accurately sort out a large proportion of obviously fit people prior to operation. Nevertheless, at present, area health authorities all over the country (particularly those with a (T) added to their name) usually employ expensive young anaesthetists, and they are expensive, to do this work for them. The implications are obvious.

Unfortunately, during its many vicissitudes prior to publication, what the paper has gained in concision it has seemed to lose in clarity. The final and most important statement in the summary is as follows: "A simple questionnaire predicted fitness for operation by all 10 anaesthetists in 96% of cases." Happily, of course, anaesthetists do not do operations—let alone in teams of 10. The real conclusion of the paper is, in fact, as follows. Out of the 200 routine surgical cases studied, the consensus opinion of 10 anaesthetists was that 149 of them were fit for surgery (fitness category 1 or 2). This conclusion was arrived at after they had studied the answers to a simple 10-point questionnaire. However, after further study of the case notes and results of special investigations the consensus opinion of the 10 anaesthetists was that nine of the patients should be downgraded to fitness category 3.

How important was this? The question still remains unanswered, but that it can be asked is perhaps a sign that we are at last on the road to a more rational use of scarce resources.

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Randomised controlled trials and retrospective controls

SIR,—The methodological problem addressed by Sir Richard Doll and Mr R Peto in their contribution (5 January, p 44) to the debate on my article (17 November, 1979, p 1265) is that of measuring small (20%) differences in the effectiveness of alternative treatments of common disease. This is a problem quite different from any discussed in my article, where the issue is the "inherent fallaciousness" of retrospective controls—a doctrine conspicuous in an influential part of the literature for several decades and not explicitly questioned. My analysis dealt with that issue in the contexts of rare disease, common disease with a wide range of manifestations (cancer), and the early stages of clinical investigation generally, but not at all with the problem addressed by Sir Richard and Mr Peto.

In view of that fact, it is surprising that they assert that I have "encouraged" retrospective controls "*at the expense*" (their italics) of randomised concurrent controls, as though the two were mutually exclusive. Reasons why they should be considered complementary, even where the large, randomised trial is indicated, are given below.

(1) *Pretrial role*—To determine that the expense and labour of a large randomised trial are justified, smaller trials are necessary to establish the bounds of the results to be expected and to explore the pitfalls of large trials. This is a phase to which I particularly referred in connection with the use of retrospective controls.

(2) *Implementing the finding*—Once a randomised trial has been carried out and the finding of a small but statistically significant improvement has been publicised, one must look to its impact on practice. Since the margin of improvement is small it is certain, on statistical grounds alone, that in many instances experience will seemingly contradict the result of the large randomised trial. More serious is the possibility that a trial which has randomised physicians may have washed out real benefits which individual physicians with great experience in diagnosis and in the administration of an alternative treatment may be able to provide for their patients. The dilemma is one which has to be resolved by the individual physician. Clearly, the judgment of the physician in evaluating his own experience and in weighing it against the verdict of the trial may be seriously biased by factors which have been frequently enumerated as defects in the use of historical methods. If he does not alter his mode of treatment, however, he has been challenged to hone his skills of objective analysis in the evaluation of the histories of his own cases—a clearly complementary aspect of randomised and historical technique.

(3) *Post-trial analysis and final assessment*—So-called "retrospective stratification" is itself a recognised tool of analysis of the results of randomised trials. And if there is ever to be a "final" assessment of the worth of a given randomised trial and of the benefits realised from application of its results to practice, how is it going to be arrived at except by historical analysis?

A final comment is in order on the application of computers to the storage, retrieval, and objective analysis of case histories. Clearly, a computer provides a means of performing these tasks on a hitherto unprecedented scale with programs of every-increasing sophistication. A local computer would enable an individual physician to press a few buttons and have before him the records of all his past cases which match most closely that of the current patient. A terminal connected to a central computer could supplement that information with the case histories compiled by colleagues. In research trials the matching of individuals for assignment to experimental and control groups by computer would seem to provide important advantages over the simple flipping of a coin.

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**This correspondence is now closed.—ED, *BMJ*.

The editor regrets . . .

SIR,—You observe in the opening line of your leading article (23 February, p 508) that rejection of a paper for publication can make an author angry, frustrated, or miserable. I would say "paranoid" as well. What I find so disheartening is that after spending months on a paper it can be rejected, sometimes months later, without any reason being given. In some cases a further approach to the editor still does not extract any clues about the reason for rejection. It may well have been the case that my papers were non-starters. The difficulty was not that I could see no reason for the papers to be rejected but that there were several possibilities. For this reason I have given up trying to write full-length papers.

Why cannot editors of medical journals conform to the principles laid down by the *Journal of Neuropathology and Experimental Neurology*? Referees have to report back within two weeks according to six criteria; if the editor rejects the paper he will give reasons, often by quoting the referees. This is all the more essential for those authors who do not have colleagues to criticise or evaluate their papers.

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**Our justification for giving no reasons for rejecting most articles is simple; with 4500 rejects a year we cannot find time to write detailed appraisals on every occasion. When, however, authors ask for specific reasons we can usually provide them.—ED, *BMJ*.