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CORRESPONDENCE

We may return unduly long letters to the author for shortening so that we can offer readers as wide a selection as possible. We receive so many letters each week that we have to omit some of them. Letters must be signed personally by all their authors. We cannot acknowledge their receipt unless a stamped addressed envelope or an international reply coupon is enclosed.

Preventing thromboembolic complications in high-risk surgical patients

SIR,—With reference to the paper "Failure of low-dose heparin to prevent significant thromboembolic complications in high-risk surgical patients" by the Groote Schuur Hospital Thromboembolus Study Group (2 June, p 1447) I would like to make one point.

The authors point out that it is possible to overlook the presence of proximal segment thrombosis if one relies on ^{125}I-fibrinogen uptake alone. Nor did Doppler ultrasound identify all cases of proximal thrombosis. These were the only methods used for detecting preoperative deep vein thrombosis, whereas the only effective method of detecting venous thrombosis—bilateral ascending phlebography—was understandably used only for postoperative detection.

It is a common clinical observation that not only patients waiting for operation but several other patients with diseases who would easily come under the category of patients undergoing "major elective abdominal surgery," as in this trial, would be susceptible to the development of thromboembolic phenomena without undergoing operation.

Established proximal segment thrombosis in the preoperative phase would probably predispose to postoperative proximal segment thrombosis and non-fatal (or even fatal) pulmonary embolism even when the patient was subjected to a "prophylactic" heparin regimen.

The presence of preoperative proximal segment thrombosis in this series is unknown because, understandably, preoperative phlebography was not performed. I conclude that the deduction that "the routine use of low-dose heparin prophylaxis in all major surgical procedures in patients over 40 years may be premature on available evidence" is not valid.

It may be that prophylactic low-dose heparin should be administered to all patients who are to undergo major elective surgery immediately on admission—for example, while investigations are being carried out—rather than only two hours preoperatively. Even this may not help patients who have been ill for a time before admission and who may have already developed proximal segment thrombosis.

M ADISESHIAH

Addenbrooke's Hospital,
Cambridge CB2 2QQ

SIR,—Having read with interest the interim report of the Groote Schuur Hospital Thromboembolus Study Group (2 June, p 1447) I should like to make some observations which lend support to the very trial which this group seeks to question—namely, the International Multicentre Trial reported initially in the *Lancet* in 1975.[1]

The study group's report confirms the now-established finding that low-dose heparin substantially reduces the incidence of deep venous thrombosis (DVT). The group suggests that this reduction is achieved solely by reducing the incidence of calf-vein DVT, yet it finds no concomitant reduction in the incidence of pulmonary embolism. This leads one to the conclusion that calf DVT is of no consequence in the development of pulmonary embolism. However, if one looks at the numbers of the group's patients diagnosed as having pulmonary embolism one finds that this exceeds the number who were shown to have DVT proximal to the calf veins. From this one draws the conclusion that the calf vein thrombosis has indeed been the source of some of the pulmonary emboli. These two conflicting conclusions lead one to an inevitable third—something is wrong.

Close scrutiny shows a number of sources of error. I picked the following. Firstly, no mention is made of any treatment started in cases of diagnosed DVT. If anticoagulants were used one would not expect a significant difference in the numbers proceeding to pulmonary embolism, as the control and heparin groups would have merged. If anticoagulants were not used this too would affect the outcome. A difference in the number of patients with DVTs progressing to pulmonary embolism would be expected only if low-dose heparin were appropriate treatment for established DVT. This apparent no-win situation leads us to the realisation that what we must study is what was looked at in the International Multicentre Trial—the rate of fatal pulmonary embolism. What we are to profit by examining the occurrence of non-fatal pulmonary embolism by the seventh postoperative day I do not know. Finally, the accuracy in diagnosing pulmonary embolism by means of perfusion lung scans, even in the manner described, has been called into serious question by the work of Bell and Simon.[2] They compared independent reports of perfusion scans with those of pulmonary angiograms. Their results indicated that the interpretation of the scans, even by experts, was, to say the least, difficult and correlated poorly with the findings of pulmonary angiography.

Having made these observations on the Groote Schuur Hospital Study, I do not see that this work can at all throw doubt on the unequivocal findings of the International Multicentre Trial.

R S STUBBS

Department of Surgery,
Hammersmith Hospital,
London W12 0HS

[1] International Multicentre Trial, *Lancet*, 1975, **2**, 45.
[2] Bell, W R, and Simon, T L, *American Heart Journal*, 1976, **92**, 700.