CORRESPONDENCE

We welcome letters to the Editor concerning articles which have recently been published. Such letters will be subject to the usual stages of selection and editing; where appropriate the authors of the original article will be offered the opportunity to reply.

Letters should normally be under 500 words in length, double-spaced throughout, signed by all authors and fully referenced. The edited version will be returned for approval before publication.

DEATH AND THROMBOEMBOLIC DISEASE

Sir,

The article by Warwick et al in the January 1995 issue entitled 'Death and thromboembolic disease after total hip replacement' (1995;77-B:6-10) is a significant contribution to the debate on the prevention of mortality and morbidity. We have, however, reservations regarding their choice of subjects, results and conclusions.

First, the sample of patients studied was diverse with differing risk factors and preventative measures for thromboembolism. For example, there were 112 patients undergoing revision surgery. These are known to be more likely to suffer postoperative complications including pulmonary embolism (PE) (Hunter et al 1979; Kavanagh, Ilstrup and Fitzgerald 1985; Engelbrecht et al 1990). Of the 17 deaths, two occurred in the revision patients (a rate of 1.79%) and the remaining 15 were in the primary replacement group (15/1000, a rate of 1.5%); the difference in death rates is statistically significant ($p < 0.05$). Other patients in the sample were also more likely to suffer deep-vein thrombosis (DVT) or PE. Of the 1162 patients, 10.6% were designated "at risk" and were prophylactically anticoagulated. Neither the method of anticoagulation nor the risk factors were stated. Furthermore, 18 patients had simultaneous bilateral THR and were therefore also at increased risk.

Secondly, regarding the results, we note that five of the 17 patients who died did not have a postmortem. Of these five, in two the cause of death is stated as bronchopneumonia, which in its terminal phase is clinically similar to PE. If the cause of death was PE, the number of deaths attributable to PE would have risen from 4/17 (23.5%) to 6/17 (35.3%). This is again a significant difference ($p < 0.05$). Furthermore, five patients were anticoagulated for DVT without venographic proof. If all five had had a positive venogram, the overall DVT rate would rise from 22 (1.89%) to 27 (2.32%), an increase of 23%.

Finally, regarding the conclusions, the authors note that widely differing rates of DVT after THR are found depending on the method of screening used (clinical or venographic). They also remind us that no study has so far demonstrated the ability of chemical agents to reduce the rate of fatal PE after THR. In their conclusion, however, they state that clinically or radiographically proven DVT is a good marker for fatal PE. We find this conclusion incompatible with the statements quoted above.

No doubt debate will continue concerning effective methods of protecting patients from fatal PE after THR. We agree with Warwick et al that large prospective randomised trials are needed to evaluate both chemical and mechanical means of thromboembolic prophylaxis.

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Plymouth, UK.

Engelbrecht DJ, Weber FA, Sweet MBE, Jakim I. Long term results of revision total hip arthroplasty. *J Bone Joint Surg [Br]* 1990;72-B:41-5.

Hunter GA, Welsh RP, Cameron HU, Bailey WH. The results of revision of total hip arthroplasty. *J Bone Joint Surg [Br]* 1979;61-B:419-21.

Kavanagh BF, Ilstrup DM, Fitzgerald RH. Revision total hip arthroplasty. *J Bone Joint Surg [Am]* 1985;67-A:517-26.

Warwick D, Williams MH, Bannister GC. Death and thromboembolic disease after total hip replacement: a series of 1162 cases with no routine chemical prophylaxis. *J Bone Joint Surg [Br]* 1995;77-B:6-10.

Author's reply:

Sir,

Thank you for allowing me to reply to Dr Murphy and Mr Ricketts. I would like to take their points in turn.

There are many risk factors for thromboembolism acting to a varying degree (age, weight, operating time, postoperative mobility, etc) and false conclusions may be drawn if just one is taken in isolation. I disagree with their conclusion that there were more deaths in the revision group than in the primary group. First, the cause of death in the two revision patients was myocardial infarction (postmortem) and cerebrovascular accident (death certificate), respectively. Secondly, the difference between 2/112 revision patients and 15/1032 primary patients is not significant (difference in proportions 0.36%, 95% CI -0.29 to +2.2%, chi-squared test on 1 df 0.07, $p = 0.78$). Similarly, we found no increased risk in those with simultaneous bilateral procedures.

We cannot from our study quantify the risk factors for those who had anticoagulation, since the interpretation of risk was a clinical judgement by the surgeon concerned and, in the absence of a protocol, would necessarily have been subjective.

The comment about the similarity between bronchopneumonia and PE is important. One patient died on day 21 in the intensive care unit after proven bacterial joint sepsis and bronchopneumonia. This was not consistent with an acute massive PE as a primary cause of death against which prophylaxis would be aimed. The death of the second patient on day 60, although clinically not typical of a PE, cannot confidently be attributed to bronchopneumonia. The statement of Dr Murphy and Mr Ricketts that an increase from 4/17 to 6/17 is significant at $p = 0.05$ is an interesting application of a statistical method (and it would be intriguing to know which test was used to derive this probability value).

Anticoagulation without venographic proof should be undertaken with caution. Because of its low specificity, the clinical diagnosis of deep-vein thrombosis (DVT) is not enough to substantiate the diagnosis.

Having reviewed the conclusions of our paper, I cannot find the

statement that "clinically or radiographically proven DVT is a good marker for fatal PE". We observed that reliance on the indirect marker of DVT is more feasible than fatal PE from the viewpoint of sample size, but no comment was made on the appropriateness of so doing. The oft-assumed relationship between the *venographic DVT* after hip replacement and the *clinical expression* of thromboembolism (such as thrombophlebitis, venous occlusion, chronic venous insufficiency, fatal or non-fatal pulmonary embolism) needs further study.

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BONE CEMENT AND DEEP-VEIN THROMBOSIS

Sir,

In his reply to a letter in the July 1995 issue (1995;77-B:668) Warwick mentions the possibility that methylmethacrylate cement may be thrombogenic and questions the existence of random studies to determine this. During a study comparing the value of dextran as opposed to antithrombin III plus heparin in the prevention of deep-vein thrombosis (DVT) in total hip replacement (THR), Francis, Marder and Evarts (1986) encountered DVT in 4 out of 13 cemented prostheses as opposed to 0 out of 23 uncemented prostheses.

In 1988 we published the results of a trial involving 228 patients with THR in which the incidence of proximal or distal DVT did not differ significantly between cemented and uncemented prostheses (Planes et al 1988). Other authors reported similar findings (Hull et al 1990; Eriksson et al 1991).

Levine et al (1991) included the use of cement or not in the stratification of their trial of 665 patients as did Francis et al (1992) in their series. Again there was no significant difference in the incidence of DVT in cemented or uncemented prostheses.

Since then other large trials have not distinguished between the type of fixation, suggesting that this factor has no influence on the incidence of DVT in THR (Hull et al 1993; Colwell et al 1994; RD Heparin Arthroplasty Group 1994; Spiro et al 1994).

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N. VOCHELLE, MD
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La Rochelle, France.

Colwell CLW Jr, Spiro TE, Trowbridge AA, et al. Use of enoxaparin, a low-molecular-weight heparin, and unfractionated heparin for the prevention of deep venous thrombosis after elective hip replacement: a clinical trial comparing efficacy and safety. *J Bone Joint Surg [Am]* 1994;76-A:3-14.

Eriksson BI, Kalebo P, Anthmyr BA, et al. Prevention of deep-vein thrombosis and pulmonary embolism after total hip replacement: a comparison of low-molecular-weight heparin and unfractionated heparin. *J Bone Joint Surg [Am]* 1991;73-A:484-93.

Francis CW, Marder VJ, Evarts CM. Lower risk of thrombo-embolic disease after total hip replacement with non-cemented than with cemented prostheses. *Lancet* 1986;I:769-71.

Francis CW, Pellegrini VD, Marder VJ, et al. Comparison of warfarin and external pneumatic compression in prevention of venous thrombosis after total hip replacement. *JAMA* 1992;267:2911-5.

Hull RD, Raskob GE, Gent M, et al. Effectiveness of intermittent pneumatic leg compression for preventing deep vein thrombosis after total hip replacement. *JAMA* 1990;263:2313-7.

Hull R, Raskob G, Pineo G, et al. A comparison of subcutaneous low-molecular-weight heparin with warfarin sodium for prophylaxis against deep-vein thrombosis after hip or knee replacement. *New Engl J Med* 1993;329:1370-6.

Levine MN, Hirsh J, Gent M, et al. Prevention of deep vein thrombosis after elective hip surgery: a randomized trial comparing low molecular weight heparin with standard unfractionated heparin. *Ann Intern Med* 1991;114:545-51.

Planes A, Vochelle N, Mazas F, et al. Prevention of post-operative venous thrombosis: a randomized trial comparing unfractionated heparin with low molecular weight heparin in patients undergoing total hip replacement. *Thromb Haemost* 1988;60:407-10.

RD Heparin Arthroplasty Group. RD heparin compared with warfarin for prevention of venous thromboembolic disease following total hip and knee arthroplasty. *J Bone Joint Surg [Am]* 1994;76-A:1174-85.

Spiro TE, Johnson GJ, Christie, et al. Efficacy and safety of enoxaparin to prevent deep venous thrombosis after hip replacement surgery. *Ann Intern Med* 1994;121:81-9.

Warwick DJ. Author's reply. *J Bone Joint Surg [Br]* 1995;77-B:668.

Author's reply:

Sir,

The review of the problem of bone cement by Dr Planes and Dr Vochelle uses stratification rather than randomisation for evidence. Nevertheless, it reassures us that bone cement does not appear to be an important clinical risk factor in thrombosis, despite the theoretical risks of more prolonged femoral vein torsion and methylmethacrylate toxicity to the femoral vein endothelium.

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CYCLICAL MICROMOVEMENT AND FRACTURE HEALING

Sir,

I was interested to read the paper in the July 1995 issue entitled 'Cyclical micromovement and fracture healing' by Noordeen et al (1995;77-B:645-8).

The authors referred to our experimental work and indicated correctly that their regime for micromovement was different from that which we used. If they could clarify the actual movements which occurred on the fixators in their clinical study, these, together with an indication of the ground reaction force, would enable us to know whether micromovement did occur on the fixators and whether the regime was similar to that used in previous studies. The use of measurement of fracture stiffness to monitor healing is to be encouraged. I would like to know how the authors assessed this in the early stages of healing.

We have reported the use of a number of different mechanical regimes in relation to short periods of cyclical micromovement applied to healing osteotomies and have shown that some are inhibitory or may delay indirect fracture repair. Our studies have also shown that variations in mechanical characteristics such as the magnitude of force and the amount and rate of displacement can have inhibitory or enhancing effects on fracture repair. The geometry of the frame in relation to the force applied to the ground by the patient will also affect displacement at the fracture site. As presented, the paper only allows readers to make limited deductions and the work would be considerably enhanced by inclusion of data on ground reaction force and displacement of the fixator in the different modes used. I would like the authors to provide this information, so that the maximum benefit may be derived from their work. I was also interested in the suggestion that cyclical micromovement resulted in a higher level of pin-track infections. We have not found this in experimental or clinical studies and information concerning the specific geometry of the frame and clarification of the degree of movement in the micromovement group perhaps would assist in understanding this.

I believe that the conclusions in this paper can only be made in

the context of the data presented, and other observations which are not included may confirm or even refute these. Our studies have shown that application of micromotion at a very early stage after fixation is more beneficial than waiting until the healing process has begun. The work of Noordeen et al supports this finding in relation to the clinical management of fractures with external fixation.

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Noordeen MHH, Lavy CBD, Shergill NS, Tuite JD, Jackson AM.
Cyclical micromovement and fracture healing. *J Bone Joint Surg [Br]*
1995;77-B:645-8.

Authors' reply:

Sir,

We thank Professor Goodship for his letter and will deal with his comments in order.

1) Our patients were encouraged to bear weight in their fixators as soon as possible and were not discharged from hospital until they were doing so comfortably. They were all putting at least 30 kg through the affected limb. We did not measure the actual movement which occurred at each fixator in vivo, but we did test the fixator bodies in a Hounsfield testing machine and found a consistent force-displacement curve in those with an unlocked silicon cushion. This was an exponential curve giving a displacement of 1 mm at 180 N and 1.5 mm at 310 N. We are aware that all fixators, even when locked, have an additional degree of cyclical micromovement because of the bending moment on the pins.

2) Fracture stiffness was measured by removing the fixator, attaching an electronic goniometer and measuring force through the heel using a load cell. Measurements were started in the fourth week and carried out at two-week intervals. Our computer-derived graph on page 646 gives the misleading impression that there was a reading taken at two weeks. We apologise for this.

3) Our patients had a uniaxial Orthofix fixator applied according to the manufacturer's published instructions with two pins above and two pins below the fracture. We aimed at a pin-bone distance of 5 cm. The fixator was applied anteromedially.

4) We found that cyclical micromovement using a dynacushion produced more pin-track sepsis than axial dynamisation. Before union the dynacushion holds the ends of the fracture apart, thus reducing the component of the ground force transmitted through the fracture fragments, and increasing that which is transmitted through the pin-bone interface. In contrast, with axial dynamisation there is an increasing component of force through the fracture and a decreasing amount through the pin-bone interface. We would not expect to find such a difference in pin-track sepsis when comparing dynacushion fixators with locked fixators, because of the inevitable cyclical micromovements that occur with the latter.

5) We agree with Goodship that his work supports the premise that very early cyclical micromovement of small amplitude helps the healing process. Our work is not inconsistent with this, but does seem to show that late cyclical micromovement with an amplitude of around 2 mm is deleterious.

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PERIOPERATIVE LOW-MOLECULAR-WEIGHT HEPARIN

Sir,

I read with interest the article in the September 1995 issue entitled 'Perioperative low-molecular-weight heparin' by Warwick et al (1995;77-B:715-9). It seems to address the question of whether the use of perioperative low-molecular-weight heparin is a safe method of prophylaxis, but it does not state what the prophylaxis is against.

Their results show that there was a decrease in the incidence of calf-vein thrombosis but enoxaparin did not alter the prevalence of proximal deep-vein thrombosis (DVT).

Three patients had ventilation perfusion scans confirming the probability of pulmonary embolism. Two were in the control group and one received enoxaparin, but it is only in the discussion that we learn that they all had isolated femoral emboli. Enoxaparin does not appear to prevent pulmonary emboli which originated from a proximal DVT, and it is known that DVT occurring below the knee does not usually propagate to give pulmonary emboli. It would therefore be of use to readers to know the exact nature of the condition for which prophylactic heparin was given.

I note that the control group wore bilateral graduated compression stockings and that no other method of prophylaxis against pulmonary embolism was used. It would be interesting to know the grounds on which the local ethical committee approved the management of the control group.

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Warwick D, Bannister GC, Glew D, et al. Perioperative low-molecular-weight heparin: is it effective and safe? *J Bone Joint Surg [Br]* 1995;77-B:715-9.

Author's reply:

Sir,

Mr Carter raises three important issues.

First, the aim of thromboprophylaxis is to prevent *clinically-apparent* deep-vein thrombosis (DVT), non-fatal or fatal pulmonary embolism (PE) and chronic venous insufficiency. Each of these is difficult to measure in a randomised trial because of diagnostic difficulties (clinical thrombophlebitis), restrictions of sample size (non-fatal and fatal PE) or latency (chronic venous insufficiency). The accepted view is that *venographic* evidence of DVT is an indirect marker of these outcomes and we used this in our study. It does, however, have several important drawbacks:

- 1) the correlation between venographic evidence of DVT and the outcome measures is assumed rather than validated;
- 2) it represents a single moment in time and in several clinical trials proven thrombi have formed after a negative venogram;
- 3) the natural history and significance of an isolated femoral DVT after THR are not clear;
- 4) in clinical trials the venogram is often performed while the patient is still anticoagulated; and
- 5) the kappa values for venography, reflecting intra- and inter-observer variation, are defined as *good* rather than *very good*.

Secondly, we aimed to show the efficacy and side-effects of enoxaparin. It was reassuring to find that the perioperative side-effects (bleeding, the amount of drainage and intraoperative blood loss) were not distinguishable from those of the control group, although there was some increased bruising and discharge from the drain-site. The reduction in calf rather than proximal thrombi in our study suggests that a short course was probably insufficient. The table in our paper of published clinical trials of low-molec-

ular-weight heparin (LMWH) in which the agent was given for the traditional 6 to 14 days, showed that LMWH is effective in reducing the prevalence of both overall and proximal DVT.

Finally, three principles were accepted by the Ethics Committee. All our patients had bilateral compression stockings, regional anaesthesia and early mobilisation, each of which individually has a thromboprophylactic effect. The standard practice in our centre at the time of our study was to use only stockings and early mobilisation. In this way the issue was one of investigating a new treatment rather than of depriving patients of an existing regime. Lastly, there was sufficient doubt about the appropriate length of administration of LMWH to justify this study.

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EOSINOPHILIC SYNOVITIS

Sir,

I was interested in the article in the July 1995 issue entitled 'Eosinophilic synovitis: a new entity?' by Tauro (1995;77-B:654-6) and would like to inform you that this is not a new entity but is

commonly seen in this part of the world. I reported the condition to the Karnataka Association Conference and the Indian Orthopaedic Association Conference in 1990. The work was published in the *Journal of the Karnataka Orthopaedic Association*.

R. M. SHENOY
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Shenoy RM, Bailor TA. Eosinophilic synovitis: a new entity. *J Karnataka Orthop Assoc* 1995;2:7-10.

Tauro B. Eosinophilic synovitis: a new entity? *J Bone Joint Surg [Br]* 1995;77-B:654-6.

Author's reply:

Sir,

Thank you very much for drawing my attention to my failure to acknowledge the earlier report of this condition by Dr Shenoy. I completely agree with his findings, but his paper was published after you had accepted mine.

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