those receiving general anaesthesia with parenteral analgesics postoperatively (23 of 30).” In the latter study [4], of 94 patients undergoing total hip replacement, it was also stated clearly in the results section that “the frequency of DVT involving both the thigh veins and calf veins was significantly lower in the epidural group than the corresponding frequency in the general anaesthesia group.”

J. Modig
Uppsala

Sir,—In the editorial “Deep Venous Thrombosis and Anaesthesia” [1] and in our paper “Antithrombotic Efficacy of Continuous Extradural Analgesia after Knee Replacement” [2] published recently, it was stated that Dr Modig and colleagues [3,4] did not find a significant reduction of the total incidence of deep vein thrombosis when comparing extradural and general anaesthesia in patients undergoing hip surgery. However, we have since realized that the term used by Modig and colleagues: “the frequency of deep venous thrombosis involving only the calf and the thigh veins” was equivalent to the total incidence of deep venous thrombosis—that is, involvement of the calf or the thigh. Thus Modig and colleagues did indeed demonstrate a significant reduction in the total incidence of deep venous thrombosis.

L. N. Jørgensen
Copenhagen

Sir,—Thank you for the opportunity to reply to the letter from Dr Modig. As I consider him to be a key worker in the field of deep venous thrombosis (DVT) and anaesthesia, his comments deserve a detailed reply.

I wish first to state a philosophical point which may partly explain our apparent differences of interpretation of his work. I consider that the primary question to be answered in any study of DVT is to know the number of patients with DVT, whatever the site of DVT. Clearly, at the time of investigation, if a patient does not have a DVT, they cannot have a pulmonary embolus, unless a DVT develops subsequently. A major and extensive DVT may be a densely adherent and organized clot and pose little threat of embolism. However, an apparently minor clot in any site in the deep veins may propagate and embolize.

For these reasons, I consider the total incidence of DVT, and also incidence of normal venograms, to be the most important finding in any such study. In my editorial [1] I stated that “in both the major studies Modig did not report the total DVT incidence”. I consider this statement to be correct, but that might depend on one’s interpretation of the meaning of “report”. Nowhere in the text or tables is there any mention of total incidence, in either wording or numbers. However, in the first study [3], there are two complex figures from which it is possible, with difficulty, to derive the raw data, thus enabling the calculation and analysis of total DVT incidence. I confess that I had not done this previously, as I had assumed that Modig would have done this and, as it was not mentioned, that there was no important, or statistically significant, result. This presumption was strengthened further by the fact that Modig had gone on to conduct a larger study [4], strongly suggesting that the first had been inconclusive.

To my surprise, my analysis of the raw data shows a most conclusive and important result, as follows: Number of patients after GA developing DVT = 23, no DVT = 7. Number of patients after extradural developing DVT = 12, no DVT = 18. $2 \times 2$ Chi-square analysis with Yates’ correction yields $P < 0.01$.

This implies, therefore, that the study in 1983 which was the first published, prospective, randomized study using impeccable methodology, contains a most important result which was never stated or, apparently, analysed. I therefore have to change my conclusions about the research from “highly suggestive but not proven” to “almost certainly proven”. What I would now wish to learn from Modig and his colleagues is:

First, why did they not report and analyse this highly important finding?

Second, what was the total incidence of DVT in each group in the later, larger study?

I do hope that this reply puts the record straight.

P. J. McKenzie
Oxford

REFERENCES