Correspondence

Thromboembolic complications after arthroscopic knee surgery

Sir—Prophylaxis with LMWH is strongly recommended after arthroscopic knee surgery in the report by Schippinger et al. "Thromboembolic complications after arthroscopic knee surgery," Acta Orthop Scand 1998; 69 (2): 144–146.

After reading the paper, I can find no documentation for this recommendation. Since all the patients were given LMWH, their study is unable to tell whether LMWH are better than no prophylaxis or other types of prophylaxis. Therefore, the design of the study does not allow such a recommendation to be made.

The finding of a positive ventilation perfusion scan was interpreted as pulmonary emboli and by this criterion, 9 of 101 patients had this complication. Arthroscopic surgery is considered a safe procedure for a patient, but now it seems to be very hazardous. In the paper by Schippinger et al., the use of chemical thromboprophylaxis is recommended following the results of various paraclinic tests. However, it is not clear whether these tests reflect any clinical problem. To my knowledge it has not yet been documented that clinical thromboembolic complications resulting in mortality or morbidity after arthroscopic knee surgery are of major significance. If chemical thromboprophylaxis is to be used for the patient's benefit, a clear clinical problem must be defined. If this is possible, then randomized studies should be carried out with clinical end-points such as mortality or morbidity but not end-points from various paraclinic tests.

The study by Schippinger et al., together with most thromboprophylactic studies in orthopedic surgery, has hardly any clinical value, since the basic question has not yet been answered. Can postoperative mortality and morbidity be reduced by chemical thromboprophylaxis? Despite many years of various industrially financed thromboprophylactic studies, we do not know whether the use of chemical thromboprophylaxis benefits our patients at all.

Investigators should be encouraged to use clinical end-points in thromboprophylactic studies. Thromboprophylactic studies using paraclinical tests as end-points for the purpose of clinical recommendations should be abandoned and should not be published.

Jens V. Sørensen
Department of Orthopedics, Aarhus University Hospital, Aarhus Amtssygehus, Tage Hansens Gade, DK-8000 Aarhus C, Denmark

Sir—Our experience, before we started the study with LMWH, is that DVT can be a serious complication after arthroscopic knee surgery. We had seen patients with proximal femoral vein occlusion and in one patient even DVT of the inferior vena cava. These patients were admitted directly to the Department of Angiology and we therefore originally missed the diagnosis. They were not given thromboprophylaxis prior to surgery. Therefore we found it unethical to have a control group without LMWH, since it is a fairly standardized prophylaxis regimen with other orthopedic procedures, such as total joint replacements, below-knee plaster application, etc.

Dr. Sørensen is right in stating that arthroscopic knee surgery is safe, but this is not true for every patient and we as physicians want to offer as much safety as possible. The so-called paraclinical tests are well established test procedures in detecting DVT and PE, as clearly shown in the literature.

The aim of our study was to assess the frequency of DVT and a clinical prophylaxis regimen, not to define the probable mortality rate without prophylaxis. This study was at no time financially supported by any pharmaceutical industry and the pre- and postoperative screening tests were very demanding for both clinician and patient. I strongly encourage Dr. Sørensen to carry out a baseline study on the incidence of DVT, without chemical prophylaxis under the same preconditions and to compare the morbidity with our results. However, I am not sure that this is still ethical in these days under the circumstances.

In conclusion, we feel that chemical prophylaxis is of value for the patient because major complications in most patients as well as the manifestations of DVT and/or PE were avoided in our study population. Furthermore, I think there is no doubt that chemical prophylaxis has reduced the risk of DVT tremendously, as discussed at the 1997 SICOT meeting and published in the journal "Orthopaedics" Volume 5, No. 2/ Supplement March/April 1997, for example, and in various other publications.

Gert Schippinger
Department of Traumatology, Universiy of Graz, Auenbruggerplatz 7 a, AT-8036 Graz, Austria

Editor's comment

Dr. Sørensen thinks that Dr. Schippinger's article should not have been published. Dr. Schippinger cannot be blamed for its publication, although he suggested it. The decision was taken by the Editorial Office!

Anders Rydholm, Editor