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

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Outcome after reconstruction of the anterior cruciate ligament in athletic patients

Sir,
We read with interest the article in the May 2003 issue by Meighan et al entitled ‘Outcome after reconstruction of the anterior cruciate ligament in athletic patients’. We appreciated the effective study design and the simple and easy exposition. However, there was no mention of sample size or the statistical power. It is disappointing that a valid paper is invalidated by the absence of this fundamental information. It is well known that sample size is important to establish the number of people for inclusion in a study in order to provide valid results. The statistical power gives information about the validity of significant results. If the sample is small, the results can be non-significant for this reason.

R. PADUA, MD
R. BONDI
E. CECCARELLI
San Giacomo Hospital
Rome, Italy.


Author’s reply:

Sir,
I would accept the criticism made by Dr Padua and his colleagues regarding our article. The sample size was small both in the early and late groups. The difficulties of recruiting patients to this type of surgery were referred to in our discussion. Although a larger sample size in both groups would have been desirable, we were unable to demonstrate any significant advantage for an immediate anterior cruciate ligament (ACL) reconstruction. In fact, the significant differences observed were in favour of delaying the procedure.

We consider that our study does contribute to the debate on this issue as there is an increasing tendency in some parts of the world to carry out immediate ACL reconstruction, without any evidence-based support that this is a more effective method of management.

J. F. KEATING, FRCS Ed (Orth)
Royal Infirmary of Edinburgh
Edinburgh, Scotland.

Fixation of fractures of the femoral neck

Sir,
We read with interest the paper by Lykke et al in the April 2003 issue entitled ‘Fixation of fractures of the femoral neck: a prospective, randomised trial of three Ullevaal hip screws versus two Hansson hook-pins’. The authors compare two different devices for internal fixation of femoral neck fractures. The study is based on radiographic outcome as well as the need for a secondary arthroplasty. One conclusion is that “osteosynthesis is the method of choice for most displaced fractures of the femoral neck”. We don’t find any support for this statement, in this study or in the literature. Furthermore, if followed by the readers, it may lead to tragic consequences for a large number of patients.

Firstly, 24 of 224 (11%) displaced fractures in the study were regarded as ‘irreducible’ and excluded prior to randomisation. If these fractures, prone to healing complications, had been included, the failure rate after internal fixation of displaced femoral neck fractures would have been higher than the 70 of 200 (35%) reported in the study. The results are in the range seen in other studies.

However, our main point is that the re-operation rate after internal fixation is not a suitable single outcome measure as it incorporates other factors such as local traditions and a willingness by the surgeon to re-operate.

Six Swedish prospective, randomised studies have compared internal fixation and primary arthroplasty in the treatment of displaced femoral neck fractures in the elderly. The general conclusion is that a primary cemented arthroplasty is superior to internal fixation regarding need for secondary surgery, function, pain, nutritional status and quality of life in the active and lucid patients without being more costly.

T. JOHANSSON, MD, PhD
Linköping University Hospital,
Sweden.

G. NEANDER, MD, PhD
Danderyds Hospital, Karolinska Institutet,
Stockholm, Sweden.

C. ROGMARK, MD, PhD
Malmö University Hospital,
Sweden.
An observation not previously reported is that the penetration of the drill into the joint during surgery changes the fate of the fracture dramatically. This observation is to our knowledge new. It is also a warning against relief of pressure by intentionally drilling up into the joint to evacuate bleeding tamponade. High intracapsular pressures have been registered in undisplaced fractures of the femoral neck. \(^1\) Aspiration of the haemorrhage is recommended in these cases.

Arthroplasty in all cases of displaced femoral neck fractures can be considered as unnecessary overtreatment. An optimised balance between primary internal fixation and primary arthroplasty is the goal for future treatment. At the present stage we recommend: do not behead all because some fail.

**Authors’ reply:**

Sir,

We thank Drs Johansson, Neander and Rogmark who have all published important randomised studies between osteosynthesis and arthroplasty for femoral neck fractures, for their comments.

We chose not to include the irreducible fractures. Inserting screws or pins into a fracture which is not reduced predisposes to complications of healing, as our study shows. In this setting, choosing an ‘intention-to-treat’ – analysis, or classifying these fractures as failures, would, in our opinion, be wrong.

Johansson et al seem to have misunderstood the message in our study, thinking that all displaced femoral neck fractures should be internally fixed according to our recommendations. We are well aware of the randomised studies they referred to and the present trend in Sweden to perform more primary arthroplasties for the displaced femoral neck fractures. The important message from our study is that a certain number of these fractures benefit from primary internal fixation and that better results are achieved if the operations are performed according to criteria of accurate reduction and positioning of internal fixation. Therefore, we found it important to report results from Norway, where the quality and the number of cases of internal fixation is still high.

All our patients were called in for radiographic and clinical evaluation at four months, one year and two years after the operation. The outcome is thus based not only on ‘need for reoperation’, but also on repeated radiographic evaluation, evaluation of functional outcome, assessment of pain, as well as a description of the living conditions of the patients.
A combination strategy with co-ordinated pre-, peri- and post-operative elements is far more likely to achieve maximum practical impact. Both articles addressed different aspects of the same problem, but failed to develop this into a co-ordinated solution based on the whole patient process. One of the incentives for the Helm article was the restrictive cost of cell salvage. However, they failed to consider the potential value/role of anti-fibrinolytic therapy as a cheaper alternative. Jeserschek et al\textsuperscript{1} used aprotinin; other drugs have been used successfully in this context,\textsuperscript{12,13} are cheaper and have reduced allergic side effects.

Pivotal to any pragmatic improvement in current practice is the need for re-education and the development of a consensus approach. To be achievable and to realise the full potential of any change, close cross speciality co-operation (surgeons, anaesthetists and GPs) is vital. With appropriate dialogue, the role of each clinician can be defined and protocols drawn up and adapted according to local factors.

D. BOOTH
E. KOTHMANN
M. TIDMARSH
Cumberland Infirmary,
Carlisle, Cumbria.


7. Guidelines for the blood transfusion services in the United Kingdom. 6th edition. Published by TSO.


15. The Handbook of Transfusion Medicine. 3rd Edition. Published by the Blood Transfusion Services of the United Kingdom. Edited by DBL McClelland.

Author’s reply:

Sir,

May I thank the authors for their interest in our paper. The pre-operative, peri/intra-operative and post-operative factors listed in their letter are a useful summary of the various modalities which one can employ to limit the transfusion of allogenic blood. Our protocol was devised according to the facilities available to us at the time. Although our protocol clearly does work, I do not suggest that it is perfect in every respect. However, as stated in our discussion, I would advocate that many of the principles of our protocol, such as the introduction of rigid guidelines, education of medical and nursing staff, use of clinical audit, bedside haematocrit estimation and a more scientific approach to the problem are important factors in reducing transfusion of allogenic blood. We did not consider anti-fibrinolytic therapy as part of our protocol, and I hope the authors would agree that if we were to use this as well as our protocol, the number of units of allogenic blood transfused could be further reduced.

A. T. HELM, MRCS
Royal Preston Hospital,
Preston, UK.