A prospective, randomised comparison of autologous chondrocyte implantation versus mosaicplasty for osteochondral defects in the knee

Sir,

We read with interest the article by Bentley et al\textsuperscript{1} in the March 2003 issue entitled ‘A prospective, randomised comparison of autologous chondrocyte implantation versus mosaicplasty for osteochondral defects in the knee.’ While the authors are to be commended we felt compelled to clarify two areas of comparison in the study. The patient selection, and the post-operative regime cast doubt about the validity of their comparison of mosaicplasty and autologous chondrocyte implantation.

The authors noted that the mean size of the lesions treated in both groups was 4.66 cm\textsuperscript{2}. As has been repeatedly reported in the literature by us and others,\textsuperscript{2-8} the prime indication for the mosaicplasty is for chondral or osteochondral lesions in the range of 1 cm to 4 cm\textsuperscript{2}. In these defects, the mosaicplasty continues to provide good to excellent results in approximately 90% of cases. For lesions greater than 4 cm\textsuperscript{2} and when the patient has undergone previous surgery, we consider mosaicplasty as a salvage procedure offering a significantly lower success rate. The lesion, as depicted in Figs 1a and 1b of this article, would certainly fit into the latter offering a significantly lower success rate. The lesion, as depicted in Figs 1a and 1b of this article, would certainly fit into the latter category. When mosaicplasty is compared with autologous chondrocyte transplantation employing the proper indications, such as reported by Horas et al,\textsuperscript{9} mosaicplasty demonstrates a 90% good to excellent clinical outcome and more rapid improvement than autologous chondrocyte implantation. Control arthroscopies and biopsy analyses at two years reported in the same paper demonstrated hyaline cartilage cover of the transplanted area of mosaicplasty cases and fibrocartilage filling after autologous chondrocyte implantation.

There was also an important difference in these two studies which reported different outcomes of mosaicplasty and autologous chondrocyte implantation. Horas et al\textsuperscript{9} used mosaicplasty to treat the optimal defect followed by the recommended rehabilitation protocol; Bentley et al\textsuperscript{1} ordered a special form of rehabilitation. In all of our former publications and in independent mosaicplasty papers, immediate exercises and non-weight-bearing for two to four weeks followed by a two week period of partial loading were recommended.\textsuperscript{2-7} Bentley and colleagues immobilise the operative knee in plaster for ten days while allowing weight-bearing immediately. These measures differ from our post-operative regime for mosaicplasty.

Considering these two main differences and different results of the two randomised, prospective comparisons, it seems that the defect size and rehabilitation protocol may have a crucial influence on the clinical outcome of mosaicplasty. Our 11 years experience with mosaicplasty has demonstrated that the early good to excellent results continue to be sustained and several other retrospective and prospective evaluations have confirmed this clinical outcome.\textsuperscript{4,5,9-13}

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\textsuperscript{10} Solheim E. Mosaicplastikk ved leddbruskskader i kne. Tidsskr Nor Laegefor 1999;27:4022-5.
\textsuperscript{13} Barber FA, Chow JCY. Arthroscopic osteochondral transplantation: histologic results. Arthroscopy 2001;17:832-5.

Author’s reply

Sir,

I thank Doctors Kish and Hangody for their letter and interest in our article.

With regard to the size of lesions treated by the surgeons who carry out mosaicplasty, the article by Hangody may have indicated that the prime indication was for small osteochondral defects, but...
the method was being proposed by the author for larger defects at the time of the initiation of our study. With regard to the results of biopsies, it is difficult to see how a mosaicplasty could demonstrate hyaline cartilage when part of the repair is fibrocartilage in the spaces between the plugs. This obviously depends on the site of the biopsy and is a big problem in interpreting results of mosaicplasty.

In our study we saw hyaline-like cartilage in a proportion of the biopsies after autologous chondrocyte transplantation and the excellent and good results showed an intact surface.

With regard to the rehabilitation programme, of course the post-operative mobilisation was different and the rationale for our method is that not only is motion beneficial to articular cartilage, but also load-bearing. For this reason we chose to avoid the possibility of detachment of the grafts, particularly in the patellofemoral joint, by a short period of immobilisation, whilst allowing the patient to fully weight-bear so that the joint as a whole would benefit from this stimulus to cartilage metabolism.

Finally, I would say that we agree that mosaicplasty may have indications for very small defects but the quality of the repair produced is always likely to be inferior to that which can be achieved with a successful articular chondrocyte graft. Therefore, it appears that mosaicplasty should only be used for small defects until such time as a better method is apparent, and we consider that autologous chondrocyte implantation is a better method.

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**Reduction of blood loss using high-dose aprotinin in major orthopaedic surgery**

Sir,

We read with interest the article by Jeserschek et al in the March 2003 issue entitled ‘Reduction of blood loss using high-dose aprotinin in major orthopaedic surgery’. The authors present a prospective, randomised, placebo-controlled study on the effect of high dose aprotinin on blood loss in patients admitted for major orthopaedic surgery. In the discussion the authors conclude: “We have shown that aprotinin decreases blood loss and transfusion requirement in major orthopaedic operations.”

Although there is a clear outcome in this study we have some questions, especially about the design.

1. The authors chose four different types of surgery in 18 patients.
2. The authors investigated types of surgery, which normally show great variations of blood loss (revision hip surgery, tumour surgery).
3. Some important factors concerning peri-operative blood loss are not mentioned. For instance the pre-operative use of a NSAID can increase blood loss by 45%. The patient’s body temperature at the end of surgery is not mentioned. Studies in total hip surgery show a 30% increase in peri-operative blood loss if the body temperature is only 1.6°C lower. The type of anaesthesia, again not mentioned, can influence the amount of peri-operative blood loss.
4. The transfusion of red blood cells during surgery was performed by a (different?) anaesthesiologist based on ‘estimated’ blood loss.

In all, we demonstrate that at least seven unknown or subjective factors can change the outcome of this study. That is why the further items in the discussion section (hospital stay and cost effectiveness) cannot be based on it. Unless the authors can add the above-mentioned factors to this study, the results of this prospective, randomised, double-blinded and placebo controlled study are useless.

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Author’s reply:

Sir,

We thank Doctors Slappendel, Dirksen and van Hellemontd for their interest in our paper. We appreciate the issues they brought up in their letter as this gives us another opportunity to point out different aspects of this study.

1. The study was carried out in order to assess the effect of aprotinin on blood loss in major orthopaedic surgery and the patients were included consecutively, if high blood loss was expected. The primary end-point was to evaluate orthopaedic operations of long duration which included revision arthroplasties of the hip and knee joint and resections of soft tissue sarcomas. Obviously these operations cannot be standardised for studies concerning blood loss even if only one procedure is chosen so the inclusion of four different types of major orthopaedic surgery seems to be justified.

2. These operations show great variations of blood loss. Nevertheless the range of intra-operative blood loss (aprotinin group 531 ml to 2.538 ml, control group 679 ml to 5.550 ml) indicates that the groups were comparable with respect to the similar lower value. These figures are similar or even higher than the mean blood loss in total hip arthroplasties where the effect of aprotinin is still controversial.

3. We also looked for other factors which could have influenced the outcome of this study. There was a similar distribution of the pre-operative use of NSAIDs. Two patients in the aprotinin group and one patient in the control group received NSAIDs pre-operatively. In our department of anaesthesia the use of the ‘WarmTouch 5000’ is well established to maintain normal body temperature. It is well-known that warming of one half of the body can preserve normothermia in patients undergoing major orthopaedic surgery. All our patients were warmed about 30 minutes pre-operatively with the WarmTouch 5000, Stage III, which is about 39°. All but one patient in the aprotinin group received general anaesthesia. This lady is case No. 6 with an intra-operative blood loss of 1305 ml.
4. Our department of anaesthesia has clear guidelines for blood transfusion. Patients with coronary disease will receive red blood cells if the haematocrit is lower than 30%. All other patients will receive red blood cell units with a level of 25% to 26%. In operations over three hours blood samples are taken by the anaesthetist intra-operatively to control the haematocrit and if required red blood cells are given according to their guidelines.

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Incidence and natural history of deep-vein thrombosis after total hip arthroplasty

Sir,
I read with interest the article by Kim et al1 in the July 2003 issue entitled ‘Incidence and natural history of deep-vein thrombosis after total hip arthroplasty’, in which no pulmonary embolism is reported for post-operative pulmonary embolism.

Sir,
I note with interest the comments of Dr Hoaglund and thank him for his interest in our paper. From my extensive investigation into the prevalence of deep vein thrombosis in Korean patients for almost two decades, I was not able to find any relevant risk factors for post-operative pulmonary embolism.

Therefore, I think that Dr Hoaglund’s comment on the prothrombin gene mutation G20210A is very important. Although I have not studied this factor yet, I believe that the virtual absence of this mutation gene in Asians indicates genetic protection for this population. I completely agree with Dr Hoaglund that genetic protection for the Asian population reinforces our knowledge that genetic factors are involved in post-operative pulmonary embolism.

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Primary and revision lumbar discectomy

Sir,
We read with interest the paper in the August 2003 issue by Morgan-Hough et al1 entitled ‘Primary and revision lumbar discectomy: a 16-year review from one centre’. We feel that the title of the article is a little misleading. It suggests to the reader that the authors intended to present the results of primary and revision lumbar discectomy with an average follow-up of 16 years. In fact, as the authors state, the minimum follow-up was one year and the maximum was just over 16 years. Hence a title such as ‘Primary and revision lumbar discectomy: a review after one to 16 years from one centre’ would have been more accurate. Including the minimum and maximum period of follow-up in the title of an article is accepted practice, as evidenced by several of the references given by the authors.2,3

Secondly, there is no mention of the mean follow-up period. Therefore, the reader has no idea of when the majority of the operations were performed. This has a bearing on the impact of the
study, since it is quite possible that most of the patients in this study have only a short follow-up.

We feel that these small omissions detract from an otherwise good study.

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Author’s reply:

Sir,
I thank Doctors Choudhary and Ahmed for their comments. I cannot agree with their criticism of the title, ‘Review’ is used as defined in the Oxford Dictionary i.e. a survey in retrospect. This title I think leaves little confusion.

The mean follow up was 6.9 years.

C. V. J. MORGAN-HOUGH, BSc (Hons), MDBBS, FRCS (Eng) Robert Jones and Agnes Hunt Hospital Shropshire, UK.

Errata

G. Cinotti, A. M. Patti, A. Vulcano, C. Della Rocca, G. Polveroni, G. Giannicola, F. Postacchini. Experimental posterolateral spinal fusion with porous ceramics and mesenchymal stem cells.

It is regretted that Figure 1a was reproduced as Figure 1b. The correct Figures 1a and 1b are shown below:

Fig. 1a Fig. 1b
Photomicrographs showing cell adhesion to ceramic granules not pre-soaked (a) or pre-soaked (b) in fibronectin solution. Figure 1a – There is no evidence of cell adhesion to the surface or inner pores of ceramic granules soaked in the cell suspension. Figure 1b – Ceramic granules pre-soaked in a fibronectin solution showing cell adhesion to the surface and inner pores of the granules (arrows) (20 x).


It is regretted that the name of one of the authors, Muratli, was incorrectly spelt as Murath.