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

©2005 British Editorial Society of Bone and Joint Surgery
doi:10.1302/0301-620X.87B2.16082 $2.00

The prognosis following acute primary glenohumeral dislocation

Sir,

We read with interest the article by Te Slaa et al1 in the January 2004 issue entitled ‘The prognosis following acute primary glenohumeral dislocation’.

Statistics are wonderful. Statistics packages give the power of computing to everyone. Sometimes they provide interesting results. Te Slaa et al1 have reported for instance that following primary glenohumeral dislocation, there was a decrease of 16% in the probability of a patient returning to work with each additional year of age. Overall however, in the 105 patients, 34% had stopped working, but in only two was this because of the shoulder injury.

We suggest that the astute reader will be confused by these figures. Common sense and some simple maths tells one that if there is a 16% decrease in probability per year of someone returning to work then in someone five years older than the base age there is only a 42% chance an individual is still working (100 x 0.845) but that in someone ten years older, the chance of them returning to work is only 17% of that of someone at the base age.

We suspect that while the statistics may be right, the question asked of the patients was not. Presumably many people had changed their job at some stage since the index dislocation, or given up work for other reasons. The p value of < 0.01 seems highly significant but common sense says otherwise!

R. J. GRIMER, FRCS(Ed) (Orth)
P. COOL, FRCS(Orth)
The Royal Orthopaedic Hospital NHS Trust,
Birmingham, UK.


Authors’ reply:

Sir,

We thank Messrs Grimer and Cool for their interest in our paper. We fully agree with them that statistics can be wonderful; and that

the interpretation of statistical quantities warrants caution, as well as knowledge of the subject.

Our estimate of 0.84 describes the effect of the age of an individual at the time of dislocation and not the ageing process of a single individual. The only statement we made was that when comparing two individuals who differ one year in age at the time of dislocation, the older individual has 14% less probability (in terms of odds) of returning to work. Indeed, as the authors state, if two individuals differ five years in age, the older one has approximately half the odds of returning to work compared with the younger one. It is obvious that, when taking a big difference of say ten years, you will predominantly be comparing people above and below the age of 65.

Hence, we think that Grimer and Cool extrapolated our estimate which was only meant to give an order of magnitude in a small time scale to a period over which effects other than just a gradual increase of age play a role.

A reversal of the same relationship makes more clinical sense (ignoring an adjustment for ‘dominance’). The mean age at the time of dislocation among those who did not resume work was 66 years; among those who did resume work, it was 30 years. Our odds ratio of 0.84 was simply meant to give an average expression of the decrease of the odds (resume work vs not resume work) with each increasing year.

R. L. TE SлаA
M. P. J. M. WIJFELs
R. BRAND
R. K. MARTI
Reinier de Graaf Groep
Delft, The Netherlands.

Intra-operative findings in varus osteoarthritis of the knee

Sir,

We read with interest the article in the January 2004 issue by Ritter et al1 entitled ‘Intra-operative findings in varus osteoarthritis of the knee’.

There is no doubt that there is a need for improvement in the methods of selection of the patients who may benefit from unicompartmental knee arthroplasty. However, the fundamental flaw of the study was the measurement of alignment of the leg with the use of anatomical axes of the tibia and femur. It was not stated whether long-leg views (including the hip, knee and ankle) with standard protocol (patella pointing forwards, unshod feet, weight-bearing on both legs) were used. The measurement of alignment and mechanical axis deviation using the methods described by Paley and Tetsworth2 are important in establishing joint orientation, confirming joint malalignment and excluding occult angular deformities. In our view, the method described in the study would not provide any meaningful information regarding alignment of
the knee, and would have compromised the findings of the entire study.

W. Y. KIM, MRCS (Glas)
Hope Hospital,
Manchester, UK.

S. JARI, FRCS (Eng), FRCS (Orth)
University of Manchester
Manchester, UK.


Author’s reply:

Sir,

We appreciate the concerns of Messrs Kim and Jari. While it is certainly possible that measurements of alignment could always be compromised to some degree, including errors, even on long-leg films, we feel these small potential differences are more than offset by the fact that more than 4000 knees were studied. All radiographs were obtained using exactly the same technique (100 cm distance, full weight-bearing, patellae pointing forward and beam level at the patella).

We disagree that the use of this method did “not provide any meaningful information” and, therefore, “compromised the findings of the entire study”, including the conclusion that within an ideal “range” of varus deformity (8 to 5˚); 91% of knees did not qualify to be ideal candidates for unicompartmental knee arthroplasties. The primary aim of the paper was to determine the percentage of patients that have normal appearing cartilage in the opposite compartment and the patellofemoral joint which may, then, represent the ideal patient for a unicondylar total knee replacement.

M. A. RITTER, MD
The Center for Hip and Knee Surgery
Mooresville, Indiana, USA.

Resurfacing arthroplasty of the hip

Sir,

We read with interest the editorial by Villar¹ in the March 2004 issue entitled ‘Resurfacing arthroplasty of the hip’.

In it he states that “What is of no doubt however, is that metal-on-metal hip resurfacing arthroplasty has a part to play in modern orthopaedic surgery and that the change from a metal-on-polyethylene design was for the better”. Based on small studies of four or less years duration, with or without roentgen stereophotogrammetric analysis, this is a bold statement.

Charnley’s resurfacing operation used polytetrafluoroethylene and not polyethylene. Charnley was so disenchanted with the procedure that he added an Appendix B to his book entitled “Low friction arthroplasty of the hip”² to try to discourage the resurfacing fad of the 1970s and 1980s. Perhaps those attracted to the ‘new resurfacing’ should read this appendix.

In 1995, Murray, Carr and Bulstrode³ posed the question, “Which primary total hip replacement”? and warned, “If clinical results are not available, a new implant should only be used if it is included in a properly conducted clinical trial”. I question the wisdom of an editorial that in any way suggests a less cautious approach.

H. HAMILTON, FRCS(C)
Port Arthur Clinic,
Ontario, Canada.


Author’s reply:

Sir,

I am most grateful to Mr Hamilton for his comments. He is correct in stating that Charnley’s resurfacing used polytetrafluoroethylene and not polyethylene and I apologise for this error. Murray, Carr and Bulstrode’s warning in 1995¹ about using new implants without properly conducted clinical trials remains true. However, in the same paper they state “we do not believe that surgeons should be forced to use only established implants; this might prevent the development of new implants that in the long term will have superior results”. And Murray was indeed a co-author of the recent paper entitled ‘Roentgen stereophotogrammetric analysis of the Birmingham hip resurfacing arthroplasty’² which described satisfactory stability of the femoral component.

The editorial reflected my view that the results of resurfacing arthroplasty may allow for cautious optimism although there clearly, as indicated, remain many unanswered questions. The current models represent a considerable improvement on previous designs and the change from metal-on-polyethylene to metal-on-metal seems to have many advantages.

R. N. VILLAR, FRCS
Cambridge Hip and Knee Unit,
Cambridge, UK.