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.

Ipsilateral recurrent lumbar disc herniation

Sir,

I read with interest the article in the September 1998 issue entitled ‘Ipsilateral recurrent lumbar disc herniation’ by Cinotti et al.

In the abstract they state that in the study group, 42% of patients related the onset of recurrent radicular pain to an isolated injury, but none of the control group did so (p < 0.001). Since the control group was selected on the grounds of not having recurrent radicular pain, this is not surprising.

S. EHRENDOERFER, MD
Law Hospital
Carluke, UK.

Author’s reply:

Sir,

I thank Dr Ehrendorfer for his interest in our paper and agree that the statement in the abstract could have been better expressed. Since patients in the control group had no recurrent radicular pain after primary discectomy, however, the only possible meaning of the statement is that none of the patients in the control group related the onset of radicular pain to an isolated injury before primary discectomy, i.e., an operated disc is more likely to develop a herniation, as a result of an isolated injury, compared with a non-operated disc. This concept is explained in the discussion.

G. CINOTTI, MD
Rome, Italy.

Redisplaced unstable fractures of the distal radius

Sir,

I read with interest the article in the July 1998 issue entitled ‘Redisplaced unstable fractures of the distal radius’. I have two queries in relation to the discussion. The author states that “theoretical concerns about infection of a distal pin track leading to septic arthritis of the wrist, have been unfounded,” but she reports about three times as many pin-track infections in the non-bridging as in the bridging group. Were these infections reported per individual pin, or per patient?

The higher reported rate of infection could well lead to an increased rate of arthritis in a larger series. Was this the case in the other consecutive series of 50 patients which the author mentions?

The discussion of the different methods of treatment of unstable fractures of the distal radius does not mention Kapandji pinning of the wrist.

Although not very popular in the English-speaking world, this technique is often used in Latin countries and provides results comparable to those of the non-bridging fixator.1-5

P. P. CASTELEYN, MD
Free University of Brussels
Brussels, Belgium.

Author’s reply:

Sir,

Professor Casteleyn’s concerns about the higher rate of distal pin-track infection are unfounded. Statistically, there was no difference in the number of pin-track infections (which were reported per patient) in the non-bridging and bridging groups, although he correctly points out that there was an increased number in the non-bridging group. Of 150 patients treated by this technique none had deep pin-track infection or septic arthritis of the wrist. Our definition of pin-track infection includes some increased redness around a pin track. Major pin-track infections are extremely unusual with either technique. There is no evidence that pin-track infection makes any difference to the final outcome.

The discussion did include comment on percutaneous wiring techniques, one of which is, of course, Kapandji pinning of the wrist. The acknowledged problem with this technique is the difficulty in making the pins hold in very osteoporotic bone. I am sure that Kapandji pinning gives excellent results in the young patient, but I believe that as yet its place has to be proven and compared against other techniques in the older osteoporotic patient.

M. M. McQUEEN, FRCS Ed(Orth)
The Royal Infirmary
Edinburgh, UK.

Hydroxyapatite augmentation of the porous coating improves fixation of tibial components

Sir,

We read with interest the article in the May 1998 issue by Önsten et al entitled ‘Hydroxyapatite augmentation of the porous coating improves fixation of tibial components’.1


©1999 British Editorial Society of Bone and Joint Surgery
0301-620X/99/29767 $2.00
They performed a well-structured study on three different types of tibial fixation, but the conclusions from the study, namely, improvement of fixation by hydroxyapatite (HAPC) and similar micromotion of both cemented and HAPC components, do not appear to be supported by the results. As is obvious from Table IV, both porous and HAPC tibial components have significant higher migration values than those of cemented components as judged by maximal total points motion (MTPM), rotation and maximum subsidence at three months.

We wondered if HAPC does improve fixation since the authors based their view on only two significant differences namely, a difference in MTPM data between 12 and 24 months (and this only after correction of the reference point, Table IV) and the lower percentage of tibial components which had micromotion of <0.2 mm at 24 months. With regard to the first, ‘normal’ MTPM data, transverse, sagittal and longitudinal rotation, and proximal migration and subsidence were not significantly different between groups. Secondly, when studying the presented MTPM data (Table IV and Figure 3), the variability (i.e., the standard deviation) of the HAPC components was significantly higher compared with that of the porous and cemented components.

Also, the HAPC and porous components stabilised after 12 months, whereas cemented components continued to migrate. The continuous micromotion of the cemented components after 12 months is also present for all rotations and subsidence values (Table IV, Figures 4 to 7).

R. G. H. H. NELISSEN
E. R. VALSTAR
P. M. ROZING
Leiden University Medical Centre
Leiden, The Netherlands.


Author’s reply:

Sir,

We have used the maximum total points motion (MTPM) difference between 12 and 24 months as the strongest variable in interpreting our results. This is based on the prognostic findings of RSA in knee replacement presented by Ryd et al. I agree that these results may appear to contradict the MTPM results as measured with the postoperative examination as a reference. It must be remembered, however, that the MTPM represents a vector length, and as such cannot be subjected to simple addition or subtraction without noting the direction. The uncemented components may well move backwards and forwards in the bone bed, while the cemented components may move steadily in one direction. From this it follows that Figure 3 cannot be used to interpret what happens between 12 and 24 months.

The MTPM difference had a very skewed distribution, and it is best to examine that of the median values. If these are studied in Table IV, it will be found that the HAPC group had the lowest median value. We have also used non-parametric tests when comparing the different groups.

I. Önsten, MD, PhD
Malmö University Hospital
Malmö, Sweden.


The role of methylmethacrylate monomer in the formation and haemodynamic outcome of pulmonary fat emboli

Sir,

We read with interest the article in the January 1998 issue entitled ‘The role of methylmethacrylate monomer in the formation and haemodynamic outcome of pulmonary fat emboli’ by Elmaraghy et al.

The authors observed that there was haemodynamic evidence of pulmonary embolism, in the form of an increase in pulmonary artery pressure (PAP), with reaming and intramedullary canal pressurisation irrespective of the presence of methylmethacrylate (MMA) monomer in the femoral venous blood. They concluded that MMA monomer had no effect on the formation of fat emboli or their pulmonary haemodynamic outcome during cementing. The reader may conclude erroneously that the process of cementing and intramedullary pressurisation does not adversely affect pulmonary haemodynamics. Although it is true that MMA monomer is not required for the formation of fat emboli, the process of cementing and the resultant intermediary hypertension cause release of extensive marrow content and fat with adverse effects on pulmonary haemodynamics.1

In their study the mean PAP rises from 12 mmHg at rest to 16 mmHg during reaming and then to 21 mmHg during cement pressurisation. The authors have not analysed the data to check whether this increase in the PAP is statistically significant, but it may indeed be so. Certainly, their main conclusion that MMA monomer is of little or no significance is borne out by this data.


Author’s reply:

Sir,

We thank Dr Parvizi and his colleagues for their interest in our paper.

Neither the data nor the text suggest that the process of cementing and intramedullary pressurisation do not adversely affect pulmonary haemodynamics. On the contrary, they point out that cementing and intramedullary pressurisation consistently adversely affect pulmonary haemodynamics. This effect, however, does not correlate with, and occurs in the absence of, methylmethacrylate monomer. Hence the conclusion that “the presence of MMA monomer has no effect on the incidence or haemodynamic outcome of pulmonary fat emboli produced by femoral reaming and the preparation of the intramedullary canal”.2

We have analysed the data to compare whether the increase in PAP and intramedullary pressure from reaming to cementing is significant using a two-tailed unpaired Student’s t-test. There is no significant difference in the PAP from reaming (16 ± 1.3, n = 20) to cement (21 ± 1.8, n = 5), p = 0.0844. There is also no significant difference in intramedullary pressure from reaming (325 ± 40, n = 20) to cement (449 ± 51), p = 0.1567. This may be due to
a small effect and no real difference, or to small sample sizes for these specific group comparisons, given the trends seen.

E. H. SCHEMITSCH, MD, FRCS C
University of Toronto
Toronto, Canada.

Ankle fractures in diabetics

Sir,
The article by McCormack and Leith1 in the July 1998 issue entitled ‘Ankle fractures in diabetics’ called attention to a high level of complications; it suggested that non-operative management may be preferable in view of a high risk of major wound complications after surgery and the acceptance of malunion after closed treatment. This is contrary to our reported experience2 with ankle fractures in diabetics as well as with literature on the subject, which does not support the belief that diabetes may increase surgical risks.3 Alberti,4 for example, concludes that the liability to infection and poor wound healing can be counteracted by good control of the blood sugar and that in the diabetic morbidity and mortality after surgery should be little increased except in those with severe cardiovascular disease.

In our reported series,5 cases which McCormick and Leith may term ‘malunion’ we considered to be due to Charcot neuroarthropathy. None of our patients with this type of severe ankle deformity considered it acceptable and several sought medico-legal recourse. The problem of ankle fractures in patients with long-standing diabetes is indeed a difficult one and we appreciate the authors’ emphasis of this point. We feel, however, that the problems are reduced by careful surgical fixation of the ankle fracture and meticulous postoperative medical control of the diabetes. In addition, the destructive osteolysis associated with Charcot ankle deformity may possibly be diminished by treatment with bisphosphonates.5

J. F. CONNOLLY, MD
Orlando Regional Medical Centre
Orlando, USA.


Author’s reply:

Sir,
Dr Connolly raises some interesting points in his letter, but I am not convinced that the references which he quotes completely support his point of view. In his article he presents five patients who developed rapid progression of neuropathic joint destruction, but in only one did this follow surgical intervention. This represents five patients over a ten-year period and is therefore a small, select, subset of diabetic patients with ankle fractures. His statement that complications can be avoided by good control of the blood sugar is based on the report by Hjortrup and Sorensen,6 which looked at abdominal, vascular and proximal limb surgery. One would expect that wound problems in the foot and ankle would be more important, because the problem in diabetics is with the small peripheral vessels. The results of Hjortrup and Sorensen may therefore not apply to peripheral limb surgery.

His comment that destructive osteolysis may be diminished with bisphosphonates is interesting, but based on a report of six patients without controls. This clearly warrants further study, but there is no evidence that this would change the risk of developing significant wound problems.

We agree with Dr Connolly that these injuries represent a challenging problem, but believe that the ideal treatment has yet to be determined.

R. G. MCCORMACK, MD
University of British Columbia
New Westminster, Canada.


A new test for estimating iliotibial band contracture

Sir,
I read with interest the article in the May 1998 issue entitled ‘A new test for estimating iliotibial band contracture’. The authors claim that this test is superior to that described by Ober and that it is of quantitative value.

In a contracted iliotibial band, one of the known deformities is a flexion contracture of the knee. This means that any passive flexion of the knee makes the iliotibial band lax. The test described by Gautam and Anand requires flexion of the knee to 90°. Thus, it appears that the assessment of tightness in their method is being carried out on an iliotibial band that has been made lax by knee flexion. This defeats the very purpose of the test.

Further, the authors’ claim that the two-plane deformity is converted into a single plane is unclear, because surgical release of any contracture has to take into account the entire range of deformities and should not be selective.

The new test seems to offer no real advantage over existing methods and may lead to an underestimation of the amount of tightness since it is being performed on a lax iliotibial band.

T. M. SUNIL, MS Orth, DNB Orth
M. S. Ramiah Medical College
Bangalore, India.


Authors’ reply:

Sir,
We thank Dr Sunil for his interest in our article. His contention is based on two presumptions. The first is that the iliotibial band acts as a flexor leading to a flexion deformity of the knee and secondly that bending the knee causes relaxation of the iliotibial band which will decrease estimation of the hip deformity.

In regard to the first it has been shown that any tension on the iliotibial band has no effect on the movement on the knee.1,2 This is because of the firm attachment of the iliotibial tract through the lateral intermuscular septum on to the linea aspera of the femur.

THE JOURNAL OF BONE AND JOINT SURGERY
down to the lateral epicondyle. Since the insertion of the iliotibial band is on the anterolateral aspect of the tibia at Gerdy’s tubercle, it is unlikely to act as a flexor of the knee. We agree that the iliotibial contracture is often associated with a flexion deformity of the knee in cases of poliomyelitis, but then other factors are also involved such as contracture of the biceps femoris and tautness of the hamstrings secondary to a primary deformity of flexion of the hip.

In regard to the second assumption, the free distal portion of the tract between the lateral epicondyle of the femur and Gerdy’s tubercle acts as an accessory ligament of the knee. The tight adherence of the iliotibial tract to the femur prevents any movement of the band at the knee from being transmitted to the hip. It has been found that in all positions of the knee this portion of the iliotibial tract always remains taut. The question of knee flexion relaxing the iliotibial tract and leading to decreased estimation at the hip therefore does not arise. We have verified this from clinical measurements in various patients. Ober in his classical test kept the knee in 90° of flexion. We prefer to keep the knee at 90° for the ease of conducting the test and to control hip rotation which may be a confounding factor.

As regards the conversion of this multiplane deformity into a single plane, abduction of the hip by relaxing the band allows for extension at the hip. The amount of abduction required to achieve neutral extension can be used as a measure of the degree of contracture due to the iliotibial band.

V. K. GAUTAM, MS
S. ANAND, MS
Maulana Azad Medical College
New Delhi, India.


The treatment of subluxation of the hip in children over the age of four years

Sir,

We read with interest the article in the September 1998 issue entitled ‘The treatment of subluxation of the hip in children over the age of four years’ by Fixsen and Li. We wish to question, however, some details of posterior subluxation of the femoral head after an innominate osteotomy. In this article the authors stated that for case 10 “failure to assess and treat the considerable femoral anteversion led to posterior subluxation after a Salter osteotomy” but in a quoted previous article it is stated that “anterior displacement after (innominate) osteotomy” but in a quoted previous article it is stated that “anterior displacement after (innominate) osteotomy...is associated with increased femoral anteversion.” We would value clarification of the role of femoral anteversion in this situation.

N. KIELY, FRCS
P. MARSHALL, FRCS
Royal Lancaster Infirmary
Lancaster, UK.


Incarcerational cellulitis after total hip replacement

Sir,

I read with interest the article in the September 1998 issue entitled ‘Incarcerational cellulitis after total hip replacement’ by Rodriguez et al.

I must congratulate the authors for bringing this problem to our attention. Most orthopaedic surgeons have faced the situation described in the article and, to their relief, seen the inflammation subside in response to a short course of antibiotics.

I had an opportunity to study the case notes of some of the patients possibly included in the article while doing a fellowship at the Hospital for Special Surgery in New York. Of the seven patients whom I studied, three had some perianal pathology; a symptomatic anal fissure, a healed but recurrent anal fistula and haemorrhoids. Although all three are common conditions, their coexistence with cellulitis after a hip replacement could be more than just coincidence. The lymphatic drainage of the anus and perianal skin is to the inguinal lymph nodes. Most of it is along the medial aspect of the upper thigh, but some may be around the lateral aspect and thus prone to interruption by the incision for a total hip replacement. This may be associated with the condition described in the article.

R. G. DESHMUKH, FRCS (Tr & Orth)
Pilgrim Hospital
Boston, UK.

Author’s reply:

Sir,
The fellowship which Mr Deshmukh mentions at the Hospital for Special Surgery was with me, and he did see several of these patients during that time.

His suggestion that the cellulitis may be related secondarily to infection around the perianal and buttock areas has some merit. Failure, however, to achieve bacterial growth from the area of cellulitis as well as the fact that none of the patients had a deep joint infection, led us to believe that more research is needed to understand the pathogenesis of this unusual form of cellulitis after total hip replacement.

C. S. RANAWAT, MD
Lenox Hill Hospital
New York, USA.

Acute compartment syndrome of the thigh after joint replacement with anticoagulation

Sir,
I read with interest the article in the September 1998 issue by Nadeem et al entitled ‘Acute compartment syndrome of the thigh after joint replacement with anticoagulation’. In their diagram of a cross-section of the thigh (Fig. 1) the anterior compartment should be labelled ‘quadriceps and sartorius’ and not just ‘quadriceps’, since one arrow clearly indicates sartorius and not quadriceps.

S. EHRENDORFER, MD
Law Hospital
Carluke, UK.


Author’s reply:

Sir,
The anterior compartment of the thigh does contain both quadriceps and sartorius. Mr Ehrendorfer is correct to point out that we have missed specifically identifying sartorius. We thank him for his observation.

R. D. NADEEM, MCh Orth, FRCS
Dundee Royal Infirmary
Dundee, UK.

Femoral stem fixation

Sir,
I read with interest the Topic for Debate by Shen1 entitled ‘Femoral stem fixation’ published in the September 1998 issue. The paper outlines the arguments in favour of the Exeter polished tapered stem over the flanged Charnley stem from a biomechanical perspective, citing a number of seminal references on the long-term survival of each design of implant.

No reference is made to statistical analysis of the differences in reported rates of long-term survival highlighted by this ‘highly selective meta-analysis’. I feel that Mr Shen relies too heavily on data from the Norwegian arthroplasty register which cites a difference of 1.4% in the survival rates between the polished Exeter stem and the flanged Charnley at a mean of 4.5 years. Although comparisons between implants are most easily made by survival analysis, the method has a number of potential problems which are not always circumvented by using large numbers.

The evidence presented does imply a trend, which is supported by the bio-engineering principles presented, but nevertheless it requires statistical validation and the author should acknowledge this.

P. HARRINGTON, FRCS I, FRCS Orth
St James’s University Hospital
Leeds, UK.


Author’s reply:

Sir,
“Engineering is useful to explain but not to predict clinical outcome” (quotation, C. Lee). This paper is an outcome of this principle. I cannot agree more with Dr Harrington’s comments on the importance of the statistical treatment of data and the need to carry out randomised multicentre clinical trials on future implants.

The inclusion of data from the Norwegian arthroplasty register was not intended to split hairs (1.4%) between the Charnley and Exeter stems when used with high-viscosity cement. From 1987 to 1996, data from the Swedish hip register at 4.5 years are similar for 21 109 Charnley and 3380 Exeter stems.1 They provide a baseline for the Boneloc data.

The Norwegian data illustrated the fundamental difference between the composite beam and the taper-slip systems. The cement (foundation) of the former is under vertical shear stress and cannot stress relax. The taper-slip system experiences compression and since it can also stress relax when the patient is at rest, even a weaker cement will hold up in the short term (4.5 years). A weaker cement is defined either as one with low material properties and/or a poorly prepared material as in the first-generation Charnley stem.

G. SHEN, MS
West Covina
California, USA.