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.

The effect of the patellar tendon-bearing cast on loading

Sir,

I read with interest the article in the March 2000 issue by Tanaka et al1 entitled ‘The effect of the patellar tendon-bearing cast on loading.’ The investigation seems to miss the point of the function of a patellar-bearing cast. The design, although originally perceived by Sarmiento7 as an off-loading device, has in retrospect been seen more as a means of maintaining position and enhancing the support of the soft tissue, thereby improving stiffness around the site of the fracture. There is growing evidence that stiffness is probably the most important factor in inducing stability and healing in fractures and that the overall effect of off-loading is negligible. This was also confirmed by Wardlaw et al1 and Meggitt, Juett and Smith1 in the early 1980s. Our own work3 confirms their findings that the so-called patellar-bearing boss contributes little to off-loading and has minimal influence on the control of rotation.

The role of a patellar-bearing cast is in the middle period of fracture healing when stiffness of the fracture should be enhanced to encourage the formation of callus. The well-crafted cast which controls rotation and excludes a patellar-bearing boss does exactly that. It seems strange that the off-loading of fractures is thought to be good for them. The work presented by Kenwright and others6 made it fairly clear that a little movement at the site of a fracture is good.

DAVID I. ROWLEY, B Med Biol, MD, FRCS
Ninewells Hospital & Medical School
Dundee, UK.


Author’s reply:

Sir,

We thank Professor Rowley for his comments on our article.

We think that achieving a certain stiffness around the site of the fracture is most important during the treatment of a below-knee fracture, and that proper loading has an important role in accelerating bone union after the middle period of healing. We therefore believe that excessive and/or prolonged unloading is not an appropriate treatment for a below-knee fracture.

We usually use a patellar tendon-bearing cast during the early stage of a below-knee fracture, including fractures around the foot or ankle. Proper unloading is sometimes necessary for children or elderly patients who may have difficulty in walking, depending on the site or the type of fracture.

Our study clarified that the unloading effects of a conventional patellar tendon-bearing cast were approximately 30%. Moreover, we have developed a below-knee walking cast which facilitates rapid improvement and regulation of the effects of unloading.

In 1998, Aita et al3 stated that “the indications for the use of patellar bearing casts and braces can be expanded to neuropathic conditions of the foot, osteoarthritis of the ankle and subtalar joints, fracture of the calcaneus, and avascular necrosis of the talus”.

Our article does not deny Sarmiento’s theory and the clinical usefulness of a conventional patellar tendon-bearing cast. Nevertheless, there is a need for a weight-bearing walking cast which facilitates certain and controllable unloading.

T. GOTO, MD
Kurume University School of Medicine
Kurume, Japan.


Bone mineral density of the radius in patients with Colles’ fracture

Sir,

With reference to the article entitled ‘Bone mineral density of the radius in patients with Colles’ fracture’ in the January 2000 issue, the conclusions of Wigderowitz et al,1 that patients with Colles’ fracture under 65 years of age require investigation, need elaboration. The measurement of bone mineral density (BMD) in control subjects has shown that the standard deviation of the normal old-age group is so low that many normal subjects over the age of 65 years have a score of T-2.5 at the distal forearm. This does not equate with T-2.5 at the spine or neck of the femur: adjustments have been suggested.2 Most of their patients are ‘osteopenic’ rather than ‘osteoporotic’. Fractures of the forearm are a risk factor for future fractures of the hip, but up to age 60 years the lag

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time to hip fracture is about 14 years. The value of early treatment is uncertain and the use of bisphosphonate is unlikely to be cost-effective, although hormone replacement therapy (HRT) may be considered. In studies of the prevention of hip fractures with alendronate in patients with vertebral fracture, the incidence of hip fracture falls maximally in the first two years: in the third year the rates of incidence in the placebo and treated groups are similar. Most fractures occur after the age of 65 years. It would be more effective to identify patients who have sustained a Colles’ fracture before 65 years and to measure their BMD when they reach their mid-sixties. In patients sustaining a Colles’ fracture after 65 years the risk of hip fracture is higher and the lag time to fracture shorter; it would be reasonable to measure the BMD in these patients. Osteoporosis can be treated with a drug such as a bisphosphonate knowing that a significant reduction in the incidence of fractures occurs during the first year. Since the risk of further fracture is much higher fewer patients will be treated needlessly and a worthwhile reduction in the incidence of fractures will be achieved.

M. W. J. Davie, MD
The Robert Jones and Agnes Hunt Orthopaedic and District Hospital NHS Trust
Oswestry, UK.


Authors’ reply:

Sir,
We agree that further research is needed in this area and we are already carrying out a much larger study of over 200 patients with Colles’ fracture who are being investigated at the time of their attendance at the fracture clinic. Our preliminary results support the conclusions in our paper that many of these patients have a reduced BMD relative to an age-matched control group.

We do not believe that some sites of densitometry are intrinsically better than others, at least for the prediction of the overall risk of future fracture. In a large meta-analysis it was clear that, at least for the prediction of all future fractures, there was no significant difference between a wide range of densitometric sites. We, and others, have drawn attention to the dangers inherent in the inappropriate use of T-scores.

We agree that the frequent use of any bisphosphonate in the patients identified as substantially osteopenic relative to age-matched peers is unlikely to be cost-effective. We emphasise in our clinical practice the importance of identifying all the contributory factors for osteopenia including low calcium intake, lack of exercise and smoking before considering the balance of advantage for any drug therapy. We agree that for many patients HRT is a more appropriate regime than the bisphosphonates.

The arguments for and against any form of screening for osteoporosis continue to be unresolved, as is the question of the most appropriate age. In our paper we were anxious to draw attention to the need to regard young patients with Colles’ fracture as a special group in whom further evaluation may well be of clinical importance.

C. A. Wigderowitz, MD, PhD
C. R. Paterson, FRCP
University of Dundee
Dundee, UK.


Displaced supracondylar fractures of the humerus in children

Sir,

I read with interest the article in the March 2000 issue by O’Hara, Barlow and Clarke entitled ‘Displaced supracondylar fractures of the humerus in children’. They have mentioned that the fractures were classified according to the Wilkins modification of the Garland system and have gone on to classify the fractures as I, IIA, IIB and III. This is not how Wilkins’ modification has been described. The correct version of the classification is I, II and III with two subgroups in III. A protocol has been suggested for the management of these common, yet difficult fractures based on their version of Wilkins’ modification. The classification system appears to have been misquoted in their paper and causes confusion.

S. Harish, FRCS
Peterborough Hospitals NHS Trust
Peterborough, UK.


Authors’ reply:

Sir,

Wilkins proposed a modification to the Garland system in 1984. He recognised that a displaced supracondylar humeral fracture may be greenstick in nature with an intact posterior cortex. He noted that this greenstick injury may also have a rotary component. A fracture with an intact posterior cortex and angulation only was termed type IIA and that with a rotary component type IIB. Garland made no reference to the greenstick injury in his system, merely classifying it in regard to the severity of displacement: non-displaced, minimal to moderate and severely displaced. In his original article there is a radiograph of a fracture which is completely displaced with no cortical contact and this is classified as moderately displaced.

Wilkins termed the completely displaced supracondylar fracture with no cortical contact as type III. A type-III A fracture has
posteromedial and type IIIB posterolateral displacement. These subtypes determine which neurovascular structures are most likely to be injured and which pin should be placed first. This subclassification, however, does not affect the ultimate management of these injuries, as both the type-III A and the type-IIIB fractures require closed/open reduction and stabilisation with Kirschner (K-) wires. We believe that it is fundamental in the treatment of these injuries to appreciate the difference between the type-II A and type-II B fractures, since this has a direct bearing on their management. Type-II A injuries rarely require supplementary K-wire stabilisation, whereas we strongly recommend pin fixation of the type-II B fracture. We did not subclassify the type-III fractures in our series since this would not alter the management.

Mr Harish has commented that the current edition of Fractures in children by Rockwood, Wilkins and King subclassifies the type-III injury and not the type-II fracture. The possibility of malrotation of the type-II injury is in fact discussed in the text, but it is not represented in the Table on page 680. While this is unfortunate, this text was published before our recommendations for the treatment of the type-II B injury. We did not cite the current edition in our bibliography and refer Mr Harish to the earlier work of Wilkins for a more comprehensive discussion of the classification of supracondylar fractures in children. We hope that this has clarified any confusion.

L. J. O’HARA, FRCS
I. W. BARLOW, FRCS Orth
N. M. P. CLARKE, ChM, FRCS
Southampton General Hospital, UK.


Acute compartment syndrome

Sir,

The article by McQueen et al in the March 2000 issue entitled ‘Acute compartment syndrome: who is at risk?’ raises a number of important issues. Many publications have already defined the high-risk patient. We do not agree that patients suspected of having a compartment syndrome should be monitored, with a fasciectomy if the readings are abnormal. Most surgeons would proceed to fasciectomy if the clinical features dictated, irrespective of pressure recordings.

We also question the authors’ figures concerning rates of fasciectomy for their tibial diaphyseal fractures. A flow chart giving information on how the patients presented, the classification of the fracture, the method of treatment and the timing of diagnosis in relation to treatment would have been of greater benefit to the reader. It is also unclear which compartments were involved in each treatment group. This would be of value if contemplating elective fasciectomy.

The rate of fasciectomy is likely to be higher if relying on pressure studies and we doubt that measurements were recorded in all patients. Diagnosis on the basis of muscle bulging or expression of blood at surgery is likewise of dubious value, as any compartment under pressure would behave in this way. No reference is made to the component of time.

Our own experience is that pressure monitoring is rarely performed outside dedicated units, and departments with a device seldom use it to its full potential. Perhaps surgeons are happy with their clinical skills for what is, in reality, a rare condition. Other reasons for not monitoring pressure include lack of experience in positioning sensors, lack of knowledge of whether pressures need to be measured at different sites within each fascial space, and the unknown accuracy of the monitors available.

In the light of increasing litigation, are we now negligent if we do not follow the recommendations from a leading unit? The Journal of Bone and Joint Surgery has previously published work from this centre and must be careful to balance authors’ recommendations with reality, especially if the conclusions do not quite fit the data.

R. ROACH, FRCS Ed
R. PERKINS, FRCS
Princess Royal Hospital
Telford, UK.


Author’s reply:

Sir,

We note with interest your correspondents’ comments. There have been no previous studies of acute compartment syndrome in a defined population to identify the groups of patients who are at most risk of developing the condition.

The recommendation that patients at risk of acute compartment syndrome should undergo tissue pressure monitoring is based on work published four years ago from this unit. At that time we established that the safest threshold for fasciectomy is a differential pressure of 30 mmHg between the diastolic pressure and the tissue pressure. In that paper we stated that use of the tissue pressure without reference to the diastolic pressure results in unnecessary fasciotomies. This was confirmed by Ovre et al. The rate of fasciectomy in this unit has remained the same despite the introduction of continuous monitoring over 15 years ago.

The advantage of continuous pressure monitoring is that the diagnosis of acute compartment syndrome is made at a mean of 16 hours earlier than occurs when the surgeon relies on clinical findings. This results in a significant reduction in the disabling complications of the neglected acute compartment syndrome. Perhaps surgeons should not be happy with their clinical skills for diagnosing a condition which, with an incidence of around 4%, cannot be considered rare. Lack of experience should not be cited as a reason for not using a technique with proven benefits.

The comments about litigation are timely. One of the commonest causes of claims of negligence against orthopaedic surgeons is neglect of an acute compartment syndrome. We recommend continuous monitoring because we feel that the medi-
Test of stability as an aid to decide the need for osteotomy in association with open reduction in developmental dysplasia of the hip

Sir,
I would like to comment on the article in the January 2000 issue by Zadeh et al. entitled ‘Test of stability as an aid to decide the need for osteotomy in association with open reduction in developmental dysplasia of the hip’. In the main I agree with the authors’ suggestion of the use of an intraoperative test of stability after open reduction. I would like to comment, however, on some points of their operative technique.

I disagree with regard to the delay of surgery until about 11 to 12 months after failed conservative treatment. In my experience of children treated surgically under 18 months, those between seven and 12 months seem to have the best results. Walking may have adverse effects and the presence or absence of radiological evidence of the femoral capital epiphysis seems to have no effect on adverse effects and the presence or absence of radiological evidence of the femoral capital epiphysis seems to have no effect on the results. Of the authors’ 95 hips, 51 are in patients younger than two years at the time of surgery. Did the authors perform bony procedures besides open reduction in children under 18 months? In my opinion, soft-tissue procedures alone are most effective under 18 months.

I believe that over two years all grade-IV and some grade-III dislocated hips need femoral shortening osteotomy and femoral varus and derotation osteotomies to avoid ‘tight reduction’ if preoperative traction has not been used. This need is more obvious in the presence of an acetabuloplasty. The authors’ series of patients has 28 grade-IV hips and only five have needed femoral shortening osteotomy. How many of the remaining 23 hips have undergone on children over two years of age and in how many has an innominate or a Pemberton osteotomy been performed? These ratios may help to explain the significantly higher rate of avascular necrosis and premature physeal arrest in grade-III and grade-IV hips in the authors’ series.

H. ÖMEROĞLU, MD
Osmanagazi University Hospital
Turkey.

Authors’ reply:

Sir,
We thank Dr Ömeroğlu for his interesting comments. The issue of timing of open reduction for developmental dysplasia of the hip is highly controversial and will not be fully resolved until it is subjected to a long-term prospective, randomised trial. We are not aware of such a project. Our study shows that open reduction with concomitant osteotomy in the age group of one to two years is associated with good or excellent results in 94% at skeletal maturity (Severin groups I and II). It is therefore for others to show that earlier surgery carries a better prognosis. Furthermore, our results apply to open reduction through a Smith-Petersen approach and could not directly be compared with other methods such as a medial approach with shorter follow-up.1 Of the 51 hips in the age group of one to two years, only two children had no concomitant osteotomy. Most (47) had an upper femoral varus and derotation osteotomy and two an innominate procedure.

In this series all children had preoperative traction for one to two weeks and most hips were therefore easily reduced without tension when the soft-tissue contractures/blocks were overcome during the open reduction. We would agree that femoral shortening is an appropriate procedure in certain cases. The senior author (AC) considers using femoral shortening when the femoral head will not easily reduce with the hip in neutral position and the knee in extension. In this series five hips were judged to require shortening, but this did not guarantee complete protection against avascular changes. In two hips (40%) premature physeal arrest was noted during the adolescent growth spurt. The serial radiographs of one of these cases appear in the original paper (Fig. 3).

H. G. ZADEH, FRCS Orth
A. CATTERALL, MCh, FRCS
The Royal National Orthopaedic Hospital Trust
Middlesex, UK.