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.

Extramedullary or intramedullary tibial alignment guides: a randomised, prospective trial of radiological alignment

Sir,

We read with interest the article by Reed et al¹ entitled 'Extramedullary or intramedullary tibial alignment guides: a randomised prospective trial of radiological alignment' in the August 2002 issue. Their article shows the importance of proper alignment of components in total knee replacement surgery. We feel, however, that their results do not necessarily lead to the final inference regarding the superiority of intramedullary alignment guides.

The authors mention that they were unable to obtain acceptable radiographs in 35 patients of 135 recruited. The reason for this is not entirely clear and no information is available regarding the proportion of the two groups in these missing patients. Their 'tibial component angle' was measured by marking the radiographs and then measured using 'a goniometer with a precision of 1°'.¹ Manual measurement of alignment on radiographs has potential for errors.²⁻⁴ This has to be kept in perspective with their final mean difference of 'tibial component angle' in the two groups of only 0.5°. In Figure 2, where they have given the distribution of 'tibial component angles' in their cases, only four (two in each group) fall outside a range of 90 ± 4°. This small variation from normal, coupled with an expected zero error of measurement technique, suggests that in their study both groups had comparable good results. With such a small difference in angles in the two groups, it is necessary to know how they standardised their technique of taking radiographs, which is not mentioned.

Although malalignment of the components is correlated with poor outcomes, it would be interesting to see if such a minimal deviation from normal would lead to a long-term difference in outcome in their two groups. Finally, their results would be applicable to the specific knee instrumentation system used in their study and would not lead to the general conclusion that intramedullary devices are better than extramedullary devices in all total knee replacement systems.

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1. Reed MR, Bliss W, Sher JL, et al. Extramedullary or intramedullary tibial alignment guides: a randomised, prospective trial of radiological alignment. *J Bone Joint Surg [Br]* 2002;84-B:858-60.
2. Wright JG, Treble N, Feinstein AR. Measurement of lower limb alignment using long radiographs. *J Bone Joint Surg [Br]* 1991;73-B:721-3.
3. Cooke TDV, Scudmore RA, Bryant JT, et al. A quantitative approach to radiography of the lower limb. *J Bone Joint Surg [Br]* 1991;73-B:715-20.
4. Floyd AS. Is the measurement of angles on radiographs accurate? *J Bone Joint Surg [Br]* 1988;70-B:486-7.

Authors' reply:

Sir,

We thank Mr Anand and his colleagues for their interest in our paper. In order to gain a definitive answer to the research question (correct alignment), radiographs were strictly assessed. As stated in our paper, the radiograph was only considered suitable for analysis if the knee was in maximum extension with the patella pointing forward, with both the hip and ankle being visible on the film. Alignment was then assessed on acceptable films only. The same 'blinded' observer assessed acceptability and alignment. In fact, however, on reviewing the unacceptable radiographs, 12 were excluded in the intramedullary group and 23 in the extramedullary.

Similar mean tibial component angles of around 90° simply means that 'on average' the alignment works out right, but ignores the fact that the extramedullary group had a larger proportion of valgus or varus knees with 'unacceptable' alignment. If you are prepared to accept up to 4° of malalignment, then there is nothing to separate the methods in our study.

It is an interesting question as to whether malalignment beyond 2° is unacceptable, but with navigation systems now offering accuracy of 0.5°, this seems a reasonable stance to take. Ritter et al¹ reported a 14% revision rate in a group of knee replacements with any degree of varus. Although only 10% of knee replacements in that institution were in varus, they accounted for 56% of their revisions.

Your final question relates to whether intramedullary superiority applies to knee systems other than the AGC. We entirely agree from a scientific perspective that this trial is system specific, although jig design differs little between most systems. The AGC system has the benefit of allowing a 0° tibial slope cut, which minimises errors of malrotation.

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1. Ritter MA, Faris PM, Keating EM, Meding JB. Postoperative alignment of total knee replacement: its effect on survival. *Clin Orthop* 1994;299:153-6.

The Ionising Radiation (Medical Exposure) regulations (IRME) 2000 – radiological considerations

Sir,

The editorial in the August 2002 issue by Butt and Walkowiack¹ on the Ionising Radiation (Medical Exposures) Regulations (IRMER)² has only recently been drawn to our attention. It is encouraging to see papers on radiation protection in clinical journals. We were concerned by the assertion that 'very few items are

relevant to clinicians'. In fact, IRMER enshrines in law the principles of good clinical practice in the requesting and performance of imaging investigations, and much which is of direct clinical relevance. The statement that the regulations 'do not apply to doctors and other health-care professionals directly' could cause confusion. The buck now stops with the Trust Chief Executive, but individual doctors and other members of staff *will* be held responsible for any breaches of IRMER resulting from their actions. We should also point out that the legislation is enforced by the Department of Health, not the Health and Safety Directorate (sic).

We would like to highlight three areas:

1) The legislation defines three key roles; those of referrer, practitioner and operator. It is unlikely that many clinicians will need to be operators or practitioners under the terms of the regulations (but *see* point 2, below). All clinicians will, however, act as referrers, and their duties are now defined in law. They are required to give the practitioner (usually the relevant radiologist or radiographer) sufficient clinical information to justify the request. It is an offence to subject a patient to irradiation without good cause. The greatest single contribution that any clinician can make is to limit referrals to those which are likely to result in significant clinical benefit.

Referrers must be properly authorised. Most Trusts will have stated in their IRMER Procedures and Protocols that all registered medical and dental practitioners will be authorised to refer patients to their departments. Non-medical personnel will have to be individually authorised by the Trust, and demonstrate that they have been properly trained to assume this role. The radiology department will require, amongst other things, a list of the names of authorised non-medical referrers and their specimen signatures. It is now an offence for an unauthorised person to instigate an exposure to ionising radiation. Nurses or ward clerks who undertake this role without proper authority could face prosecution, and in the current climate of hypersensitivity concerning the effects of radiation, and the readiness to litigate, it is likely that prosecutions *will* occur.

Clinicians who pre-sign request cards are also taking a risk, because one day someone will request something really absurd on a card that they have pre-signed; this was always bad clinical practice, now the doctor could face prosecution. We know that there are many situations where clinical nurses, can, for example, fill out request cards. It is much better that they should work to a protocol devised by the relevant clinician and sign the card themselves, rather than being given free access to a pile of pre-signed cards (effectively blank cheques) with no guidance on how to use them.

2) 'Operator' is the term used for a person undertaking any physical aspect of an exposure, the obvious example being the radiographer who presses the button to expose the patient. A key principle of IRMER is that anyone acting as an operator must have received the appropriate training. This has implications for any clinician using X-ray equipment when pinning a hip or reducing a fracture, for example. If that clinician wishes to be regarded as an operator under the terms of IRMER, he/she will need a considerable amount of training – certainly much more than the half day of lectures that doctors sat (or slept) through under the POPUMET legislation, which IRMER replaces. So far, cardiologists are the only group identified as definitely needing this training, in view of the complex high-dose procedures routinely performed by them.

The easiest and most sensible solution for clinicians using fluoroscopy (including orthopaedic surgeons) will be to work with a qualified radiographer who can act as the operator. The clinician will then be free to concentrate on the clinical procedure, with the radiographer giving advice on technical aspects of the exposure. For each procedure, there will be a diagnostic reference level (DRL) based on local practice, and the radiographer will inform the surgeon when that level has been reached. The procedure will not, of course, be terminated at that point, but a dose record will be

kept, and if a particular surgical team or piece of equipment consistently exceeds the locally agreed DRL, an investigation will be carried out. This may reveal a perfectly valid reason for higher exposures (different case mix or the adoption of a new surgical technique, for example) but where there is no such explanation, remedial action can be taken.

3) Butt and Walkowiak rightly showed that it is an offence to conduct an exposure knowing that no-one will act on the result, and the outcome of the examination must be recorded. Where it is the practice for radiologists not to report a certain group of films, as is often the case for fracture clinic or orthopaedic plain films generally, there should be a formal agreement with the clinicians concerned that they will take responsibility for recording the outcome of the exposures. If that agreement is not forthcoming, the Trust will need to ensure that either radiologists (or, in some cases, radiographer practitioners) issue reports. Any Trust which consciously allows a situation to develop in which there is no written record of the outcome of investigations is in breach of IRMER.

Compliance with IRMER involves co-operation between clinicians and their radiology departments, and it is important that all parties understand their responsibilities. Implementation of IRMER has imposed a significant workload on radiology and medical physics departments, but Butt and Walkowiak are correct in implying that this has not impinged very much on clinicians, since working practices have not changed significantly from the users' point-of-view. However, our clinical colleagues do need to understand that good practice is now supported by the force of law, and that none of us are at liberty to cut corners.

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1. Butt WP, Walkowiak J. The Ionising Radiation (Medical Exposure) Regulations (IRME) 2000: radiological considerations. *J Bone Joint Surg [Br]* 2002;84-B:781-2.

2. The Ionising Radiation (Medical Exposures) Regulations. (SI 2000, no 1059). London: HMSO, 2000.

Authors' reply:

Sir,

We are grateful for Dr Bury and Mr Taylor's comments on our editorial; in particular, their expanded coverage of points we referred to is greatly appreciated. We recommend that all orthopaedic surgeons read their comments carefully.

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The efficacy of the Pavlik harness, the Craig splint and the von Rosen splint in the management of neonatal dysplasia of the hip

Sir,

I read with interest the paper in the July 2002 issue by Wilkinson, Sherlock and Murray¹ entitled 'The efficacy of the Pavlik harness, the Craig splint and the von Rosen splint in the management of neonatal dysplasia of the hip'.

The authors have assigned a numerical value for the Graf types for statistical analysis. The problem is that Graf types IIa and IIb are morphologically the same by definition and the only difference is that type IIb patients are above three months in age.² If a hip is

scanned at two months and classified as IIa and later scanned at four months and classified as IIb, that means there has been no improvement in the hip, but the numerical classification will show this as deterioration.

On the contrary, if a hip is scanned at one month and classified as III and later scanned at two months and classified as IIa this shows a -3 improvement. If the baby was four-months-old at the second scan, the hip would be classified as IIb and this would be a -2 improvement. In fact, both situations are the same. This may have affected the results, since the von Rosen group was significantly younger at the beginning of treatment and some of them may have been classified as IIa at their follow-up scan. The older babies in the other groups may have been classified as IIb for the same level of improvement, and this may partially explain the better mean improvement in the von Rosen group. I think that the authors should give the same numerical value for IIa and IIb hips and re-evaluate their results accordingly.

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1. **Wilkinson AG, Sherlock DA, Murray GD.** The efficacy of the Pavlik harness, the Craig splint and the von Rosen splint in the management of neonatal dysplasia of the hip. *J Bone Joint Surg [Br]* 2002;84-B:716-9.
2. **Graf R.** Fundamentals of sonographic diagnosis of infant hip dysplasia. *J Pediatr Orthop* 1984;4:735-40.

Authors' reply:

Sir,

We thank Dr Avci for his comments. We considered combining Graf types IIa and IIb into a single numerical value, but although IIa and IIb have identical appearances, the significance is not the same. Type IIa is a normal appearance of a hip less than 12 weeks of age, whereas IIb represents delayed development. We felt it more logical to assign a numerical value representing a more abnormal hip to type IIb.

The proportion of hips classified IIb on the second ultrasound examination in each group was broadly similar (27%, 13%, 23% and 19% in the Craig, non-splinted, Pavlik and von Rosen groups, respectively), and the conclusion drawn from the ultrasound part of the study is unlikely to be altered by revision of the analysis. The main endpoints of the study are the radiographic appearance at one year and the need for further treatment, both of which are independent of the analysis of the ultrasound appearances.

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Sir,

We read with interest the paper in the July 2002 issue by Wilkinson et al¹ entitled 'The efficacy of the Pavlik harness, the Craig splint and the von Rosen splint in the management of neonatal dysplasia of the hip'. We would like to congratulate the authors on an interesting and useful comparison between the different braces for the treatment of DDH.

The paper is an assessment of splintage for ultrasound diagnosed dysplasia of the hip without instability. The conclusion that the von Rosen splint is more effective than the Pavlik brace for dysplasia may not be true as this is a retrospective study. The article does not state the length of time the splints were used, or if there was a difference in splintage time between the three splints. A disparity would make a difference to the ultrasound and radiological results. The von Rosen splint was used in a younger age

group than the other two splints, and this, rather than the type of splint, is the important variable. This could possibly result in bias as there is evidence to suggest that the earlier the splintage the more effective the treatment.

Sochart and Paton² found the Pavlik brace to be an effective device for dysplasia and instability. Elbourne et al³ noted that no long-term proper prospective trial has been undertaken on abduction bracing and its efficacy and Wood, Conboy and Benson⁴ found that most dysplasias resolved without treatment. We would agree with the authors that a prospective trial is necessary to evaluate the efficacy of bracing and types of splints in dysplasia or instability in DDH.

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1. **Wilkinson AG, Sherlock DA, Murray GD.** The efficacy of the Pavlik harness, the Craig splint and the von Rosen splint in the management of neonatal dysplasia of the hip. *J Bone Joint Surg [Br]* 2002;84-B:716-9.
2. **Sochart DH, Paton RW.** Role of ultrasound assessment and harness treatment in management of developmental dysplasia of hip. *Ann R Coll Surg Engl* 1996;79:505-8.
3. **Elbourne D, Dezateux CA, Arthur R, et al.** Ultrasonography in the diagnosis and management of developmental dysplasia (UK Hip Trial). *The Lancet* 2002;360:2009-17.
4. **Wood MK, Conboy V, Benson MKD.** Does early treatment by abduction splintage improve the development of dysplastic but stable neonatal hips? *J Paediatric Orthopaedics* 2000;20:302-5.

Authors' reply:

Sir,

We thank Mr Paton and Dr Paniker for their comments on our paper. By definition, Graf type 3 and 4 hips considered in our study are all unstable. Wood et al specifically considered only stable hips in their study.

In the discussion we commented that a weakness of the paper included its retrospective nature, and noted that earlier treatment of the von Rosen group was a confounding factor. The mean duration of treatment with the Craig splint, Pavlik harness and von Rosen splint was similar at 93, 85 and 98 days, respectively, so it is unlikely that any difference in effectiveness can be attributed to this factor. We agree that others have had excellent results from the Pavlik harness. The effectiveness of any splint depends upon many factors including the early treatment of appropriate cases, the expertise of the person applying and adjusting the splint and parental compliance. Local expertise may favour one type of splint, but in our institution over the course of the study, the von Rosen splint performed best.

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Correction of hammer toe with an extended release of the metatarsophalangeal joint

Sir,

We read with interest the paper in the September 2002 issue by Dhukaram et al¹ entitled 'Correction of hammer toe with an extended release of the metatarsophalangeal joint'. There is general agreement that the main deformity in hammer toe is a flexion contracture of the proximal interphalangeal joint, as the authors state. However, the definition of claw toes given in the article is

confusing. In claw toes, akin to claw fingers, the primary deformity is hyperextension of the metatarsophalangeal joint. There can be flexion deformities of both the proximal and distal interphalangeal joints.^{2,3} Contrary to general consensus, the authors define claw toe as a flexion contracture of the distal interphalangeal joint.

The authors go on to state that the metatarsophalangeal joint has never been addressed when correcting hammer toes. This is not true as release of the metatarsophalangeal joint has been described, particularly in cases with hyperextension deformity of this joint.³

They conclude by stating that their procedure is less extensive and a randomised controlled trial with proximal interphalangeal joint arthroplasty may be of value. Clearly this operation involves more dissection than proximal interphalangeal joint arthroplasty, and in 14% of patients, converts a pain-free metatarsophalangeal joint to a painful one. Are we making the cure, worse than the disease?

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1. **Dhukaram V, Hossain S, Sampath J, Barrie JL.** Correction of hammer toe with an extended release of the metatarsophalangeal joint. *J Bone Joint Surg [Br]* 2002;84-B:986-90.
2. **Coughlin MJ, Mann RA.** *Surgery of the foot and ankle*. Vol. 1. Seventh ed. St Louis, USA: Mosby, 1999:320-22.
3. **Canale ST.** *Campbell's operative orthopaedics*. Vol. 2. Ninth ed. St Louis, USA: Mosby, 1998:1750-59.

Authors' reply:

Sir,

We thank Messrs Theruvil and Kapoor for their comments on our paper. A claw toe has often been defined as having extension at the metatarsophalangeal (MTP) joint with flexion at both the proximal and distal interphalangeal joints. However, in the literature, the terms hammer and claw toes have been used interchangeably.¹ A claw toe has also been defined as a hammer toe with an hyperextension deformity,² which makes the exact definitions confusing. In addition, there has been no classification of severity; hence the Blackburn classification of hammer toes.³

We agree that the MTP joint has been addressed in the correction of hammer toe but previous authors have not addressed the collateral ligaments and dorsal capsule, despite Myerson and Sherreff's basic science paper on the subject. They usually consider extensor tenotomies.⁴

We did not conclude that our procedure was less extensive. On the contrary, we questioned the need for the extensive surgery in view of our results and expressed the view that extended correction should be reserved for the more severe cases as supported by Coughlin, Dorris and Polk.⁵ Therefore we suggested a randomised controlled trial with an isolated PIP joint arthroplasty and an extended MTP joint release in the treatment of hammer toes.

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1. **Coughlin MJK.** Mallet toes, Hammer toes, claw toes, and corns: causes and treatment of lesser-toe deformity. *Postgrad Med* 1984;75:191-8.
2. **Mizel MS, Yodkowski ML.** Disorders of the lesser metatarsophalangeal joints. *J Am Acad Orthop Surg* 1995;3:166-73.
3. **Dhukaram V, Hossain S, Sampath J, Barrie JL.** Correction of hammer toe with an extended release of the metatarsophalangeal joint. *J Bone Joint Surg [Br]* 2002;84-B:986-90.
4. **Harmonson JJ, Harkless LB.** Operative procedures for the correction of hammertoe, claw toe and mallet toe: a literature review. *Clin Podiatr Med Surg* 1996;13:211-20.
5. **Coughlin MJ, Dorris J, Polk E.** Operative repair of the fixed hammer toe deformity. *Foot Ankle Int* 2000;21:94-104.

The classification of congenital talipes equinovarus

Sir,

We read with interest the paper in the September 2002 issue by Wainwright et al¹ entitled 'The classification of congenital talipes equinovarus'. We agree with the authors that the ideal classification system should:

- 1) Yield prognostic information that is independent of the stage of disease;
- 2) Aid in the selection of the appropriate treatment modality;
- 3) Prove user-friendly and unambiguous in its interpretation and
- 4) Be open to validation.

However, we have several important reservations about the validity of their conclusions arising from the methodology of the study and the choice of statistical analysis.

With regard to the study method, the authors do not state which type of classification they routinely use when assessing clubfeet at their institution. This could lead to bias. There is also no mention of a trial period, giving all observers (two of whom were considered non-experts) an equal chance to become acquainted with each of the four classification systems. They state that the Diméglio classification² is complex in its use and admit to being confused by the original article with regard to the scoring system and its conversion into the four classes of deformity. Yet they chose to use their own interpretation of the original article, without contacting Diméglio et al for clarification.

We also question the nature of the statistical analysis. First, the authors have chosen to treat largely ordinal data as nominal since there was 'no absolute standard' upon which to base a comparison. The authors are quite explicit about this in the first sentence of their statistical methods section. This has led them to choose the ordinary kappa statistical method instead of the more appropriate "weighted" kappa.³ The ordinary kappa statistic gives equal importance to any disagreement, whether it is by one category or several on the scale. In the context of their study, this seems quite inappropriate. Secondly, only a small number of observers produced this analysis. In particular, the results for the consultants, to which a good deal of importance is attached, were generated by only two investigators. One cannot be confident that their impressions could necessarily be replicated in a further sample of consultants, who might show quite different patterns of agreement or disagreement.

Finally, there is the important issue of whether good interobserver agreement is the all-important quality of a classification system. Clearly, it is highly desirable, and a very poor agreement would limit the usefulness of any classification system. However, since clubfeet are not exclusively treated by paediatric orthopaedic surgeons, other considerations, such as prognostic value⁴ and simplicity of use in a busy outpatient department, influence the choice of a classification system.

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1. **Wainwright AM, Auld T, Benson MK, Theologis TN.** The classification of congenital talipes equinovarus. *J Bone Joint Surg [Br]* 2002;84-B:1020-4.
2. **Diméglio A, Bensahel H, Souchet P, Mazeau P, Bonnet F.** Classification of clubfoot. *J Pediatr Orthop B* 1995;4:129-36.
3. **Armitage P, Berry G.** Further analysis of categorical data. In: *Statistical methods in medical research*. Third ed. Oxford: Blackwell, 1994:445-7.
4. **Macnicol MF, Nadeem RD, Forness M.** Functional results of surgical treatment in congenital talipes equinovarus (clubfoot): a comparison of outcome measurements. *J Pediatr Orthop B* 2000;9:285-92.

Authors' reply:

Sir,

We thank Mr Macnicol's group for their interest in our paper. We are pleased they agree that an ideal classification system for clubfoot is needed. Our aim in undertaking this study was to identify the most reliable classification system for our clinical use. We were not routinely using a structured system prior to the investigation. Before the trial period all observers familiarised themselves with each system.

Although Diméglio's¹ system initially appears complex, and there were minor discrepancies within the original paper, we did not find it confusing. Having discussed this issue with Professor Diméglio, he confirmed that the scores should be grouped in the manner in which they were applied in our paper (personal communication).

Although some of the data were ordinal, the Catterall system² is nominal, which led us to use the Kappa statistic. While we agree that a 'weighted' kappa statistic may be more useful for ordinal data, it may introduce greater subjectivity. The use of this statistic has led to much heated discussion. We agree that it may help to extend this study to a larger group of observers, but in practice, there is a limit to how many clinicians can assess a baby's foot in clinic simultaneously.

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1. Diméglio A, Bensahel H, Souchet P, Mazeau P, Bonnet F. Classification of clubfoot. *J Pediatr Orthop B* 1995;4:129-36.
2. Catterall A. A method of assessment of the clubfoot deformity. *Clin Orthop* 1991;264:48-53.

Minimal external fixation and calcium-phosphate cement in the treatment of fractures of the tibial plateau

Sir,

We read with interest the paper in the January 2003 issue by Keating, Hajducka and Harper¹ entitled 'Minimal internal fixation and calcium-phosphate cement in the treatment of fractures of the tibial plateau'. We agree that the calcium-phosphate cement is a useful alternative to bone grafting in the treatment of metaphyseal fractures, but minimal internal fixation with screws cannot provide adequate stability, especially in patients with severe comminution. There is the possibility of secondary depression and displacement of the bone fragments.

In our practice after performing calcium-phosphate cementing/bone grafting, we use a minimal invasive technique of fixation with a thin wire Ilizarov frame. The modular Ilizarov frame allows safe, closed reduction and fixation, not only of metaphyseal fragments, but it is also useful if there is an extension of the fracture into the tibial shaft. Thus, the use of the thin-wire, circular external fixation frame is minimally invasive, gives stability in three planes, allows early mobilisation and enhances axial micromovements, which stimulate fracture healing.

The authors mention that additional external fixation was used in the treatment of two patients. We suggest that the wide use of calcium-phosphate cementing in combination with thin-wire Ilizarov external fixation can improve the results of treatment of tibial plateau fractures, especially in patients with high-energy injuries.

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1. Keating JF, Hajducka CL, Harper J. Minimal internal fixation and calcium-phosphate cement in the treatment of fractures of the tibial plateau. *J Bone Joint Surg [Br]* 2003;85-B:68-73.

Authors' reply:

Sir,

I thank Drs Lerner and Horesh for their interest in our paper. There is now evidence in the orthopaedic literature from our own experience and those of other authors that limited internal fixation is an effective method of treating many tibial plateau fractures. I agree that in fractures with a very complex morphology, such as type C bicondylar plateau fractures, this minimal form of internal fixation will generally be inadequate. In our recently published study, we augmented calcium phosphate and minimal internal fixation with transarticular fixation in two patients with bicondylar plateau fractures.

The use of a thin-wire Ilizarov external fixator is becoming a popular alternative to conventional external fixation for management of bicondylar tibial plateau fractures. Although this type of fixation might well be a reasonable choice, there is limited published data to support this method of treatment at present. The use of the Ilizarov frame in itself does not guarantee an anatomical reduction of the fracture, which is clearly an important determinant of final outcome. In addition, with use of wires close to the joint, there is a risk of septic arthritis. I would suggest, therefore, that a recommendation to use Ilizarov external fixation in combination with calcium phosphate cement requires further support from published scientific studies.

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