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.

At the crossroads – neonatal detection of developmental dysplasia of the hip

Sir,
We read with interest the Editorial in the March 2000 issue, moderated by Mr D. H. Jones, entitled ‘At the crossroads - neonatal detection of developmental dysplasia of the hip’. We were surprised to find that none of the contributors mentioned the role of litigation in answering the question ‘What are the current problems?’ with regard to screening for developmental dysplasia of the hip (DDH). Recent settlements for missed DDH have reached six-figure sums and have therefore a considerable impact on the financial costings of any screening programme. Clegg, Bache and Raut have recently published detailed information with regard to the financial justification for universal screening. According to their costings a sum of £100 000 would provide an ultrasonographer to perform universal screening for four years, or alternatively cover the screening of 16 667 births at a cost of £6 per child. This factor could therefore play a significant role in considering the benefits of any screening programme, and should not be overlooked.

J. CHELL, FRCS Orth
J. B. HUNTER, FRCS Orth
Queen’s Medical Centre
Nottingham, UK.


Author’s reply:

Sir,
I thank Messrs Chell and Hunter for their letter. It contains an important message and I recognised the significance of the medicolegal aspect in my comments.

Any system of screening for DDH in the UK will have limitations in effectiveness or implementation with inevitable medicolegal consequences.

J. CHELL, FRCS Orth
Queen’s Medical Centre
Nottingham, UK.


It is only by reassessment of our guidelines for good practice that we will arrive at practicable and fair standards, robust and clear enough to satisfy patients, clinicians and lawyers alike.

D. H. A. JONES, FRCS
The Hospital for Sick Children
London, UK.

Thromboprophylaxis – which treatment for which patient?

Sir,
In his Editorial entitled ‘Thromboprophylaxis - which treatment for which patient?’ in the May 2000 issue, Professor Prentice1 states that, the results of the Pulmonary Embolism Prevention (PEP) trial2 showed that aspirin produced significant reductions in total thromboembolism, total pulmonary embolism (PE), fatal PE and deep-vein thrombosis (DVT), as compared with a placebo. While this is true for the main body of the study, in which patients with fractures of the hip were assessed, it is manifestly untrue for the subgroup of 4000 patients who had replacement arthroplasies. In these latter patients, there were eight cases of PE in those given aspirin and eight in those given placebo; there was also no significant difference in the incidence of DVT (15 cases v 19). In terms of overall mortality, there were nine deaths in the aspirin group and 11 in the placebo group.

In a symposium devoted to prophylaxis after total hip replacement, it is therefore misleading to state that “aspirin reduces the risk of clinical PE and DVT by at least one-third, and of fatal PE by about one-half”, without noting that these figures relate only to patients with fractures of the hip. The PEP study showed no such effectiveness after replacement arthroplasty, and indeed in these patients the only reasonable conclusion is that aspirin is no more effective than placebo as a thromboprophylactic agent.

D. P. THOMAS, MD, DPhil, FRC Path
Oxford, UK.


Author’s reply:

Sir,
The findings of the Pulmonary Embolism Prevention (PEP) trial,1 together with those of the previous meta-analysis of antiplatelet trials, demonstrate clearly that daily aspirin for a few weeks can reduce the risks of deep-vein thrombosis and pulmonary embolism by at least a third in a wide range of patients. It would be inappropriate to suggest, as Dr Thomas does, that the lack of significant result among the relatively small number of patients in the arthroplasty subgroup of the PEP trial, considered on its own, shows that aspirin is not effective among patients undergoing elective arthroplasty. As discussed in my Editorial, the hazard ratio for venous thromboembolism of 0.81 (95% CI 0.47 to 1.42) among elective arthroplasty patients allocated aspirin was entirely
compatible with that of 0.64 (95% CI 0.5 to 0.81) among patients with fracture of the hip (heterogeneity between proportional reductions \(p = 0.04\)). Furthermore, in the previous meta-analysis, antiplatelet therapy produced similar proportional reductions in deep-vein thrombosis and in pulmonary embolism among patients undergoing cold and traumatic orthopaedic surgery.

The problem with using results from particular subgroups considered on their own may be illustrated by analysis of the evidence for heparin thromboprophylaxis. In the subgroup of orthopaedic patients included in the meta-analysis of heparin trials, pulmonary embolism was recorded in 52 patients allocated heparin and in 49 of those allocated placebo. These event rates were clearly not significantly different. Instead, orthopaedic surgeons who use heparin rely, in a large part, on the significant reduction in venous thromboembolism observed with heparin in patients undergoing all types of surgery. In a similar manner, given the strength of the evidence now available, it would be reasonable for orthopaedic surgeons to consider the use of aspirin as a thromboprophylactic agent.

C. R. M. PRENTICE, MD, FRCP
University of Leeds, UK.


The Baumann procedure for fixed contracture of the gastrosoleus in cerebral palsy

Sir,

I read with interest the article by Saraph et al\(^1\) entitled ‘The Baumann procedure for fixed contracture of the gastrosoleus in cerebral palsy’ in the May 2000 issue. The authors are to be congratulated on providing prospective, objective information as to the outcome of this method of gastrocnemius lengthening. However, when they compare the Baumann method with the traditional methods of aponeurotic lengthening described by Vulpian and Strofel\(^1\), Baker\(^2\) and Strayer,\(^3\) they comment adversely on the advisability of operating on the musculotendinous junction because of potential damage to the ‘growth plate’ of the muscle. A careful reading of the paper by Ziv et al\(^4\) gives the relative percentage contributed to longitudinal growth by various parts of the muscle-tendon unit. Although a substantial amount of longitudinal growth comes from the area of the musculotendinous junction, no histological or other evidence has ever been advanced to support the idea of a musculotendinous ‘growth plate’. Until this information is provided, this remains a theoretical rather than an established or practical concept. In any case, the diagrams in Figure 3 illustrate that the site of surgery is in the conjoined gastrosoleus fascia and not at the musculotendinous junction.

Secondly, the comment that “a review of the literature shows that there is higher incidence of recurrence and overlengthening after lengthening of the tendon than the aponeurotic lengthening” cannot go unchallenged. It is difficult to compare the results of aponeurotic muscle lengthening with those of lengthening of tendo Achillis from the retrospective studies which dominate the literature.\(^5\) To date there have been no randomised clinical trials. On balance, however, the literature suggests that the type of cerebral palsy is more important than the type of surgery in determining the risk of recurrent equinus \textit{versus} calcaneus. Children with hemiplegia are more likely to have recurrent equinus, and children with diplegia to have calcaneus.

Given that the study population in this report was made up of children with diplegia, lengthening of tendo Achillis is much more likely to be associated with overlengthening than recurrent equinus, as we have confirmed.\(^6\)

H. K. GRAHAM, MD, FRCS Ed, FRACS
Royal Children’s Hospital
Melbourne, Australia.


Authors’ reply:

Sir,

We thank Professor Kerr Graham for his comments. We agree that no histological evidence has been presented in the paper by Ziv et al,\(^1\) but they did report that 45% of the muscle growth occurs at the musculotendinous junction and it cannot be refuted that a major part of the growth takes place in this area.

It is true that the aponeurotic lengthening procedures have been described at the conjoined gastrosoleus fascia and not at the musculotendinous junction, but the proximity to this junction presents a possible risk of surgical injury to the junction as mentioned in the legend to Figure 3 in our paper.

In reviewing the three studies in which different methods of triceps lengthening are compared, two had a mixed patient population of hemiplegics and diplegics and the outcome was not evaluated separately for both.\(^4\) Sharrard and Bernstein, however, found a higher recurrence after lengthening of tendo Achillis in diplegics (33%) as compared with hemiplegics (27%). The recurrence after aponeurotic lengthening was higher in hemiplegics than diplegics in their series.

We agree that the recurrence/overlengthening rates in diplegic and hemiplegic children will remain controversial until randomised clinical trials with a comparable patient population and type of involvement are presented. Our observations are based on the literature which we reviewed, as shown in Table IV. Since these papers have, however, presented mixed groups of hemiplegics and diplegics, it is possible that the conclusions from our literature review may not hold true for an isolated group of hemiplegic or diplegic patients.

The anatomical and mechanical merits of the Baumann procedure remain theoretical.\(^5\) Long-term comparative studies with different methods of lengthening (aponeurotic, tendon lengthen-
Implantation of a soft-tissue expander before operation for club foot in children

Sir,

We read with interest the article in the May 1999 issue by Roposch, Steinwender and Linhart entitled ‘Implantation of a soft-tissue expander before operation for club foot in children’.

In the introduction the authors state that primary skin closure after surgery for club foot can be difficult, and they therefore suggest the use of soft-tissue expanders before surgery to facilitate this. We would like to raise two points in relation to this.

Experience with the Cincinnati incision has shown that it is not necessary to achieve primary wound closure, as partially closed wounds heal well by secondary intention, with cosmetically acceptable scars and no significant complications.

In relapsed and neglected club feet, the Ilizarov system facilitates adequate correction without the need for extensive soft-tissue dissection and the requirement for soft-tissue expansion.

S. JONES, FRCS
F. ALL, FRCS
J. FERNANDEZ, FRCS Orth
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Sheffield, UK.

Authors’ reply:

Sir,

We thank Messrs Jones, Ali and Fernandez for their comments.

We stated that “primary wound closure can be difficult following surgery for club foot”. Different methods of addressing this problem have been described. Secondary wound healing is an option but involves prolonged rehabilitation. The use of an external fixator for the correction of relapsed club foot is an alternative. Nevertheless, not every patient is suitable for correction with the Ilizarov apparatus and not every surgeon is familiar with this demanding technique. We presented the method which we described as an alternative to secondary wound closure or the use of the Ilizarov apparatus.

A. ROPOSCH, MD
G. STEINWENDER, MD
W. E. LINHART, MD
Karls Franzens University of Graz
Austria.

Fixation of fractures of the shaft of the humerus by dynamic compression plate or intramedullary nail

Sir,

I read with interest the article entitled ‘Fixation of fractures of the shaft of the humerus by dynamic compression plate or intramedullary nail’ by McCormack et al in the April 2000 issue in which the authors compare fixation of fractures of the shaft of the humerus by dynamic compression plate (DCP) with that using the Russell-Taylor intramedullary nail. Their findings suggest that “open reduction and internal fixation with DCP remains the best treatment for unstable fractures of the shaft of the humerus. Fixation by IMN may be indicated for specific situations, but is technically more demanding and has a higher rate of complications”.

In a similar study published more or less at the same time Chapman et al reached different conclusions stating that “both IMN and DCP plates provide predictable methods for achieving fracture stabilisation and ultimate healing”.

I would therefore like to emphasise how difficult and disappointing it is to perform scientific studies simply based on clinical findings. It is depressing to see how two excellent and similar papers reach contradictory conclusions. The number of variables is so high in clinical practice that most of the articles published in the best orthopaedic journals are controversial.

It is absolutely necessary to stimulate the realisation of controlled and multicentre studies to try to clarify such tremendous dilemmas.

E. C. RODRIGUEZ-MERCHEAN, MD, PhD
La Paz University Hospital
Madrid, Spain.

Authors’ reply:

Sir,

Dr Rodriguez-Merchan brings up a very relevant point, that small clinical studies, even when they have a good experimental design, can produce conflicting results. One of the major challenges for clinical research is to obtain adequate numbers of patients. This is the reason why we used a multicentred design, which not only can produce conflicting results. The number of patients is so high in clinical practice that most of the articles published in the best orthopaedic journals are controversial.

It is absolutely necessary to stimulate the realisation of controlled and multicentre studies to try to clarify such tremendous dilemmas.

E. C. RODRIGUEZ-MERCHEAN, MD, PhD
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conclusions of our study. This was recently confirmed in a presentation by Bhandari, McKee and Schemitsch.\(^1\) They performed a meta-analysis of the four prospective, randomised trials with intramedullary nails. Three involved reamed, locked intramedullary nails but they also included Dr Rodriguez-Merchan’s study with an unreamed, unlocked, intramedullary device.

The pooled data (with 195 patients) showed that there was a significant difference in the rate of reoperation (risk reduction 62%, \(p = 0.02\)) and in the incidence of shoulder problems (risk reduction 78%, \(p = 0.004\)). When the confidence intervals for these primary outcomes are considered, all of the studies are similar, with a trend towards plates being better than intramedullary devices. It is not until all of the data are pooled, however, that the statistics become significant. Dr Bhandari and his co-authors will almost certainly be publishing the study and their conclusion is significant in that they state that the reduction of risk of secondary surgery is large enough to indicate that future trials in this area may be a poor investment of resources. There was no difference in the rate of nonunion, infection or injury to the radial nerve between the groups with either plates or nails in the published studies.

This is a good example of the problems which may be encountered in small clinical studies and indicates the value of pooling data and the need for power calculations when studies report no statistical difference between treatment groups.

R. G. McCORMACK, MD
Royal Columbian Hospital
New Westminster, Canada.


Wound infection in hip and knee arthroplasty

Sir,

I read with interest the article in the May 2000 issue by Gaine et al.\(^1\) entitled ‘Wound infection in hip and knee arthroplasty’. Recent media attention to rising rates of nosocomial infection in British hospitals makes the paper most topical. Although the article was well presented and discussed, I would seriously question the basic principles of cross-infection.

It seems that we have forgotten the basic principles of cross-infection.

J. HALL, FRCS
Harley Street, London, UK.


Authors’ reply:

Sir,

We thank Mr Hall for his comments. In this trial, hand-washing and glove-donning were conducted before removing the dressings and new dressings were subsequently applied. The management of the wound after arthroplasty varies greatly among different surgeons. Usually the wound is covered with a dry, adhesive dressing for 7 to 14 days or until the sutures are removed. Nursing staff will change dressings over a discharging wound at least once a day. The question of whether exposing wounds for inspection or change of dressing leads to increased rates of infection has not been answered by any controlled trial in the literature. The risk of cross-infection is well documented, however, and wound contamination will occur if proper sterile precautions such as hand washing and the use of gloves are not followed. Unfortunately, it is often the medical rather than the nursing staff who are guilty of lifting occlusive dressings to inspect wounds on ward rounds without proper aseptic precautions.

The importance of the timing of exposure of the wound is debatable. A recent report\(^1\) on 100 foot and ankle wounds exposed after four days, with normal bathing allowed, found no increase in wound infection. Study of animal wounds has found that a staphylococcus inoculum multiplied significantly faster in occluded wounds compared with exposed wounds.\(^2\)

Airborne contamination is potentially a greater risk on a general orthopaedic ward, especially if infective cases are present.

Airborne bacterial infection during this prospective trial will inevitably have caused some of the infections. More frequent exposure of inflamed wounds will have added to this risk.

In the 1950s, the late Professor Robin Pilcher, at University College Hospital, had to resort to encasing his operation wounds in plaster-of-Paris to convince staff that routine exposure of all wounds for inspection on daily ward rounds was both dangerous and unnecessary. The immediate reduction in wound sepsis after his radical action served to vindicate his beliefs fully.

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