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.

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The Weil osteotomy: a seven-year follow-up

Sir,

We read with interest the article by Hofstaetter et al1 in the November 2005 issue entitled “The Weil osteotomy: a seven-year follow-up”. We congratulate the authors for achieving and sharing the good results of this procedure. We believe that the Weil osteotomy may not address the exact cause of pain in dislocated metatarsophalangeal joints (MTPJ) for the following reasons: this osteotomy essentially shifts the metatarsal head proximally, in MTPJ dislocation, the plantar plates are subluxed dorsally2 which leads to metatarsalgia; the plantar plates are attached to the proximal phalanx; in the operation, the insertion of the plantar plates to the proximal phalanx is not addressed and hence they remain dorsally subluxed, which may lead to persistent metatarsalgia under the shifted metatarsal head. We would be interested to know the authors’ view on this.

The Weil osteotomy also leads to a post-operative increase in the coronal height of the forefoot, causing painful swelling.3 This has been confirmed by a recent cadaver study showing no decrease in the load at the metatarsal head after a Weil osteotomy but an increase in plantar pressure.3 On the other hand there are studies showing decreased load and reduction of the dislocated MTPJ with good results.4,5 We believe the Weil osteotomy is a good procedure for the metatarsalgia caused by dislocated MTPJs. In this procedure, we believe the Weil osteotomy may not be compared with our results as rheumatoid feet were excluded from the study and the treatment of dislocated lesser metatarsophalangeal joints: good outcome in 21 patients with 42 osteotomies.


Author’s reply:

Sir,

We were interested to read the comments from Messrs Ramisetty and Greiss. Clinical results of the Weil osteotomy with short-term and long-term results prove its value. A significant reduction in pain, disappearance of plantar calllosities, increase of patient’s satisfaction rate and walking ability are all reported.1,4 In their letter they stated that in the Weil osteotomy, the insertion of the plantar plates to the proximal phalanx is not dealt with. The plate part of the joint capsule has a substantial attachment to the proximal phalanx and the plantar fascia but, except for the collateral ligaments, it is without substantial fibrous attachment to the metatarsal head.5,6 We are convinced, as described by Hicks6 and Scheck,7 that rupture or elongation of the plantar plate in senile feet occurs where the plantar aponeurosis fuses with the plantar plate. With the Stainsby procedure (modified Keller’s procedure)2,6 you sacrifice the remaining intact attachment of the plate to the proximal phalanx and the metatarsophalangeal joint (MTPJ) when excising the proximal 3/4 of the proximal phalanx. Moreover, the insertions of the interossei and the lumbricals at the base of the proximal phalanx hold the proximal phalanx in its neutral position.10,11 With the Stainsby procedure, the insertion of the muscles is removed and there is no way to obtain flexion in the MTPJ. In theory, the latter is possible with the Weil osteotomy and the joint remains intact.

They also state that with their method it is possible to release and replace the plantar plate. However, through the shortening effect of the Weil osteotomy, the plate becomes looser and releasing and replacing the plate under the metatarsal head is possible, although we doubt the long lasting effect of this manoeuvre. Scarring of the plate occurs. Nevertheless, the reversed windlass mechanism of the weakened plantar aponeurosis will not be strong enough to prevent a post-operative extension contracture, floating or stiff toes.1,4 Myerson and Jung12 showed in their study with second toe instability in 64 feet that the MTPJ, even after a flexor to extensor transfer, remains unstable. To date, the longest Stainsby procedure follow-up study9 had a follow-up period of three years and four months in 69 feet. The indication in this study was severe claw toes in rheumatoid feet and cannot be compared with our results as rheumatoid feet were excluded from our study. Unfortunately there are no clinical studies of the Stainsby procedure spaced throughout, signed by all authors and fully referenced. The edited version will be returned for approval before publication.

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procedure related to patient numbers/demographics, nor statistical evaluation.8,9,13

The goal of the Weil osteotomy is firstly to decompress the MTPJ and secondly to alter load transmission through the forefoot by shifting the plantar fragment proximal to the area of the lesion where thicker and more compliant soft tissue is still present.14 However, instability of the MTPJ of the lesser toes by the rupture of the plantar plate is, and continues to be, a challenging problem.12 A randomised, controlled trial, to compare the Weil osteotomy with the Stainsby procedure would be of value.

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The incidence of deep prosthetic infections in a specialist orthopaedic hospital: a 15-year prospective study

Sir,

We read with interest the article by Phillips et al1 in the July 2006 issue entitled “The incidence of deep prosthetic infections in a specialist orthopaedic hospital: a 15-year prospective survey.” This large series of 10 735 patients undergoing primary hip and knee replacements is a landmark study and appears to show very low infection rates. It does, however, leave questions unanswered. We feel it is difficult to give definitive infection rates having only reviewed those patients on the hospital’s infection register. There may be patients on the hospital’s infection register. There may be patients who were admitted with suspected infection treated either in the community or other hospitals. Without reviewing all patient notes it is impossible to know whether this has occurred. Previous studies have used questionnaires to identify patients with suspected infection. We feel this is more likely to be accurate.2,3

In addition, there is no indication of the number of patients with suspected infection with negative microbiology. Presumably some may have been treated with antibiotics on suspicion of infection, potentially hampering accurate microbiological cultures. It would be interesting to know how many patients had been treated either surgically or with antibiotics.

Of note is that the mention of patients with pre-existing diabetes or previous intra-articular steroid injection. There is evidence to suggest that both of these factors may increase the risk of infection,4,5 yet it is unclear whether there is a relative increase in this study.

Over the years there was no significant difference in yearly infection rate despite the significant changes in practice - reduced length of stay, patient admission on the day of surgery, etc. Given the time period over which the study takes place, it would be beneficial to know whether there have been changes in surgical protocol, i.e. the use of exhaust gowns, prep, laminar-flow theatres, etc.

We feel it would be inappropriate to quote this as an absolute infection risk to patients about to undergo arthroplasty given the unanswered questions.

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Author’s reply:

Sir,

We would like to thank Messrs Chitre and Sadiq for their interest in our paper.

One of the problems with any series dealing with deep infection in patients with joint replacements is the definition one should use to confirm the infection. Unfortunately there is still no absolute method which is 100% reliable, and every surgeon will have come across cases where he or she was convinced there was infection but repeated cultures have all proved negative, and other cases where there are absolutely no stigmata of infection where one culture has grown a low grade organism of uncertain significance.

When we started this study in 1987 our Control of Infection Group made a decision as to how we would define deep infection and we adhered to that decision for the next 15 years. We accept that definitions have changed slightly during this time and this is discussed in our paper.

We are well aware of the recent studies by Blom et al1,2 in which they sent patients a questionnaire asking if they had any experience of infection. We were, in fact, so impressed with the papers by Blom and colleagues that we have done an identical study in Birmingham, comparing it with the results of our Control of Infection Group to see which is more accurate. The results of that study are in the final stages of preparation, but we were fascinated to find that a number of patients were adamant that they had an infection when there was absolutely no mention of infection in any of the in-patient or out-
patient hospital records. We suspect that the incidence of superficial infection or redness around the wound is probably under-reported in hospitals and possibly over-reported by patients. We believe, however, that the incidence of deep infection is likely to be quite accurate in our series as this will almost always result in the patient returning to hospital. We accept that the longer the period after the operation, the more likely it is that the patient will have moved away and been referred to another hospital, but given that our hospital is a regional referral centre, we think that we will still have kept most of the patients who did develop deep infection.

This study did not analyse the risk factors by taking a large group of patients and looking at the risks of those who had diabetes, previous surgery or previous steroid injections. This would be totally outside the remit and would require review of medical records of all 10,735 sets of notes, which is not something that we plan to do in the near future!

The point that Messrs Chitre and Sadiq make about the lack of change in the yearly infection rate is well taken. Over the course of the study, the main changes which took place were the increase in the number of operating theatres from two to eight, but all of them had laminar air flow and some the facility for use of exhaust gowns, although the latter were not used routinely by most surgeons. The main change in practice over the 15-year period of the study was the advent of antibiotic-laden cement. Given the low rate of infection during this period, it would be extremely difficult to prove that any particular factor was responsible for changing the rate of infection from one which has been commendably low throughout.

I am afraid, therefore, that whilst we accept some of the criticisms of Messrs Chitre and Sadiq, we disagree with their comments that our series does not represent the true risk of infection of patients undergoing arthroplasty.

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