# EXPERT OPINION ON PUBLISHED ARTICLES

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## Treatment of infected total hip arthroplasty with a two-stage re-implantation protocol

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T his study supports a two-stage re-implantation protocol for peri-prosthetic hip infections.

Infection occurs in from 0.3 to 2.9% of total hip arthroplasty cases. Treatment options are:

- · a two-stage revision
- · a one-stage revision
- · antibiotics suppression
- · resection arthroplasty or
- debridement and liner exchange.

The purpose of this study was two-fold, namely to determine whether a two-stage revision is still effective with the new types of infections encountered with hip arthroplasty. The second purpose was to determine which factors contribute to the success in infection eradication. The authors' protocol for diagnosis and treatment of infection in THA is as follows: aspiration of the hip joint, at least two weeks after antibiotic treatment was stopped. A clinical examination and laboratory evaluation follow. If an acute infection is diagnosed all implant materials and retained cement are removed and an antibiotics-impregnated cement spacer placed in position. Five intra-operative blood cultures as well as tissue are obtained for immediate analysis. Either tobramycin at 2.4 g per package of bone cement or vancomycin at 1 g per package of bone cement is used, depending on the sensitivity of the organism obtained with the aspiration. Intravenous antibiotics are continued for at least six weeks. The authors make use of an infectious-disease specialist and they administer dosages of antibiotics that are sufficient to obtain a postpeak serum bactericidal titre of at least 1:8. After six weeks, if there are no clinical symptoms or signs of infection and the laboratory values are normal, the second stage or so-called 're-implantation surgery' is implemented. If a cemented prosthesis is used, 1.2 g of tobramycin per packet of cement and/or 500 mg of vancomycin per packet of cement is used. During the re-implantation, tissue is sent for immediate analysis and if it is still infected another debridement and antibiotics spacer will be used and the re-implantation postponed for four weeks. If a patient presented with overt clinical symptoms of infection and positive aspiration, the pre-operative ESR was not always done. C-reactive protein analysis is rarely requested at the authors' institution.

In a previous study done between 1985 and 1988 on infections that developed in THA, the success rate of eradication of infection with this protocol was 90.6%. In the present study, the eradication rate was 95.2%, proving that a two-stage protocol is still effective with organisms with new resistance.

Contributing to the success of a two-stage versus a singlestage protocol is probably the fact that debridement is done twice, i.e. when the prosthesis is removed and again during re-implantation and the use of an antibiotics-impregnated spacer.

The weaknesses of the study are the following:

- It is a retrospective study.
- The follow-up on all the patients was inadequate.
- It was done in a small population, making it difficult to get to statistically significant figures.

The importance of this article relates to the following:

- Aspiration of the hip joint prior to revision surgery is important to determine the organism's sensitivity to antibiotics in order to use the appropriate antibiotics in the bone-cement spacer.
- 2. The use of an antibiotics-impregnated cement spacer is important.
- The authors rely on frozen-tissue evaluation at the time of re-implantation (second stage) to determine whether infection is still present or successfully eradicated.
- 4. Even in the case of infections with 'newer' organisms (MRSA, etc.) a two-stage protocol is still as effective as it was in the mid-1980s.

The authors make use of an infectious-disease specialist and they administer dosages of antibiotics that are sufficient to obtain a post-peak serum bactericidal titre of at least 1:8

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## Open reduction and internal fixation versus primary arthrodesis for Lisfranc injuries: A prospective randomized study

Jeffrey A Henning, Clifford B Jones, et al Foot & Ankle International October 2009; 30(10): 913

The debate about whether internal fixation or primary arthrodesis should be performed in these cases continues and this is a well-planned article to try and give some answers.

The authors collected patients over a five-year period with strict criteria for inclusion and exclusion. These included that the injury had to be less than three months old, and no major interarticular fracture pattern or previous foot trauma was accepted. Prior foot infection, surgery or pathology was also a cause for exclusion.

It was accepted that closed reduction with percutaneous fixation would not allow an anatomical and rigid fixation.

Of the 185 patients treated between March 2002 and August 2008, 40 patients met the inclusion criteria. This was 22% of the pool. They were randomised, and nine males and five females underwent open reduction and internal fixation with screw fixation without compression.

If a cuboid fracture was present this was treated by open reduction and internal fixation.

Eighteen patients were randomised into a primary arthrodesis group. They were treated by internal fixation and the arthrodesis fixed with lag screws.

Postoperatively both groups were immobilised until there was bony union and they were non-weight-bearing for this time.

The follow-up was done at 6 months, 12 months, 24 months and the final one after 50 months.

Hardware removal was far more common in the open reduction group because screws had to be removed to allow mobility of the fourth and fifth rays, but also the other rays.

At follow-up most of them were back at their original work with very small numbers in each group unable to do so.

Seventy-nine per cent of the open reduction group had to have the hardware removed, but this was only necessary in 17% of the arthrodesis group.

This greatly increased the number of procedures carried out in the open reduction group. This is the down side of the open reduction.

The result after 50+ months was basically the same in the two groups.

The weakness of this trial is that the follow-up is too short, as we know that complications of Lisfranc injuries probably take 10 to 20 years to start causing severe problems.

On the other hand, it proves that in the medium term the results are the same for both procedures.

## Patient-reported outcome at 62 to 67 years of age in 83 patients treated for congenital clubfoot

H Wallander, S Larsson, et al Journal of Bone & Joint Surgery October 2009; 91-b(10): 1316

This article could only have been written in Sweden where the ability to follow up patients is unequalled.

These patients were all treated in the early 1940s in southern Sweden for club-foot.

They were reviewed by giving them questionnaires, namely the so-called 'short form' (SF) -36 and the EQ-5D.

There was also the Foot and Ankle score of the American Academy of Orthopedic Surgeons.

What is interesting is that the male patients on the (SF) -36 appear to score better than their peers matched for age. On all the other scores the patients did not do as well as their peers.

The patients were initially treated conservatively but only 15 of the patients had only non-operative treatment and 106 patients had surgery.

In the 83 patients, 121 club feet were treated.

The average number of operations per patient was 2.1.

Only one patient had secondary surgery after the age of 30 years and this was a talo-navicular fusion.

Looking at this article one is struck by the fact that conservative treatment with early surgery led to acceptable results in the majority of patients so that most of them were working with few complaints.

This article has set a gold standard against which all other long-term follow-ups of clubfeet will have to be assessed.

Reviewer: Dr HM Snyckers Senior Consultant Dept of Orthopaedic Surgery University of Pretoria

### The vertebroplasty controversy

#### Introduction

Two recent articles published in the *New England Journal of Medicine* have put the proverbial cat among the pigeons in the spinal community. Both the articles report results of investigation into vertebroplasty. Vertebroplasty entails the percutaneous injection of polymethylmethacrylate (PMMA) into the affected vertebral body and has been advocated as a treatment for painful osteoporotic vertebral fractures. Many previous studies have shown that there is an immediate and sustained reduction in pain after this procedure is performed. Randomised trials have been done that have confirmed the efficacy of this procedure. None of the previous studies have been randomised double-blind controlled studies with a sham control group. The procedure has become very popular in treating these fractures and has been very positively received by treating physicians. I have found this procedure to be very successful in treating my patients with vertebral compression fractures, as have many of my colleagues. For this reason, the results of the articles below have been met with surprise and disbelief.

#### A randomized trial of vertebroplasty for painful osteoporotic vertebral fractures

Buchbinder R, Osborne RH, Ebeling PR, Wark JD, Mitchell P, Wriedt C, et al. N Engl J Med. 2009; **361**(6): 557-68

In this article, 71 patients with back pain and osteoporotic vertebral fractures were randomised into two groups. The patients from both groups were taken to theatre and prepared in the same way. They were then randomised to a sham group or a cement group. Both groups would have the needles inserted according to the standard vertebroplasty technique. Cement would be mixed in both groups so the patient would smell it, but in only the one group of patients would the cement be injected into the vertebra. The patient would not know whether they had received the cement or not.

The patient would then be followed up at 1 week, 1 month, 3 months and 6 months, and be assessed for their pain relief and functional outcome according to internationally accepted outcome scores.

There was no statistically significant difference found in the outcomes between the two groups at the follow-up periods.

### A randomized trial of vertebroplasty for osteoporotic spinal fractures

Kallmes DF, Comstock BA, Heagerty PJ, Turner JA, Wilson DJ, Diamond TH, *et al. N Engl J Med.* 2009; **361**(6): 569-79

The second article, published in the same journal, concerned a series in which there were 131 patients. The method of investigation of this series was similar to the other series. The patients were also randomised into two groups and either received an injection of cement or a sham procedure.

The results were also similar to the other series except at 1 month, there was a trend towards a higher rate of clinically meaningful improvement in pain in the vertebroplasty group (64% vs 48%, P=0.06) but this was not statistically significant. This could be due to the small sample size. At 3 months there was also a higher crossover rate in the control group than in the vertebroplasty group (43% vs 12%, P<0.001).

This means that a very high percentage of the sham groups elected to eventually have the other procedure. This was statistically significant. Sadly their results after having the procedure were not significantly better but this could be due to the 1 month delay in treatment.

#### **Discussion**

The North American Spine Society published an answer to the above controversy on its website, www.spine.org, entitled: "North American Spine Society: Newly Released Vertebroplasty RCTs: A Tale of Two Trials".

In this response they attempt to answer some of the questions that the above articles have asked. The reason for their critical evaluation of the above studies is summed up in this statement, taken from the article:

Moreover, for any physician who has performed vertebral augmentation procedures for osteoporotic compression fractures, experience has indicated that patients have dramatic pain relief, often within hours of the intervention. Some of the authors have personally seen these seemingly miraculous cases in which a bed-bound elderly person has had one or two vertebrae augmented after which they became nearly pain-free and ambulatory. The evidence and experience up to the publication of the studies by Buchbinder *et al.* and Kallmes *et al.* have been overwhelmingly positive. Spine care providers are now, however, faced with a large chasm between these previous data and experiences and the latest, highest quality data.

It seems that the only possible bias in these studies could be found in the inclusion criteria and that the majority of patients excluded from the study were patients who did not want to be part of a study that had the possibility of their receiving a sham procedure. It is possible that this group of patients were those patients who had more severe pain and would have profited more from the procedure. It would have been interesting to see the results of the procedures done on this group of patients and compare it to the study group.

There was also a concern about the different ages of the compression fractures as well as the criteria used to decide if the fracture was acute or not. It was felt that the cut-off point of 6 months for an acute fracture was too long.

In both the studies the exact origin of the back pain was not assessed. Back pain, due to causes other than compression fractures, are very prevalent in this age group of patients. It is possible that the patients may have had other reasons for back pain than the compression fractures.

# An international multicenter randomized comparison of balloon kyphoplasty and non-surgical management in patients with acute vertebral body compression fractures

D Warlaw, S Cummings, J van Meirhaeghe, et al The Spine Journal October 2009; 9(10S)

This recent randomised study on kyphoplasty showed a statistically significant improvement in pain and quality of life in the group that had the kyphoplasty when compared to the control. There was no sham procedure done in this series so it is not a double-blind study. Also the technique used is kyphoplasty which differs from vertebroplasty in that

the vertebral height is restored using bone tamps before the cement is injected. Whether this makes a difference is unclear.

The data from the two articles has to be taken seriously and considered carefully and thoughtfully. It is obvious from this data that if the indications for this procedure are not carefully followed it is no better than a placebo with potential serious complications. This is thus not a panacea for all vertebral compression fractures. The specific indications will be refined as more literature becomes available on the procedure. It is the responsibility of all physicians involved in this procedure to avail themselves of the latest scientific knowledge on the subject. This will enable them to identify the patient who will benefit the most from the procedure.

The data from the two articles has to be taken seriously and considered carefully and thoughtfully. This is not a panacea for all vertebral compression fractures

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## Current concepts in metatarsal osteotomies A remedy for metatarsalgia

Kendal D Hamilton MD, John G Anderson MD and Donald R Bohay MD Directors, Grand Rapids Orthopaedic Foot and Ankle Fellowship *Techniques in Foot and Ankle Surgery,* June 2009; **8**(2): 77-84

Metatrsalgia is briefly discussed including the definition, classification and aetiology. Aetiologies include disturbances in foot biomechanics, systemic conditions (arthritides) and conditions unrelated to weight bearing (neurologic, vascular).

The history of metatarsal osteotomies for biomechanical overload is discussed noting more than 20 variations in literature and with results reported between 57% and 100% success rate. The above goals are achieved by dorsally elevating and metatarsal shortening osteotomies (proximal, shaft and distal) with rigid internal fixation. This article focuses on the shortening osteotomies including Weil, Helal, midshaft segmental and asymmetric distal V-osteotomies.

Under the heading of 'Indications and contraindications' I would like to highlight their opinion of pursuing disease-specific therapy (for instance equines or dysfunctional first ray), and in general utilising conservative measures as the first line of treatment. The distinction between symptoms during the stance (usually elevation osteotomies) and pathologic propulsive phases (shortening osteotomies) are a practical guideline. Absolute contraindications include the usual local infection and vascular insufficiency but also very importantly a neuropathy.

Pre-operative planning is based on the understanding of disease pathophysiology and the article concentrates on the role of X-rays here.

Under 'Technique' a short description is given of the Weil, Helal, proximal V-osteotomy, the distal V-osteotomy and the midshaft osteotomy. These procedures are not without risk of complications, and non-union, hardware problems, transfer metatarsalgia and floating toes are mentioned. The summary of some of the literature results again highlights the risks of these procedures, but also the fact that good results can be obtained.

Under 'Possible concerns and future of the technique' the comment is rightly made that these procedures can be technically demanding. First ray stabilisation procedures, gastrocnemius recession to address equinus contractures, hallux valgus corrections and hammertoe realignments may influence results (may sometimes be all that is necessary in my opinion).

#### Shortcomings

This is a huge and important topic that can probably only be covered fully in a book format. For instance, the preoperative planning only covers some aspects of radiology. The techniques include some of the osteotomies and only give short descriptions of such. Important technical options, for instance removing a sliver of bone with a Wiel osteotomy, are not covered here. There are however some important principles that come out of this article that make it worthwhile reading.