zone within the PTFJ and lateral to the medial margin of the PTFJ. The authors proposed their own classification of 'hinge zone' and rather elegantly demonstrate that the position of the hinge as described by their classification is directly related to the actual risk of fracture.

Uncemented *versus* cemented total knee arthroplasty under the radiostereometric analysis spotlight

■ Total knee arthroplasty (TKA) is a slightly different beast, from the fixation perspective, to total hip arthroplasty. Knee prostheses are almost universally cemented, as there have been difficulties with the fixation of uncemented prostheses, particularly in the tibial baseplate where early loosening has plagued a number of uncemented designs. We were delighted to see this mid-term follow-up (five years) from **Hässleholm (Sweden)** of a randomized controlled trial (RCT) using radiostereometric analysis (RSA) of 60 patients randomized to either a cemented or peri-apatite-coated implant.[7] The outcome of interest was component migration as measured by the RSA analysis. There was a single revision (in the cemented group) for ligament instability. The results themselves were fascinating. The authors identified a higher rate of component migration in the peri-apatite group at five years' follow-up. However, for the most part, this was occurring in the first three months. After this, the cemented components had a significantly higher rate of migration. These two different migration patterns – subsidence then stabilization in the uncemented group, but continuous migration in the cemented group – is an important difference between the two methods. Overall, the migration in both groups was rather small (0.62 mm at five years in the cemented component *versus* 0.97 mm in the uncemented component). This paper essentially reported the results of a five-year randomized controlled trial showing no difference in outcomes, using uncemented *versus* cemented total knee arthroplasties. An important point to remember when interpreting the results of this study is that the manufacturer (Stryker, Mahwah, New Jersey) has subsequently changed their tibial baseplate, so the relevance of this study to their implant is not as straightforward as perhaps they might have you believe.

Preoperative vitamin D periprosthetic joint infection

X-ref

■ Treatment of periprosthetic joint infection (PJI) is, of course, a perennial cause of angst for the orthopaedic arthroplasty surgeon, as is any musculoskeletal infection. The difficulty with treatment of these conditions is the lack of efficacy for most antibiotics, either due to reduced bioavailability in the musculoskeletal tissue, or the effects of biofilm formation. When antibiotics are not effective, the surgeon must rely on debridement and local delivery of antibiotics to treat musculoskeletal infection. Aside from the traditional antibiotics, there are some potential adjuncts that might aid matters, and this paper from **California (USA)** caught our eye.[8] It is a basic science paper using a mouse model of periprosthetic infection and the authors sought to determine whether supplementation of 25-hydroxyvitamin D would improve outcomes of surgical treatment for PJI. The mouse model used (lys-EGFP) was modified to include fluorescent neutrophils. A cohort of 60 mice were fed either a vitamin D_3-sufficient (n=20) or vitamin D_3-deficient (n=40) diet for six weeks, after which half of the deficient mice (n=20) were 'rescued' with one intraperitoneal dose three days prior to surgery. The surgical model included insertion of a stainless-steel implant into the knee joint and simultaneous contamination with a bioluminescent strain of *Staphylococcus aureus* culture. Outcomes were assessed *in vivo* with bacterial burden and neutrophil migration monitored prior to killing the mice at day 21. Essentially, this paper demonstrates that vitamin D deficiency does contribute to post-operative infection in this model with greater bacterial bioluminescence and neutrophil fluorescence. This also translated into higher colony-forming unit (CFU) counts from the joint and implant-associated tissue. The 'rescue' group had lower rates of neutrophil infiltration and lower bacterial counts than the deficient group. This paper is interesting in that it further highlights the importance of vitamin D levels, and also questions whether preoperative supplementation could improve outcomes in both primary joint arthroplasty and revision for infection.

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Foot & Ankle

***X-ref** For other Roundups in this issue that cross-reference with Foot & Ankle see: Knee Roundup 8*

Complications following ankle arthrodesis *versus* ankle arthroplasty

■ The comparison between arthrodesis and arthroplasty of the ankle continues. There is now ample research comparing the functional outcomes of the two, although the large randomized trials are ongoing and have yet to report. The state of play for most mainstream surgeons is that in selected patients who do not have too much deformity and whose functional demands are not too high, ankle arthroplasty can be considered. In the other patients who are young, are high-demand, or have significant preoperative deformity or bone loss, the majority of surgeons favour ankle arthrodesis. One of the things that does steer decision-making in conditions such as ankle arthritis, where there are two potentially effective treatments, is the side-effect and complication profile. This study from **Charlotte, North Carolina (USA)** sets out to

describe the complication profiles of these two treatments in greater detail.[1] The authors sought to compare national rates of perioperative complications in the United States between a statistically matched cohort of patients who underwent either an ankle arthrodesis or arthroplasty. The authors identified 4192 ankle arthroplasty patients and compared them with 16 278 arthrodesis patients who underwent surgery during the same time period. The inpatient sample data were used to establish major and minor complications, and mortality was also recorded. There is a risk of selection bias and, to counter this, the cohorts were matched for age, sex, race, surgery year, hospital type, comorbidities, adjunctive procedures, and surgical indication. This resulted in a statistically matched cohort of 1574 patients. There was at least one major complication in 8.5% of the arthrodesis patients (n=134) compared with 5.3% (n=84) of the arthroplasty group, and this high rate of complication in the arthrodesis group was also reflected in minor complications (4.7% *vs* 5.9%). Once the authors had undertaken case-mix adjustment, it appeared that ankle arthrodesis was 1.8 times more likely to be followed by a major complication than ankle arthroplasty was, although the minor complication rate was 29% lower in the former group. This paper will do a lot to set the mind at rest with regard to perioperative complications in the total ankle arthroplasty group, which, in the largest study of its type, have been shown to be substantially fewer than for patients undergoing arthrodesis. This allays concerns about high rates of intraoperative and postoperative complications, which have been mostly reduced through the development of newer prostheses and refined surgical techniques. The remaining questions, of course, are functional, and we wonder what sort of longevity can be expected from the newer generation of ankle prosthesis.

Does supramalleolar osteotomy work?

One of the difficulties with joint preservation surgery for arthritis is establishing whether it actually has the desired effect. That said, realignment osteotomies of the knee and ankle are regaining widespread popularity while unicompartmental knee arthroplasty and ankle arthroplasty have not fully lived up to their early promise in terms of longevity in the younger population. However, there is little in the way of long-term outcome data for osteotomies and, specifically, there is little evidence to support the hypothesis that realignment results in a reduction in the progression of arthrosis. We were delighted to come across this report from **Gyeonggi-do (South Korea)**, where the authors have undertaken supramalleolar osteotomy with a relook arthroscopy, radiological films, and clinical follow-up at an interval of at least a year.[2] Their series of 22 patients was reported including Visual Analogue Scale (VAS) pain scores and American Orthopaedic Foot and Ankle Society (AOFAS) ankle-hindfoot scores, along with the usual gamut of radiological measures. The arthroscopic findings were reported using the Outerbridge classification, and essentially complete results were available for 14 patients. From a clinical perspective, there was a significant improvement in outcome in both the AOFAS scores (improving from 60.7 to 87.1) and VAS pain scale (improving from 6.5 to 1.1). Perhaps most crucially, in the 14 patients who underwent second look ankle arthroscopy at one year, cartilage regeneration of the medial compartment of the tibiotalar joint was observed in 12 patients (85%), and there were no observed disease progressions. Although ultimately a report of a small number of patients, the results here are very encouraging. The authors present improved outcomes in a difficult-to-treat condition, and also report that perhaps, unlike in other joints, an offloading realignment osteotomy can result in improvement in the cartilage condition, and appears to prevent degeneration in this small series. Clearly, more work is needed here. Osteotomy is a technically challenging procedure with low tolerance for surgical errors, but offers a potentially robust solution undertaken without the need for fusion or arthroplasty.

Generalized ligamentous laxity and the Broström repair

The Broström procedure is widely recognized as an excellent approach for treating lateral ligamentous laxity around the ankle. The soft-tissue repair has been shown to result in improved symptoms and reduced instability in cohort series of patients, and is generally considered the 'operation of choice' in those patients who are suitable for it. However, there are a few caveats, and many surgeons would think twice about carrying out the procedure on patients with a generalized ligamentous laxity due to concerns about poor functional outcomes and 'restretching' of the soft tissues. In a similar vein to the last paper, it was reassuring to read this report from **Seoul (South Korea)**, where the authors have undertaken a modified Broström repair and then compared the outcomes in patients with and without generalized ligamentous laxity.[3] As the surgery itself was undertaken with arthroscopic assistance, the surgical team were also able to undertake a direct inspection of the joint at the time of surgery, and also report their outcomes clinically using the American Orthopaedic Foot and Ankle Society (AOFAS) ankle-hindfoot score, a Visual Analogue Scale (VAS) pain score, and radiologically, using the talar tilt angle. This is a large series of 99 patients, which is one of the key strengths of the paper, with 24 patients in the laxity group (Beighton score of 4+) and 75 patients without laxity. There was a significantly greater change in talar tilt angle from preoperative to 12 in the laxity group (-6.9) than in the normal group (-4.2). In terms of clinical outcomes, the reported final follow-up AOFAS ankle-hindfoot score and VAS pain scores were both significantly better than the preoperative scores. This is an important study in that, although surgeons will clearly still treat patients on an individual basis, generalized ligamentous laxity perhaps should not cause as much concern as it currently does to surgeons contemplating ankle instability surgery.

Do we need to operate on Achilles tendons? X-ref

Despite the widespread polarizing views on Achilles tendon treatment, with units and surgeons either 'operators' or 'nonoperative', often with a firmly held and expressed view, the evidence is somewhat lagging behind the debate. One area where there is very little agreement is rehabilitation following conservative management of Achilles tendons. Perhaps more concerning yet is that there is little literature available comparing operative and nonoperative treatment in the now widely used functional braces. These authors from **Whangarei (New Zealand)** have stepped up to this evidence gap fill – at least partly – and they present their series of 200 patients (99 operative and 101 nonoperative), all managed with an identical functional bracing treatment protocol.[4] The study spanned ten years in their centre, and outcomes were assessed using the Achilles tendon total rupture score (ATRS). As is the case with studies of this nature, there was significant attrition, with 132 patients (62 operative, 70 nonoperative) reported at final (minimum two-year) follow-up. Overall, there were no differences between the groups, with an ATRS of 85 in both operative and nonoperative groups. The authors also undertook some subgroup analysis within the constraints of the study size in an attempt to establish what the potential differences were between male and

female patients, or between patients younger and older than 40 years. Overall, there were no differences in any subgroups using the ATRS as a clinical outcome measure, and this was confirmed by logistic regression failing to show any significant effect of gender, age at rupture, or mode of treatment on the eventual ATRS score.

Plates and nails equal in ankle fusion

■ It is an oft-quoted truism that when there is more than one described and widely used surgical technique for any operation then there is probably no single perfect operation, or differing indications yield different results for different operations. Two of the most widely described operative approaches to tibiotalocalcaneal (TCC) arthrodesis are the intramedullary (IM) nail and locked plating. It is, of course, not just the implants that differ here; so too does the operative approach. The plates are generally inserted through a lateral transfibular approach, giving the advantage of simultaneous joint preparation and plate insertion. The intramedullary nail is obviously percutaneously inserted and, as such, provides some flexibility in soft-tissue approach. In this series, the authors undertook a posterior Achilles tendon-sparing approach, giving a better soft-tissue envelope. This series from a study team based in **Durham, North Carolina (USA)** describes a comparative outcome series of 38 patients undertaken with an IM nailing and posterior Achilles tendon-sparing approach, compared with a lateral transfibular approach.[5] Patients who underwent TCC fusion but did not have the implant and approach combination described were excluded from the series. The overall union rate was 71%, slightly higher in the IM nail group (76% *vs* 64%), and this was reflected in the revision for symptomatic nonunion rates (16% *vs* 7%). These differences, however (in this relatively small series), were not symptomatic. It seems that, overall, the rates of complication, revision, and nonunion are broadly similar across this series of patients. It is therefore a reasonable idea to choose the operative approach and implant choice based on individual patient-related factors such as preoperative deformity, soft-tissue envelope, and surgeon preferences to try to get the best result for each patient. This paper again highlights the high rates of nonunion seen in TCC fusions, but shows that many of these are asymptomatic or not symptomatic enough to warrant revision surgery.

PROMIS reliable in the foot and ankle

■ The dependability of evidence-based medicine and performance-related healthcare payments is entirely reliant on the collection of patient-reported outcome measures (PROMs). While this is a development we would wholeheartedly support here at *360*, it does carry with it a substantial responder burden, with patients often required to fill out long questionnaires at multiple timepoints during their healthcare journey – a task that not only takes patient time, but can also encourage incomplete or inaccurate questionnaires. The Patient-Reported Outcome Measurement Information System (PROMIS) is an adaptive questioning system that homes in very quickly on relevant questions, and has been shown in many diagnoses and conditions to be as reliable as more traditional questionnaires. The difficulty with an adaptive system like this, of course, is that it needs to be validated against traditional questionnaires in almost every diagnosis. We were delighted to see that researchers in **St Louis, Missouri (USA)** have done just this for patients with hallux valgus.[6] The study revolves around establishing the relationship between traditional PROMs scores (in this case, the Foot and Ankle Ability Measure (FAAM)) and the PROMIS system in hallux valgus diagnoses. The authors enrolled in the study, on a retrospective basis, 85 patients with hallux valgus diagnoses who had completed both the FAAM Activities of Daily Living (ADL) and PROMIS scores (physical function, pain interference, and depression). The authors analyzed the data using a rudimentary approach of an initial Spearman's correlation coefficient and then a stepwise regression approach, in an attempt to establish which demographic and outcome variables were determining any observed variance in scores. At a top-down level, there was a strong correlation between the FAAM scores and PROMIS physical function ($r = 0.70$), while weaker correlations were seen with PROMIS pain interference ($r = -0.65$) and PROMIS depression ($r = -0.35$) indices. The authors conclude that a significant proportion of the variation in the PROMIS physical function and FAAM ADL scores can be accounted for by the PROMIS pain scale. Here at *360*, we are not sure if, with an adaptive score like PROMIS, this sort of simplified approach to scoring variation can be taken, as it is likely that the PROMIS itself is non-linear. However, this paper does clearly show that, in hallux valgus patients, the efficiency of data capture allowed by the PROMIS can be easily harnessed.

Are multiple procedures in the diabetic foot just 'creeping amputations'?

■ The diabetic foot and ankle is a fickle beast, and surgeons, diabetologists, podiatrists, and patients often input significant time, effort, healthcare expenditure, and emotion in multiple limb salvage procedures that may or may not eventually end up in amputation. The authors of this paper from **Singapore (Singapore)** ask the question, is a 'creeping amputation' really a good option in these patients? And given their time again, would the patients have opted primarily for an amputation?

The authors report a series of 41 patients, all of whom underwent below-knee amputation (BKA) over a two-year period for the sequelae of diabetic infection. The cohort was divided into those who had primary amputation and those who had a creeping amputation (multiple failed salvage procedures). Outcomes were assessed using the Barthel Index (BI) and the Reintegration to Normal Living Index (RNLI). In addition, the authors asked the participants a series of structured questions with the aim of determining whether the patient would consider having the multiple attempts at limb salvage again if faced with the same problem. The eventual outcomes were good, with a mean BI of 14.2 and RNLI of 73.2. Between the two groups, there were no differences in likelihood of prosthesis usage or in either of the outcome scores. However, perhaps surprisingly, 16 of 17 patients with creeping amputation would undergo the same procedures if given a similar option; conversely, only 15 patients (62.5%) with primary amputation would do the same again, while the other nine patients (37.5%) would choose to do everything possible to save their leg if faced with a similar situation. The conclusions one can draw from this paper are fairly clear. If faced with a diabetic foot infection, there really aren't any advantages in jumping straight to amputation, either for patient preferences or eventual outcomes, as those who fail salvage do just as well as those who have a primary amputation.

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Wrist & Hand

X-ref *For other Roundups in this issue that cross-reference with Wrist & Hand see: Children's orthopaedics Roundup 6*

Can we avoid fusion in Heberdon's nodes?

Knobbly fingers (Heberdon's nodes) are the sign of a lifetime of hard work and an unavoidable rite of passage into mature age. While most people find them pain-free, some people find that their fingers become too uncomfortable. Apart from reassurance and painkillers, and perhaps a steroid injection, the only other treatment for longstanding symptoms would be a fusion. Patients understandably often struggle between the trade-off between stiffness and absence of pain. It is encouraging to read this study from **Pittsburgh, Pennsylvania (USA)**, in which the authors take a different approach of simply debriding the osteophytes from the distal interphalangeal (DIP) joints.[1] The authors report a case series of 78 patients, all with symptomatic DIP joint osteoarthritis, who underwent a simple cheilectomy. The patients were reported to a minimum of two years follow-up. The operations were all similar and all patients underwent a simple open cheilectomy and were then immobilized for a month postoperatively. The authors report outcomes in terms of visual analogue scale (VAS) pain and motion scores to a median 36 months follow-up, and they report a significant improvement in mean VAS pain scores (improving from 8 to 1) with a surprising 20° improvement in range of motion in the DIP joint. The authors did not report any reoperations or complications in the follow-up period, and all in all this does look to be an attractive option.

Can we trust wrist arthroplasty yet?

Here at *360*, we are, as thoroughly responsible orthopaedic surgeons, a little cynical about new implants – and especially those with limited clinical data. Our world is replete with examples of apparently encouraging implants that end up being disappointing at best and thoroughly destructive at worst. The story of wrist arthroplasty so far is not one of resounding success or reliability. However, studies are just beginning to emerge to show that perhaps newer designs are becoming more predictable. Here we report on two. A group from **Oslo (Norway)** have reported the outcome of 56 wrist arthroplasties using the Motec system.[2] This total wrist arthroplasty involves a ball and socket with a long stem into the radius and a long stem into the third metacarpal. At a mean follow-up of eight years, eight of 56 patients required revision (four to arthrodesis and four revised to a further arthroplasty) and a further two had asymptomatic radiographic loosening. This gave a Kaplan–Meier ten-year survival rate of 86%. A total of 11 patients out of 17 could return to manual labour, and clinical results at final follow-up were impressive. The authors reported improved Quick Disabilities of the Arm, Shoulder and Hand (QuickDASH) and Visual Analogue Scale (VAS) pain scores. The mean range of motion improved from 97° to 126°. This improved range of motion was matched by improved grip strength, from 21 kg to 24 kg. Another group from **Providence, Rhode Island; Redwood City, California; and Durham, North Carolina (USA)** also report on contemporary results from a modern wrist arthroplasty.[3] Their paper reports the outcomes of 69 patients, all with a primary diagnosis of inflammatory arthritis and treated by fourth generation wrist arthroplasty for between five and 14 years. The Kaplan–Meier reported 14-year survival rate was an impressive 78%. In this case, as with most wrist arthroplasties, their patients did not report an improvement in their range of motion. However, pain improved from a preoperative score of 8.6 to a postoperative score of just 0.4. While two swallows do not make a summer, we can start to look towards wrist arthroplasty as a more reliable option. A lot of hard work has clearly gone in to developing better and more reliable arthroplasties, and the results are starting to look more favourable. The problem, of course, is that the wrist arthroplasty has to compete with the wrist arthrodesis, which is a surprisingly functional and robust operation. Nevertheless, despite these two reports of success, these implants should be still regarded as experimental and should be performed in specialized centres with very careful consent and follow-up.

Treating cubital tunnel syndrome: should we excise the epicondyle? X-ref

Cubital tunnel release is a widely undertaken operation for a problem that is common and sometimes difficult to treat. In the past, it was almost routine to transpose the ulnar nerve in the hope that doing so, in combination with decompression of the cubital tunnel roof, would result in a more successful outcome. However, transposition, at least in theory, renders the nerve relatively ischaemic and increases the potential for scar formation. With some clinical series demonstrating no clinical benefit, it has seemed that less is more. For that reason, there has been a trend to leave the nerve *in situ* unless it frankly subluxates forwards in most practice. If the nerve is subluxing, then there remains the oft-forgotten option of excising the medial epicondyle to prevent stretching of the subluxating nerve and to avoid the extra dissection for transposition. Given the plethora of literature on the topic, multiple potential operations and a somewhat divided community, we were delighted to see this systematic review from **Birmingham (UK)**.[4] This review team have systematically reviewed the literature, of which there is surprisingly little. Of the six studies comparing medial epicondylectomy to transposition,