metal component (not bearing prosthesis) survival as their primary outcome measure. At 15 years of follow-up, the Kaplan–Meier calculated survival was 73%. The clinical scores were only available in a disappointing but understandable 29% of patients (n=24). There was a single patient who was reported as requiring a subtalar fusion for symptomatic adjacent joint arthritis, while exchange of the polyethylene bearing for fracture was required in three patients. Although the data is to be interpreted with care, given the restrictions in the size of the groups, the authors report that American Orthopaedic Foot & Ankle Society (AOFAS) scores improved from a mean of 39.6 points, preoperatively, to a mean of 71.6. However, around half of patients with retained implants required at least a single additional surgical procedure. Although these results are from a relatively small series, there are a number of interesting take-home messages given here. The authors' figures would suggest that, if a STAR ankle is *in situ* at nine years, the chances are it will survive to 15. With an overall survival of 73% at 15 years, total ankle arthroplasty can now be considered to be a long-term option for the majority of patients.

Charcot deformity and the hindfoot nail X-ref

■ Hindfoot Charcot deformity poses a somewhat difficult problem. The traditional teaching for Charcot foot is to avoid surgery if at all possible, which is a reasonably successful strategy in the mid- and forefoot, but in the hindfoot is much more difficult to do. This is because the degenerative process associated with Charcot disease results in deformities of the hindfoot and ankle that are often not suitable for nonoperative management. This group in **Vienna (Austria)** have evaluated the options of hindfoot nailing with a retrograde nail as a treatment for Charcot arthropathy.[7] They report the use of hindfoot nails (two varieties, but both straight nails with a compression component) in 19 feet (18 patients), all undergoing hindfoot arthrodesis for Charcot arthropathy. The authors sought to establish what results can be expected with this approach in terms of both limb salvage and complication rates. Their series reports the outcomes at just over 3.5 years with an impressive limb salvage rate of 84% (n=16/19). Limb loss was, in all three cases, due to established osteomyelitis. The authors report reasonable functional outcomes, with significant improvements in the American Orthopaedic Foot and Ankle Society (AOFAS) score (71 *vs* 25), the Foot Function Index (FFI) (87 *vs* 161), visual analogue scale (VAS) score (1.9 *vs* 6.1), and various items of the Foot and Ankle Outcome Score (FAOS) (pain, 39 *vs* 81; activities of daily living, 50 *vs* 74; quality of life, 25 *vs* 56). Overall, this series is representative of the Charcot population, with a large number of smokers, diabetics, and patients with preoperative ulceration. On the whole, the results of hindfoot nailing appear to be reasonably good. There are relatively frequent complications reported in this series, as one would expect; however, the limb salvage rate was excellent, and this represents a successful treatment for a difficult problem.

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Wrist & Hand

Can we improve stiffness after distal radial fractures? X-ref

■ Stiffness is a problem we often encounter after distal radial fractures, and it can lead to significant pain and disability for the patient. In many large joints, a capsular release is undertaken if conservative measures fail, and this is sometimes combined with release of ligamentous structures. What is a relatively accepted procedure in the knee, shoulder, and elbow is far from being proven or accepted in the wrist. This is partly due to a perception among many surgeons that there is little that can be done to alleviate stiffness in the wrist, as extensive releases risk joint instability and our desire to 'first do no harm' is usually an overriding one. It has previously been confirmed that supination stiffness can be improved with a volar distal radioulnar joint (DRUJ) capsular release.[1] This paper from **Stanford, California (USA)** and **Duke, North Carolina (USA)** provides a reassuring account of the two centres' experiences with release of the volar capsule of the radiocarpal joint for extension stiffness.[2] The authors describe the outcomes of 11 patients, all with post-traumatic loss of extension following treatment of a distal radial fracture with a volar locking plate. The patients all underwent operative intervention, consisting of removal of the volar locking plate and releases. The releases were a flexor carpi radialis tenolysis and a subperiosteal release of the capsule as it attaches to the radius; only the volar extrinsic ligaments were divided. Patients were followed to a mean of 4.5 years and demonstrated significant improvements in range of motion and Disabilities of the Arm, Shoulder and Hand (DASH) scores. Importantly, there were no signs of wrist or carpal instability on plain film radiography or fluoroscopy. It seems, therefore, that the technique of volar ligamentous release does not universally lead to the feared consequences of carpal instability, and may ultimately become a more accepted part of the armamentarium for this not uncommon problem. It is probably the case here that the use of extensive ligamentous releases is offset by pre-existing contracture that prevents complete instability.

Is dorsal plating really that bad?

■ Is dorsal plating of the radius really that much more troublesome than volar plating? The original

versions of dorsal plates acquired a poor reputation for causing stiffness and tendon irritation or rupture. There has been a general recent acknowledgement that volar plating is less liable to cause complications, but, not infrequently, this approach leads to complications as well, typically from proud dorsal screws or pegs. Given the main, and now widespread, objections to dorsal plating, new low-profile plates have been developed and various small series are starting to emerge that have demonstrated good results. This, set against a considerable incidence of complications associated with volar plating, has led a review team in **Ichihara (Japan)** to design a study in order to compare the two approaches.[3] The authors report a prospective non-randomized trial in which all patients underwent open reduction internal fixation (ORIF) for displaced intra-articular distal radial fracture. Treatment allocation was via surgeon preference; at final follow-up, 38 patients had been treated via a dorsal approach and 68 patients were treated using a volar approach. Surgical approach was agreed by consensus of surgeons, and indications for dorsal approach were dorsal shear or die-punch fractures, fractures comminuted dorsally and requiring grafting, and those with associated scapholunate injury. It is therefore rather difficult to make direct comparisons of outcomes, and even interpretation of complication rates should be treated with some caution, as certain complications, such as extensor pollicis longus (EPL) rupture, are more common with specific fracture patterns, even without surgery. The complication rates were higher in the volar plate group but this was not statistically significant. Interestingly, they report no extensor tendon rupture or irritation in the dorsally plated group, and one flexor pollicis longus (FPL) rupture in the volar group. There were, perhaps as would be expected, no differences in the QuickDASH scores. At this institution, removal of metalwork is routinely offered, which is a significant departure from practice elsewhere; this is performed eight to 12 months following surgery. Their follow-up scores and outcomes are from before this second procedure, except in eight dorsal and nine volar patients who declined removal and were assessed later at up to 28 months. Given that the principle obstacle to dorsal plating is tendon complications, the authors concluded that certain fracture patterns are more appropriately stabilized using a dorsal plate fixation with equivalent outcomes and complication rates. This may be an area that is entering its 'Indian summer' and, with newer plates and the complications of old fading into the past, we wouldn't be surprised to see more surgeons attempting a dorsal approach.

Why don't we agree on the treatment of Dupuytren's disease?

There are so many published papers on Dupuytren's disease (a cursory Pubmed search of "Dupuytren's disease" + "treatment" revealed 1343 hits, and there are bound to be many more) that it is perhaps not surprising there is no consensus on treatment. Researchers from **Ontario (Canada)** undertook a study with the aim of examining agreement between experts in respect to treatment recommendations for Dupuytren's disease.[4] Although this sort of consensus paper is easy to undertake and often has little to recommend it, this particular paper caught our eyes at the *360* editorial offices, given the involvement of a good number of hand surgeons (36) originating from nine different countries. The panel of surgeons were experienced, with 15 years in practice on average, and the study was designed to require the surgeon to choose a treatment from needle aponeurotomy, surgery, and collagenase injection, using 16 different clinical scenarios. The scenarios themselves did not differ between experts and had been predeveloped using expert input. Each case represented a unique combination of four dichotomous variables including cord thickness, contracture severity, patient age, and joint involvement. Interrater reliability statistics were calculated and showed low levels of concordance, with a mean pairwise percent agreement of 26%; Krippendorff's alpha was just 0.012. Predictors that led to widespread recommendation of surgery were: contracture greater than 70°; a thick precentral cord; involvement of the metacarpophalangeal and proximal interphalangeal joints; and greater number of years in practice. More years in practice predicted a recommendation for collagenase injection and the presence of a thick precentral cord predicted a recommendation for needle aponeurotomy. It comes as no surprise to us here at *360* that, essentially, little agreement exists on treatment recommendations for common presentations of Dupuytren's disease in this sample – surgery is at least as much art as it is science, and every surgeon will have their own perspective based on experience, training, philosophy, funding arrangements, and bias.[5]

Carpal tunnel release with depression

As surgeons, we all understand that our surgical outcomes can often be heavily influenced by our patient's psychology and mental state, just as much as they are by the primary diagnosis and treatment success. This is especially so when it comes to the improvement of pain symptoms, a field where our understanding of the influence of psychology is far from complete and where depression and chronic pain are often interacting and self-reinforcing conditions. Carpal tunnel syndrome (CTS) is one of the commonest diagnoses, and outcomes are reported as somewhat variable but, overall, consistently good. This group from **Leiden (The Netherlands)** prospectively examined the relationship between depressive symptoms and the outcomes of carpal tunnel release in a cohort of 227 patients.[6] Depressive symptoms were quantified using the Center for Epidemiologic Studies Depression (CESD) scale and carpal tunnel symptoms were recorded via the Boston Carpal Tunnel Questionnaire (BCTQ). In addition, the authors recorded the documentation of palmar pain. Prior to surgery, patients with depression had a higher BCTQ score than patients without depression and, although both groups showed improvement, this effect was seen to be persistent at a year following surgery. The authors undertook a multivariate analysis with the aim of identifying the specific contribution of depression to the outcomes in this series. They were able to include an appropriate number of covariates in their model, which essentially showed that preoperative depression was not an independent predictor of residual carpal tunnel symptoms. In this series, the presence of depression had a small but statistically significant influence on palmar pain. Here is yet another condition that has previously been felt to be a 'knee-jerk reaction' from a decision-making perspective. The authors here concluded that depressive symptoms in patients with CTS decrease after release, along with an improvement in CTS symptoms.

Fixing phalangeal fractures: remember to consent for reoperation X-ref

In some parts of hand trauma surgery, there is the risk that interventions become a triumph of technique over common sense, with little evidence to support the use of surgery over conservative management, let alone one surgical technique over another. The proximal phalanx, when broken, is a troublesome challenge, with the surgeon having to balance the risk of malunion with early mobilization against the risk of stiffness with prolonged immobilization or surgery. To make matters worse, there is not much evidence on which to base these decisions. Many fractures – perhaps most – should be treated nonoperatively, but some very unstable or displaced fractures are likely to benefit from an operation. A group from **Stockholm (Sweden)** have reported their own series of retrospective phalangeal fractures with the aim of teasing out which do well from surgery and which do not.[7] The authors examined all extra-articular closed fractures of the proximal and middle phalanges treated with surgery over a four-year period in their institution. As a relatively high-volume and interventional unit, they were able to include the outcomes of 181 fractures in their paper, and, surprisingly, a very large proportion (25%) required reoperation, mainly for stiffness. The reoperation rates did vary depending on operative intervention (although there is bound to be some selection bias here) and were 25% after Kirschner wire, 15% after screws, and 42% after plate fixation. The authors used a logistic regression analysis in an attempt to correct for fracture type and other comorbidities, and determine which method of osteosynthesis was the most important factor for reoperation. They established that although the unadjusted rates of reoperation for plating were higher, they were not significantly so after then adjusting for fracture complexity. Notwithstanding this, it is incumbent on us, as ever, to help the patient to understand the implications of surgery, not least of which is the need for a second procedure (in over 40% of cases treated with a plate) to undo the stiffness caused by the first.

Proximal pole vascularity not a predictor of success in scaphoid nonunion X-ref

There are a variety of opinions regarding the best treatment for scaphoid nonunions, with many authors advocating the need for vascularized grafting in situations where the proximal pole vascularity is compromised. On the face of it, this seems to be a sensible approach, as bone healing is clearly dependent on vascularity. However, there is not yet a weight of evidence confirming its effectiveness or otherwise. One of the principle problems in interpreting previous studies is our difficulty in classifying scaphoid injuries, and accurately assessing vascularity preoperatively. Historically, preoperative imaging has correlated relatively poorly with intraoperative findings and subsequently with outcomes. If we can't identify a group that would benefit from vascularized grafting, how can we target the use of this more complex technique? This group from the scaphoid nonunion consortium in **New York, New York (USA)** sought to identify whether any marker of proximal pole vascularity affects the likelihood of healing or the time to union in patients with scaphoid nonunions treated with non-vascularized autologous bone grafting.[8] They report a series following 35 patients in a local prospective registry who were treated with curettage, non-vascularized autologous grafting, and headless screw fixation. The research team undertook a complete assessment of preoperative vascularity including MRI and the presence of visible intraoperative bleeding points. Finally, they also undertook histopathological analysis of cancellous bone in the proximal pole to establish if there was preoperative necrosis. The presence of healing was defined as 50% bony union on postoperative CT scanning. In this series of the original 23 patients, nine proximal poles were ischaemic on MRI but none were infarcted; the majority (28/33) were found to have impaired vascularity as assessed by intraoperative bleeding. In terms of the histopathology, 14 of 32 demonstrated trabecular necrosis. Despite evidence of impaired vascularity in over half of the patients, 33 of the 35 scaphoids had healed by 12 weeks, as defined by the authors as 50% bony bridging on CT. Preoperative MRI, intraoperative punctate bleeding, and histopathological examination that demonstrated these changes did not correlate with each other, nor with union rates or time to union. The authors therefore advocate treatment with a non-vascularized graft. While this seems to be a suitable approach, based on this series, here at *360* we suspect the debate is far from over.

Allograft *versus* flexor carpi radialis tendon in trapeziectomy

Suspension-interposition arthroplasty is a commonly performed operation for thumb carpometacarpal joint osteoarthritis and, although not universal, is a widely accepted technique. A variety of methods for performing the interposition have been described, along with a variety of materials, indicating that, as with so many surgical problems, we have not yet settled on the optimal solution. Autologous grafts from the flexor carpi radialis (FCR) or abductor pollicis longus (APL) are two of the most common treatments; artificial spacers, such as silicone, have been used with less successful results. This group from **Zurich (Switzerland)** investigated the use of human acellular dermal matrix with a randomized controlled trial.[9] The trial design was randomized by side; one group received the human acellular dermal matrix, while the other group was treated with the standard technique in the unit using half of the FCR tendon. In the allograft group, a similar technique was utilized and the graft was sutured onto the FCR tendon as close as possible to its insertion on the second metacarpal bone. The primary outcome was measured at 12 months, using the postoperative Michigan Hand Outcomes Questionnaire (MHQ). Overall, 60 patients, all with osteoarthritis of the first carpometacarpal joint (CMCJ), were included in the study. A total of 60 patients with Eaton disease, stage II or greater, were operated and followed up to 12 months. Similar outcomes were found for both groups at all follow-up assessments in all measures, and the complication rates were not significantly different. In the FCR group, there were five complications, with two patients developing pain and tendonitis of the FCR. In the allograft group, there were ten complications, including two with FCR tendinitis and six with FCR partial rupture, only one of which required any treatment; revision surgery was performed. Based on these results and the cost of the allograft, it would seem that it is inappropriate to use this technique in this situation when a FCR spacer is equally effective. Of course, the question that remains is whether a spacer is required at all.

Inadvertent harvest of the median nerve

One might expect a hand surgeon to be meticulous with an unerringly accurate knowledge of anatomy. So, we were surprised, and indeed rather perturbed, to read of 19 cases collated by researchers from **Pennsylvania, New York**, and **Massachusetts (USA)** of inadvertent harvest of the median nerve rather than their intended target of the palmaris longus.[10] Of even more concern was that in 12 of the cases, the surgeon did not realise and continued to use the nerve as a tendon graft for ligament reconstruction. The postoperative numbness was not recognized as being due to median nerve deletion until at least two months post-surgery in seven of

the cases. In attempting to identify the causes of these never-events, some of the surgeons were generally described as rushed, inexperienced, cavalier, or overconfident, and these are certainly characteristics and situations that we should all try to mitigate in our own practices. One surgeon was described as extremely careful and meticulous; that particular surgeon recognized the error at the time of surgery, reminding us that we can all make mistakes. The authors' conclusion is unsurprising: knowledge of the relevant anatomy is crucial to avoiding inadvertent harvest of the median nerve instead of the palmaris longus tendon.

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Shoulder & Elbow

X-ref ***For other Roundups in this issue that cross-reference with Shoulder & Elbow see: Research Roundup 5; Trauma Roundup 2.***

Better outcomes with SAD, but below clinical importance

Unsurprisingly, Can Shoulder Arthroplasty Work? (CSAW), a randomized controlled trial from the Oxford group published in the *Lancet*, takes centre stage in the shoulder and elbow roundup for this issue. This study has generated numerous headlines in the mainstream media, which are often sensationalistic oversimplifications along the lines of 'shoulder surgery doesn't work'! This crude media folly underlines the importance of a careful academic assessment of any scientific paper and of appropriate clinical interpretation with respect to our individual patients. These authors from **Oxford (UK)** undertook a large three-armed randomized controlled trial reporting the outcomes of over 300 patients, all with subacromial impingement-type pain, randomly allocated to decompression surgery (n=106), arthroscopy only (n=103), or no treatment (n=104).[1] Outcomes were assessed at both six months and one year, and were reported using the Oxford Shoulder Score. Conducting a trial of this nature is a significant challenge and the group are to be commended. Although cloaked in caution, their conclusion that the results of subacromial decompression (SAD) are no different to those of arthroscopy alone is an uncomfortable one for many surgeons, and much criticism has therefore been levelled, both justified and unjustified. Observers have highlighted the 42% of patients in the sham arthroscopy group and 23% of patients in the surgical arthroscopy group who were non-compliant with their treatment allocation. Furthermore, 12% of the 'no treatment' group did not continue in their study allocation; some patients chose to undergo surgical decompression. The treatment effect in the surgical group may be due to the surgery or the postoperative physiotherapy. Finally, subacromial pain has a variety of potential aetiologies, and pathologies such as partial cuff tears were included in the study. Randomization should eliminate the effects but, with 100 patients in each arm and significant crossover, the results are therefore rather difficult to interpret. There may be remaining questions over the underlying diagnoses at the time of inclusion in the study, but a pragmatic study is often the best way to examine real-life situations. It is indeed likely that subacromial decompression is performed too frequently in many countries, as many patients would do just as well with conservative management, but it may well be the case that there is a subset of patients who would still benefit from the procedure. The British Elbow & Shoulder Society/British Orthopaedic Association (BESS/BOA) response to this paper highlights exactly this point, urging careful patient selection and informed shared decision-making; their general guidance for the management of subacromial pain is due to be updated in the near future.

Elbows and the Norwegian Arthroplasty Register

Registry data are extremely powerful with the weight of numbers behind them; however, they are just observational cohort data and, despite the numbers, drawing inferences can be tricky. In the United Kingdom, while lower limb arthroplasty data are well established, the shoulder and elbow arthroplasty data in the National Joint Registry are in their relative infancy. We therefore applaud the foresight of the Norwegian register to set up early and allow the production of this long-term follow-up study. This study from **Bergen (Norway)** sets out to present 20 years' worth of follow-up data from a national perspective.[2] Over 800 elbows were recorded on the register between 1994 and 2017. The authors extracted data from the register to study for survival and reason for revision. Comparisons were also made between different types of replacement. The longest follow-up in this series is now at 24 years, and although the survivorship is inevitably inferior to that of lower limb arthroplasty, the rates at 20 years are quite respectable: five-, ten-, 15-, and 20-year overall survival rates for all elbow arthroplasties were reported as 92%, 81%, 71%, and 61%, respectively. Unsurprisingly, aseptic loosening dominated the reasons for revision, followed by defective polyethylene, infection, and dislocation. As the authors point out, the reasons for these failures are often somewhat design-specific, and newer and more refined designs may overcome this issue. Considering that this data spans the last 20 years, in which time we have learned much about implant design, these figures should improve with future evolutions. Certainly, our knowledge of the mechanisms of failure have improved. We now have a greater appreciation of the importance of