

Electrotherapy News

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NEWS

Hi there and welcome to the latest update on various aspects of Electrotherapy Research, Publications and associated matters. I am sorry for the delay in getting this out to you – I was hoping to get it distributed a couple of weeks ago but have been working like crazy to get the next edition of Electrotherapy : Evidence Based Practice off to the publishers – all done now though it will be the best part of a year before it is out on the shelf I gather

Anyway, there is a fair selection of new material for your perusal in this issue, so without further ado, lets get on with some news and then on to the papers.

Firstly, just to let you know about a research project that we have gotten off the ground after a fairly long lead in time. For several years now, I have been getting a steady stream of enquiries about the use of ultrasound for the treatment of abdominal nodules, most usually occurring as a result of apomorphine injections for patients with Parkinsons. Anecdotally, several therapists have found that the ultrasound appears to help in limiting the problems that arise from these nodules – pain, tenderness and of course, limited opportunity for further injection. Additionally, I get numerous e mails every year from therapists who have been asked to do some ultrasound for these nodules, but want to know whether it works and what dose to use etc. We have got some limited finding from the Parkinson Disease Society to run a pilot study to evaluate the effectiveness of ultrasound in these circumstances, to see whether it actually does make a difference as there is no objective evidence out there (that we have found anyway). The trial is just about underway, and we will be comparing ultrasound, placebo ultrasound and control (i.e. nothing) conditions for these nodules and taking measurements of nodule size, hardness, subjective data with regards nodule behaviour and pain and also ultrasound scans of the nodules in the anticipation that we can determine whether the US is actually making any real difference or not. If the results are encouraging, then we will move to a full clinical trial – hopefully a funded one! Will let you know the outcome as soon as we have some data to report.

Second piece of news – another Electrotherapy textbook has made an appearance – Practical Electrotherapy : A Guide to safe Application by Fox and Sharp (based in Cardiff), published by Elsevier. This is a new publication rather than a new edition of an existing text, and in the blurb, it is described as ‘ . . . the first and only book of its kind which describes how to apply common electrotherapy modalities to a patient in the clinical setting’ It is a 2007 publication and therefore should be pretty well up to date. The authors deal with commonly employed modalities – heating and cooling modalities, various forms of electrical stimulation and the ultrasound, laser and pulsed shortwave triplet. Each chapter looks at (briefly) what the modality is, how to apply it (with several variations) and the dangers, contraindications and tests required. In addition, there are summary tables that identify where the information came from – published studies and reviews. It is an interesting take on a theme, and although not intended to be a text that fully evaluates the theory and background material for the modalities, it should provide a useful resource for the practitioner needing a quick ref-

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erence guide, the student looking for an application reminder or those who use electrotherapy infrequently and need a quick look up. I have some issues with some of the content – but I guess I would say that eh? Well worth a look if you are in the market for such a text. It is due (according to the publishers) in May 2007 and the 'official' price is around 22 GBP.

CONTENT

In this issue, there are a range of recent(ish) publications (26 in all) that cover several topics in electrotherapy and tissue repair. I have tried to group them sensibly and their place in the newsletter is not ranked in terms of importance, quality or anything other than what I think is logical.

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- Magnetics and osteopenia : Taniguchi et al (2007)
- Magnetic stim vs NMES for quads : Han et al (2006)

Tissue repair

- Neovascularity and tendon problems : Fowble et al (2006)
- Effect of stretching on collagen and muscle : Coutinho et al (2006)

OK, this is a lot of material, so better get on with it. Not going to do loads on each paper or else you will lose the will to live and I will still be here at the keyboard in a months time. As ever, this is just a taster of what each paper is all about and you are strongly encouraged to access the original

Ultrasound and Wound Dressings : Poltawski and Watson (2007)

OK, selfish I know, but one of the pair or recent papers that we have published regarding ultrasound transmission (**Poltawski, L. and T. Watson (2007). *Transmission of therapeutic ultrasound by wound dressings. Wounds 19(1): 1-12***) which evaluates the capability of 48 different types of wound dressings and their ability to transmit (or not) ultrasound. Therapeutic US can be used to treat open wounds – typically chronic venous ulcers and pressure sores – and rather than repeatedly disturb the wound by removing the dressing which can be detrimental to repair, it could be advantageous to deliver the therapy through the dressing. Additionally, if one wanted to use ultrasound scanning to evaluate the wound, this too could be performed through the dressing if it were able to effectively transmit the energy. A wide range of currently employed wound dressing materials were tested using a similar procedure to that described in our previous papers (Poltawski, L. and T. Watson (2007). Relative transmissivity of ultrasound coupling agents commonly used by therapists in the UK. *Ultrasound Med Biol* 33(1): 120-8 – covered in the last issue). Transmissivity varied significantly between those tested, ranging from around 100% right down to zero, and therefore it is of the utmost importance that one considers very carefully which dressings are employed if this is the therapy route you are employing. It was interesting to note that although some dressing types were generally better than others (generally the films, hydrogels and alginates were better and the hydrocolloids and the foams were worse) there were inconsistencies with the groups. The hydrocolloids for example ranged from 92% transmissivity right down to 0%. Whilst US applied through dressings is a bit of a specialist application, this information could make a whole lot of difference to those practitioners that use this method. Having done work on regular gels and now dressings, the one major area where we still plan to do some work when we can is to look at transmission through gels that include various medications – NSAID's for example. Our preliminary data would suggest that a very substantial proportion of the US energy is lost going through these gels, but it has not been previously documented and having established the method, it would make for a useful final study in the series – watch this space.

Ultrasound Transducer Velocity : Weaver et al (2006)

There have been questions around for some time about whether it makes any difference what speed you move the ultrasound treatment head during application. A recent paper has looked at just that, using intramuscular temperature and the primary outcome (**Weaver, S. L., T. J. Demchak, et al. (2006). *Effect of transducer velocity on intramuscular temperature during a 1-MHz ultrasound treatment. J Orthop Sports Phys Ther* 36(5): 320-5**). The observation was made that in the clinical setting, US treatment head movement was commonly faster than the supposed optimum. The temperature of the gastrocs was monitored in 11 healthy volunteers who were exposed to three different US treatments – all at the same dose (1MHz, continuous, 1.5 W/cm² for 10 minutes, covering an area of 2x the treatment head). The tissue temperature was monitored and allowed to return to baseline before the next intervention. Applicator movement was varied between 2-3 cm/s, 4-5 cm/s and 7-8 cm/s and all resulted in a temperature increase in the gastrocs (3cm depth below skin and fat), but there was no significant difference between the applicator velocities, therefore confirming what several of us have been saying for some time – that it is the energy delivered that determines the outcome rather than the speed at which the treatment head is moved. It is important to keep the treatment head moving, and also important that it is moved 'evenly' over the intended treatment area – no problem with either of these points – just that the speed is not an issue in clinical practice.

LIPUS and Fracture Healing : Erdogan et al (2006)

I think that I have included something on ultrasound and fracture healing in just about every issue of the newsletter, and this one is no different! The recent animal study from a research group in Turkey (**Erdogan, O. et al. (2006). *Effects of low-intensity pulsed ultrasound on healing of mandibular fractures: an experimental study in rabbits. J Oral Maxillofac Surg* 64(2): 180-8**) has employed a now standard US mode – low intensity pulsed ultrasound or LIPUS to evaluate its effect on an experimental fracture healing model. The applied dose was at 1.5MHz, pulsed, 20 minutes a day for 20 days at a low intensity of 30mW/cm² (standard stuff). The fractures were effectively surgical osteotomies that were fixed with (small) plates and screws. Half of the 30 animals got the US and the other half were treated in the same way except that the US device was not active. The animals were sacrificed at day 22 and evaluation made of the bone state using several key outcomes including mechanical (3 point bending) radiographic and histochemical techniques. The mandibles that were exposed to the 'real' US were significantly better than those not on all three assessments. The paper includes an interesting discussion which I will leave for the interested person to go find, but this adds to the rapidly growing body of evidence that supports the use of LIPUS as an effective intervention post fracture.

Longwave US and Osteoblasts : Maddi et al (2006)

There is a relative lack of published work on longwave ultrasound, and I am frequently asked about whether LWUS is better (as claimed) or not when compared with traditional (MHz) ultrasound. This is not a clinical study and therefore is unlikely to provide evidence for the clinician that will directly inform practice, but it will contribute to the evidence out there concerning the modality – which of course is welcomed. Maddi et al (**Maddi, A. et al. (2006). Long wave ultrasound may enhance bone regeneration by altering OPG/RANKL ratio in human osteoblast-like cells. Bone 39(2): 283-8**) have evaluated the effect of this type of US on osteoblasts. Treatment was at 45kHz, 30mW/cm² for 5 minutes (single treatment) and incubated for various time periods following isonation (up to 24 hours). There was a control group that were exposed to the same procedure except that the US device (PhysAssist from Orthosonics) was not activated. The outcome measures were related to various aspects of gene expression related to osteoblast activity that would be expected during fracture repair (plenty of detail in the paper for those that want it), but essentially it was shown that there was a significant response to the US in terms of upregulation of osteoprotegerin (OPG) and alkaline phosphatase (ALP) but no change in other markers (RANKL and TNF alpha). The OPG and ALP are significantly involved in bone regeneration and may provide additional evidence as to how ultrasound – longwave in this case – might serve to influence the process of bone repair.

Shockwave for Achilles Tendon : Rompe et al (2007)

Couple of papers on Shockwave therapy – becoming more widely used by therapists rather than just be specialist (usually orthopaedic) practitioners. This is a 2007 paper by Rompe and associates – a name that will be familiar to many of you I guess. (**Rompe, J. et al. (2007). Eccentric loading, shock-wave treatment, or a wait-and-see policy for tendinopathy of the main body of tendo Achillis: a randomized controlled trial. Am J Sports Med 35(3): 374-83**). Rompe has provided several papers along a similar theme one way or the other over the last few years, and this one is interesting in that it compares 3 different interventions : eccentric loading : shockwave therapy and 'wait and see'. It was a decent sized trial with 75 patients all of whom presented with chronic and recalcitrant Achilles tendinopathy (the one we all love to hate!) and all patients had been unresponsive to previous therapy. There is a detailed analysis of the patient profiles in the paper. The eccentric and shockwave therapies are additionally detailed. The shockwave intervention consisted of 3 applications at weekly intervals with 2000 pulses at each session. The results were analysed on an intention to treat basis and the main outcomes were assessed at baseline and at 16 weeks and included a specific TA pain score (VISA-A), algometry and analgesic consumption together with US scan. The results summarise thus : Pain scores significantly improved for both the eccentric exercise and shockwave groups compared with baseline and were significantly better than the wait and see group. Pain levels improved in all groups, but again, more so in the treatment groups compared with controls. Tenderness improved in all groups at 16 weeks, but in this case, there was no significant difference between the groups, and the US assessment of tendon diameter showed no significant changes. The authors conclude (reasonably) that both the eccentric exercise group and the shockwave group made significant improvement over the wait and see group, but that there was no significant difference between the two treatment group outcomes. There are some very interesting points in the discussion, and well worth a read for anybody involved in either eccentric loading or shockwave therapies related to chronic TA patients.

Shockwave for Plantar Fasciitis : Kudo et al (2006)

The second shockwave paper for this issue is from a research group in Canada using a multicentered placebo controlled RCT design for shockwave therapy with a group of patients with 114 subjects with chronic plantar fasciitis (**Kudo, P et al. (2006). Randomized, placebo-controlled, double-blind clinical trial evaluating the treatment of plantar fasciitis with an extracorporeal shockwave therapy (ESWT) device: A North American confirmatory study. J Orthop Res 24(2): 115-123**).

The authors used morning (walking pain) as a primary outcome, comparing shockwave with placebo intervention. As with the previous study, the patients had a long standing problem (6 months plus in this case) which were also recalcitrant to therapy. Patients attended for a single treatment session (series of some 3800 shocks delivered in a particular pattern – detailed in the paper) and the placebo group clearly received no actual treatment though they thought that they did. The main comparison was between baseline measures and those taken at 3 months, and there was a significant difference in favour of the shockwave group in terms of pain during the first part of walking. There were other additional significant differences in favour of the treatment group and the details provided include data analysis at 3-5 days, 6 weeks as well as 3 months post intervention. This is an interesting study and one that adds to the growing body of

evidence supporting the use of shockwave for a number of chronic musculoskeletal conditions including tennis elbow, supraspinatus, TA and plantar fasciitis.

TENS vs Interferential for Pain Relief : Shanahan et al (2006)

This study from Australia is one that has been needed for some time, and throws up some fascinating outcomes which (I think) reflect patient views and challenges therapist views at the same time (***Shanahan, C. et al. (2006). Comparison of the analgesic efficacy of interferential therapy and transcutaneous electrical nerve stimulation. Physiotherapy 92: 247-253.***

The logic is simple and the trial a straightforward but effective design involving 20 healthy subjects in a laboratory based setting. Essentially, subjects attended on two separate occasions and once they were treated with IFT and once with TENS using a repeated measures randomised design. There was not a control condition (which in some ways is a bit of a shame) but the authors rationalise why this decision was made – basically on the basis that both therapies had been previously shown to be effective – it was the comparison between them that was needed. The pain was generated using an established cold immersion mechanism (detailed nicely in this and other papers) and the primary outcomes were the time to pain threshold and the pain intensity and unpleasantness. Both the TENS and the IFT were applied at 100Hz, the IFT being delivered on a pre-modulated basis (2 pole) with the electrodes placed anterior and posterior mid forearm (the hand was subjected to the cold immersion. I'll not detail the experimental cold immersion technique as it has been used and widely reported previously, but if you are not familiar with it, then it is well explained in the paper. The results summarise like this : Pain threshold increased under both conditions, but the TENS was clearly more effective (almost 2 fold in fact). The pain intensity results were similar in that both modalities were effective, but in this case, there was no significant difference between them, though a trend for the TENS to have the advantage. Pain unpleasantness was also reduced in both groups to a similar extent. What is interesting and very insightful was the subjects perception of the intervention in that a majority found the IFT more comfortable and more effective than the TENS (63%) (21% thought the reverse and 16% reported no difference. The discussion is worth a read, but essentially the results show that both interventions were effective at achieving a change in pain induced in this way, with the TENS being objectively the more effective even though there was a majority 'preference' for the IFT. There are some methodological issues raised by the authors which are worthy of note for anybody planning a study along these lines, but in terms of clinical practice, the implication is that under these conditions, TENS had the edge even though in practice, IFT is more commonly employed in the clinic, and TENS more commonly used for home based therapy. The preference for the IFT in terms of subject perception concurs with the anecdotal evidence found in practice – the patients prefer the IFT whilst the objective evidence supports the use of TENS. Interesting and worthy of further debate I would suggest.

Next come a series of papers all dealing with hyperalgesia one way or another which I will summarise rather than go into extensive detail.

TENS for Primary Hyperalgesia : Vance et al (2007)

The first paper from Physical Therapy earlier this year (***Vance, C. et al. (2007). Transcutaneous electrical nerve stimulation at both high and low frequencies reduces primary hyperalgesia in rats with joint inflammation in a time-dependent manner. Phys Ther 87(1): 44-51***) describes an animal study (rats) in which a primary hyperalgesia was brought about using an irritant (kaolin and carrageenan) injected into the knee which results in a significant inflammatory response (a well established experimental acute pain / inflammatory arthritis animal model).

The primary hyperalgesia (main outcome) was determined as the response to pressure applied at the knee before and 4hrs, 24hrs and 2 weeks after the inflammatory induction, and compared with a real and a sham TENS treatment. The TENS was applied at either 100Hz or 4Hz – i.e. classic high and low frequency applications. Essentially the results indicate that both the TENS interventions were effective at the 24hr and 2 weeks points but not the 4 hour time frame. Although both treatments were significantly beneficial, there was no effective difference between them at the 24hr and 2 week points.

Whilst it is acknowledged that this is (a) an animal model experiment and (b) involves an induced inflammatory response rather than a 'naturally occurring pathology', the results are of interest.

TENS and Hyperalgesia : Liu et al (2007)

The second paper in this set comes from China and again involves a rat experimental model, but this time with chronic as opposed to an acute hyperalgesia (*Liu, H. et al. (2007). Repeated 100 Hz TENS for the Treatment of Chronic Inflammatory Hyperalgesia and Suppression of Spinal Release of Substance P in Monoarthritic Rats. Evid Based Complement Alternat Med 4(1): 65-75*) and the authors have published (in Chinese) on this topic previously

An established chronic joint inflammatory model (ankle) was used and the study aimed to establish the optimal TENS parameters using 100Hz repeated application and evaluating spinal substance P as a primary outcome. This was a complex design, but involved TENS, always delivered at 100Hz but with variations of intensity (low medium and high) and also variation of treatment frequency (daily, twice weekly or 5 times weekly). A wide range of outcome measures were employed including behavioural observation, ankle circumference, leg withdrawal reflex and an arthritic flexion pain test, and in addition, X ray examination. This is too much to explain in detail here, but the original paper has sufficient detail and rationale. The ankle circumference and other tests detailed above was used as an index of the acute / chronic nature of the induced arthritis. The TENS results summarise like this : In the more acute phase, TENS twice a week was most effective whilst in the chronic (stable phase) – between 4 and 9 weeks – TENS once a week appeared to be the most effective. Considering the intensity differences, the low intensity TENS was more effective in the chronic (stable) period than the medium or high intensity groups. Clearly, the results are a lot more complex than this brief summary, with several levels of interaction, but it is interesting to note these key findings.

TENS and Chronic Hyperalgesia : Ainsworth et al 2006)

The last of the TENS and hyperalgesia papers comes from a research group in the States (*Ainsworth, L et al. (2006). Transcutaneous electrical nerve stimulation (TENS) reduces chronic hyperalgesia induced by muscle inflammation. Pain 120(1-2): 182-7*) and is in a well known and highly reputable journal.

This was another rat based animal model study, but in this instance the inflammatory response was generated in muscle (gastrocnemius) rather than in a joint as per the previous two papers. Having generated a significant inflammatory muscle lesion, TENS was applied either at high (100Hz) or low (4Hz) frequency for 20 minutes using a contralateral or ipsilateral treatment approach (plus a no TENS group). The injected carrageenan (unilateral) brought about a significant bilateral mechanical hyperalgesia (which is an interesting issue in its own right). The TENS intervention at either high or low frequency significantly reduced the mechanical hyperalgesia whether applied ipsilaterally or contralaterally and therefore it is proposed that there must be a central mechanism to explain this combined response. While I would have expected the bilateral effectiveness of the low frequency TENS (anticipated opioid mediated effect) I was impressed that the high frequency application also demonstrated a significant bilateral effect. Although one has to be careful as ever with the transfer of data from animal model experimental pain to the patient / clinical environment, this data provides some interesting and insightful material which may extend out concepts of TENS physiological intervention and central control systems.

Three more TENS papers, but not experimental hyperalgesia for these.

TENS and Median Nerve Thresholds :Dean et al (2006)

This paper was presented as a short communication last year and many of you will have come across Mark Johnson in relation to the both TENS and IFT papers in recent years (*Dean, J. et al (2006). The effects of unilateral transcutaneous electrical nerve stimulation of the median nerve on bilateral somatosensory thresholds. Clin Physiol Funct Imaging 26(5): 314-8*).

The study involved 16 healthy volunteers who were subjected to a TENS treatment (at 100Hz) over the median nerve in the forearm. Various sensory thresholds were evaluated in the thenar eminence of the ipsilateral and contralateral

Seen any interesting papers?

Is there a paper that you have written and ought to be reviewed here?

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hands following a 10 minute application. The measurements related to pressure (von Frey hairs) sharpness, warmth, cold and heat pain. The von Frey filaments were used to determine mechanical perception threshold, weighted needles used for sharpness threshold. The thermal thresholds were measured for innocuous heat and cold and for thermal pain threshold. The detailed procedures are detailed to some extent in the paper and x referenced to other studies. The TENS was applied over the (R) median nerve at the wrist at 100Hz and at 200microsec pulse duration for 10 minutes. Each subjected acted as their own control, comparing pre – post measures. The TENS intensity was sufficient to bring about a strong but comfortable paraesthesia (which is clinically appropriate). Measures were taken before, during, 10 minutes after and 30 minutes after stimulation on both the (R) (ipsilateral) and the contralateral sides.

Given the number of thresholds measured on both hands at repeated time points, there are a lot of results to work through, but I will summarise the essential ones here. The quick one to deal with was the contralateral measures in that there were no statistically significant differences in the pre - during measures on the contralateral hand but than the cold threshold was raised at the 10 minute post measure compared with the pre measure.

All other results identified here relate to the ipsilateral data (i.e. from the stimulated (R) hand). Ipsilateral touch threshold (von Frey) increased during TENS, at 10 mins post but the difference was negated at 30 minutes post. Sharpness threshold increased during the TENS but not post 10 minutes. The warm and cold thresholds did not change significantly. The hot pain threshold was raised during and at 10 minutes post treatment, but did not reach statistical significance. The authors urge caution as this was an exploratory study and would need to be repeated with a larger group for the power to be increased, but the preliminary findings do illustrate that the use of TENS appears to elevate somatosensory thresholds in the distribution of the stimulated nerve, and although the largest effects occur during the stimulation period, there are some changes that persist beyond this time frame.

TENS for Low Back Pain in Multiple Sclerosis : Warke et al (2006)

This is a paper from the Ulster camp and I think it is part of a larger study on low back pain in MS patients (**Warke, K. et al. (2006). Efficacy of transcutaneous electrical nerve stimulation (tens) for chronic low-back pain in a multiple sclerosis population: a randomized, placebo-controlled clinical trial. Clin J Pain 22(9): 812-9**). The study was evaluating the potential benefit of self applied TENS for LBP in this patient group and involved 90 patients divided into three groups (low frequency TENS at 4Hz; high frequency TENS at 110Hz; placebo TENS). Patients self managed their TENS, using 2 x 45minute sessions a day (minimum) for 6 weeks. There was an extensive follow up in that in addition to the expected outcome measures pre and at the end of treatment (6 weeks) there were additional measures at 10 and 32 weeks. There were 2 primary outcomes (VAS for average LBP and McGill pain questionnaire) plus 9 additional (secondary) measures.

In short, the results were not statistically significant but the authors note that the effects that were reported were possibly important for the patients, and it is interesting that this paper may contribute to the debate regarding the difference between statistical and clinical significance – my comment rather than theirs).

Some of the key findings are : average LBP (primary outcome) – this decreased in all groups over the time frame and the decrease was greatest in the high TENS group. Similarly, with the McGill data, all three groups showed improvement and again, the high TENS group did better than the others even though the difference was not statistically significant.

Of the other outcomes, worst LBP was most influenced by high TENS though the low TENS group came through to a good result by the 32 week follow up. The spasticity scores also showed better change in the high TENS group, but again not a significant result. The low TENS group showed some improvement on the Barthel Index and Roland Morris scores. Patients in all groups recorded a perceived change in physical and mental health and this was stronger in the 2 treatment groups compared with the placebo group.

The authors provide a useful and informative discussion which is well worth a read (I always say that –I know! – but it is). Although the results are not reaching statistical significance, this does not mean that it is a waste of time to try TENS for low back pain for patients with MS, and as identified previously, there is a fundamental and an important difference between statistical and clinical significance.

TENS post stroke – Cognition and Emotional Aspects : Rorsman and Johansson (2006)

The next electrical stimulation paper this time around is a bit different from normal – though interestingly, I did have something similar in a previous edition with the use of electrical stim for severe behavioural disorders. Anyway, this one from a research group in Sweden looks at the use of electroacupuncture and TENS post stroke in relation to cognitive and emotional outcome (**Rorsman, I. and B. Johansson (2006). Can electroacupuncture or transcutaneous nerve stimulation influence cognitive and emotional outcome after stroke? J Rehabil Med 38(1): 13-9.**

Basically a group of (54) patients were treated post stroke with one of three interventions : acupuncture / electroacupuncture ; TENS high intensity, low frequency ; TENS low intensity, high frequency. Treatments were carried out twice weekly (20 sessions over 10 weeks) starting 5 – 10 days post stroke.

The acupuncture/electroacupuncture details are provided in the paper, and I'll not reproduce the list of points used here. Essentially 9 or 10 points were employed and had been used in a previous study by this group. The high intensity, low frequency TENS (considered to be potentially similar in action to the electroacupuncture intervention) was used with 2 electrode pairs operating at 2Hz at an intensity sufficient to bring about muscle contraction. The electrodes were placed relative to the acupuncture points used in the first group.

The third group were also treated with TENS but at a low intensity – actually sub threshold – and a high frequency (80Hz) with the electrodes in the same location. This was in effect used as a control group. In addition to the experimental intervention, all patients were in receipt of 'regular' physiotherapy, occupational therapy and speech therapy.

The outcomes recorded were wide ranging and included ADL assessed with the Barthel Index, a Rivermead Mobility Index, walking capability and a raft of outcomes related to cognition and emotional status (3 tests for emotional status and 8 for cognition). These assessments were made at baseline and at 3 and 12 months post stroke.

With 3 treatment groups and that number of outcome measures, you can imagine the fun (sic) that could be had with the results and analysis. For the sake of brevity and sanity, I'll not detail them all here – if you are interested, as ever, head for the original. At baseline, the ADL and physical outcomes were not significantly different between groups. In terms of cognition, the control group were actually somewhat worse off at baseline (this might have been an issue for design improvement with some stratified randomisation to provide more equity at the start?) but at the end of the day, there was no significant difference in cognition changes between the groups. There were improvements on overall cognition for all groups, and at various time points, there were differences between various test outcomes and group allocation BUT in the follow up period (3 and 12 months) there were no significant differences. With regards the emotional functioning, there were no differences between groups (i.e. no treatment effect) although again all 3 emotional outcome measures improved between baseline and 12 months.

It would seem therefore that neither electroacupuncture nor high intensity TENS when applied in this was shortly following stroke has a significant effect on cognition or emotional status of the patient. There are a number of design issues, and the raft of outcomes makes for some hairy analysis and big data tables that are not exactly inspiring. There have been previous studies that have identified encouraging results in terms of both cognition and emotional change with electrical stimulation, and I would have thought that there was some mileage in pursuing this further. The outcome of this particular trial does not mean that the intervention (e stim) is ineffective, but does mean that when used in this particular way, it does not achieve significant change over and above what was likely to happen anyway. There is an interesting and extensive discussion in the paper and you will get a whole lot more out of it by reading it than relying on me to try and summarise it in a couple of paragraphs!

Quads NMES to Quads Post Total Knee : Petterson and Snyder-Mackler (2006)

Heading away from the TENS papers and into some on NMES issues – the first one on quadriceps function and NMES post total knee arthroplasty (**Petterson, S. and L. Snyder-Mackler (2006). The use of neuromuscular electrical**



stimulation to improve activation deficits in a patient with chronic quadriceps strength impairments following total knee arthroplasty. J Orthop Sports Phys Ther 36(9): 678-85).

This is a case study rather than any kind of controlled trial, but nevertheless, it provides some useful data (in my opinion anyway, no matter what the Cochrane crew might say!). The 'case' was a patient who had undergone bilateral total knee arthroplasties and the 'treatment' described here was some 12 months down the line. The patient presented with a very significant deficit on quads strength (26%) on the (L) compared with the (R) and a 13% reduction in activation. There was no way of comparing this to normal of course given the bilateral surgery. NMES was used in combination with a weight training programme over a 6 week period (16 treatment sessions) and there was a 25% improvement in strength by the end of the treatment phase and also a significant improvement in quads activation. Follow up at 18 and 24 months demonstrated sustained improvement and in fact at the 24 month assessment, the (L) quads – previously weaker – were actually 6% stronger than the (R). The stimulation was applied along the lines of previously conducted research and essentially, two large electrodes were employed (over rectus proximally and V medialis distally) and a stim frequency of 2500Hz set with 75Hz bursts for 10 seconds followed by an 80 second rest period at an intensity set to 30%MVIC output (patient received stim whilst attached to an isokinetic device). Treatment was for 10 contraction periods per session.

It is of course not possible to say with this case study whether it was the NMES, the weight training or both (most likely) that contributed to the clinical change, but it raises an interesting issue in that the combination of exercise and NMES appears to provide a clinical advantage to either intervention in isolation (plenty of previous work in this area). Although this particular programme was carried out as part of a clinic based intervention, my personal view is that one of the potential benefits of this type of combined approach lies with the patient using a small, inexpensive, battery powered NMES device as an adjunct to a home exercise programme – the patient gets on with it after appropriate instruction. From the previously published work, it does not appear to be essential to use big mains powered devices, and it would enable patients to also gain more from their resource limited treatment sessions in the clinic in that valuable treatment time is not used doing something that could be patient driven therapy outside the clinic. I know that there are those that disagree fundamentally with this philosophy, but when healthcare resources are limited and the patient is competent to make an active contribution to their 'treatment' outwith the clinical setting, it occurs to me that this might be an effective way of helping to manage a limited resource.

Whatever your views on this (e mails welcomed as ever), this case study is useful and certainly adds to the 'evidence' around out there.

NMES and Ankle Swelling : Man et al (2007)

Ivy Man completed her PhD at Kings looking at ankle swelling and NMES, and this paper challenges some of the widely held views about ankle swelling and NMES intervention (***Man, I. et al. (2007). Effect of neuromuscular electrical stimulation on ankle swelling in the early period after ankle sprain. Phys Ther 87(1): 53-65).*** A group (n=34) of patients were treated following ankle sprain (within 5 days) with the NMES applied to the gastrocs and tibialis anterior for 3 sessions. Patients were randomly allocated to one of three groups – NMES, sub-threshold NMES and a sham NMES group. Outcome measures for swelling included volumetric data and limb girth (both (L) and (R) limbs) and an ankle rating scale. The electrical stim pattern was complex and is not easily described in terms of simply frequency and pulse duration. The parameters are detailed in the paper and a figure is used to try and clarify the protocol. It is difficult to ascertain why this complex pattern was employed other than that it was available on the stimulator being used. The 'average' stim frequency was at 80Hz, but this is far too simplistic as a description of the reality of the patterned pulsing as there were burst and repeat cycle patterns within the automatic programme. The NMES group had the intensity turned up to tolerance level, the sub motor NMES group had it turned up till a flicker appeared and then turned down until it had disappeared. The sham NMES group went through the same procedure but no actual current was applied, and the patients were told not to expect to feel anything. Treatments were for 30 minutes. Three attendances were scheduled (usually on consecutive days) at which outcome measure tests were repeated. Analgesic consumption and other treatments used were also recorded at each session.

There were some statistically significant differences between the groups at baseline which (as per the comments for the previous study in this edition) is a shame as it may have masked some of the outcome – and certainly added to the list of confounders in this case. There are a LOT of results in this paper with stats tables galore presented, but essentially, there were no significant differences between the groups in terms of ankle volume but there were significant changes in ankle girth over time for the NMES group but not for the sub-motor NMES or sham groups. The rating scales showed a significant change from test 1 through to test 3 but not a significant difference between groups – i.e. all patients got significant improvement on the rating scales with time no matter which treatment group they were allocated to.

There are several potential confounding variables that may have influenced these results, not least of which was the inequality of the groups at baseline, and the authors deal with some of these issues in the discussion. The overall outcome of this work – that using NMES to anterior and posterior leg muscles for 30 minutes on three occasions using a patterned NMES device does not provide significant reduction on ankle swelling (objectively or subjectively). Looking at Figure 3 (page 61) provides a quick summary of the outcome in that you can see the ‘improvement’ in both the NMES and sub-motor NMES groups in terms of ankle volume, but the standard deviations are big, and the start conditions unequal, hence the non significant statistical results. There are issues with regards the stimulation protocol and some aspects of the design, and the discussion is well worth a read for anybody contemplating further research in this area. Given the outcome of this particular study, there does not appear to be anything convincing about using this protocol of NMES for this client group in the early phase post injury.

Case Report – Burn following IFT : Ford et al (2005)

Only one interferential paper in this issue – a case report relating to a skin burn experienced by a patient treated with NMES and IFT following unicompartmental knee arthroplasty (**Ford, K. et al. (2005). Full-thickness burn formation after the use of electrical stimulation for rehabilitation of unicompartmental knee arthroplasty. J Arthroplasty 20(7): 950-3).**

The patient presented some 4 weeks post surgery with knee pain and swelling and on examination there was warmth, tenderness and some effusion and limited range with quads atrophy and weakness. In addition to exercise, the patient was treated with NMES for the swelling and IFT for the pain. The NMES electrode application was over the quads and the IFT used electrodes over the knee joint area and proximal tibia. No treatment parameters are reported (other than electrode position – photos) in the text, though as three of the 4 authors are MD’s rather than PT’s, this might explain why it was not mentioned – though still a shame.

The patient appears to have sustained a full thickness skin (electrical) burn around the most distal electrodes used for the interferential (looks like it was over the tibial tuberosity) although there was an improvement in quads function and a reduction in swelling/effusion. This did heal up with conservative management by the 6 month follow up.

The patient appears to have had some sensation deficit in the area, and this combined with the electrode position are likely to be the main contributors to the problems experienced. NMES and IFT are not contraindicated when there is metal in the tissues (i.e. the knee arthroplasty in this case), but current precautions and contraindications guidance does indicate that skin sensation testing (sharp/blunt) should be carried out and abnormal sensation would preclude electrode positioning in these areas. The electrode placement combination appears to be somewhat strange for knee pain anyway, and the authors suggest that IFT is perfectly acceptable in these circumstances (I would agree), but a more appropriate electrode positioning combination, avoiding superficial bony areas and areas directly over the implant would be preferable. The recommendation (photo) seems to be what I think is more normal practice anyway. I would add one further thing that is not mentioned in the paper – unless I missed it – that the applied electrodes were very small, and hence the current density would have been high. This might have made a significant contribution to the problem, and in compromised tissue (whether due to sensory problems or tissue state) it would be prudent to use larger electrodes, enabling the same current to be applied, but at a lower current density, and hence with less risk to the tissues.

The paper need not make therapists back away from NMES or IFT or any electrical stimulation around an arthroplasty or other passive metal implant, but reasonable caution needs to be employed, and delivered properly, electrical stimulation in such circumstances can be both effective and safe.

Systematic Review : Iontophoresis and Anti Inflammatories : Hamann et al (2006)

I do get several e mails every week asking why I don’t have iontophoresis as a topic up on the website, and I know it deserves to be there, but time is limited, and it is on my ‘to do’ list – just has not reached the top yet! Anyway, this paper would make a useful read for anybody with an interest in this field : (**Hamann, H, et al. (2006). Effectiveness of iontophoresis of anti-inflammatory medications in the treatment of common musculoskeletal inflammatory conditions: a systematic review. Physical Therapy Reviews 11(3): 190-194).**

Although iontophoresis is not widely practiced in the UK and some other countries, it is routinely used in the USA and elsewhere, and the use of direct currents to facilitate the delivery of various medications and substances across the skin and into the tissue has a substantial literature base, though not all of it is research based.

This review examines some of that literature – concerning the use of this modality as a means to deliver anti-inflammatory medication. The review method employed a system described by Guyatt as a means to identify whether the reviewed data constitutes a reason to change practice – i.e. a scale that provides an evaluation of the quality of the presented evidence, and is worthy of consideration outside the context of iontophoresis and anti-inflammatories.

Eleven papers (human clinical trials) are reviewed in this publication (1990 – 2005) looking at the combination of ionto, anti-inflammatory and musculoskeletal conditions (like plantar fasciitis, Achilles tendonitis, carpal tunnel etc). Some of the evaluated studies failed to meet the 'acceptability' criteria of the Guyatt system, and the review describes the salient points of each paper (not going to repeat them all here – the newsletter would be a million pages long if I did that for every paper!), but at the end of the day, the authors conclude that the available evidence for iontophoresis and delivery of anti-inflammatories is limited, and almost predictably, there is a need for further quality studies with larger patient numbers.

Laser and Chronic Neck Pain : Chow et al (2006)

There are two laser based papers in this issue, and the first one (***Chow, R. et al. (2006). The effect of 300 mW, 830 nm laser on chronic neck pain: a double-blind, randomized, placebo-controlled study. Pain 124(1-2): 201-10***) reports the outcome of a particular laser therapy intervention on 90 patients with chronic neck pain, divided into treatment and sham treatment (control) groups. The primary outcome measure was a VAS pain rating and was accompanied by a range of secondary outcomes (5 of them) including SF-36 and McGill Pain Questionnaire. The therapy was delivered to tender areas in the neck with 14 treatment sessions over a 7 week period, and assessments were made at baseline, at the end of the treatment phase and at 12 weeks from baseline.

The short version of the results are that there was a significant reduction in VAS Pain for the treatment group compared with the sham group and almost all the other measures also showed significant improvement.

Chronic neck pain in this context means more than 3 months duration and could be unilateral or bilateral in nature, but with a range of detailed exclusion criteria (listed in the paper). The laser was infra red (by virtue of the 830nm wavelength) and delivered a continuous output with a power density of 0.67W/cm² (300mW power). Tender points were identified by following a systematic palpation protocol and each identified tender point was treated with the laser for 30 seconds, with up to 50 points being treated (within the max 30 min treatment session). It was determined prior to the commencement of the trial that in order to see a 2 point change in VAS – a clinically relevant change – 45 patients per group would be needed (allowing for patient drop out).

The results are nicely described in the text and clearly laid out. As summarised above, the VAS reduced significantly in the treatment group when compared with the sham group, and furthermore, only one patient in the treatment group experienced worsening of symptoms compared with 12 in the sham group. The results indicate that the benefit of being in the treatment group compared with the sham group is approximately eight fold (impressive) and it was also interesting that the regression results indicate that the patients who were worse off at the start of the trial were those that demonstrated the greater the odds of an improvement. The changes in the secondary outcomes are also detailed, but for the most part, the treatment group improvement was significantly greater than that in the sham group

This intervention was delivered in isolation (so far as I can see) which is not necessarily the clinical norm, but in order to determine whether it was the laser therapy or the 'other intervention' that was effective, this was an intentional design feature. The outcome does certainly indicate that laser therapy, employed in this was is effective for patients with chronic neck pain, and is significantly better than a sham therapy, and thus is worthy of consideration as a therapeutic option for patients in this group. Nice study, well presented and easy to read.

Review – Laser and Acute Pain : Bjordal et al (2006)

Bjordal it is again then! In combination with Mark Johnson et al – should be a good one. This systematic review (***Bjordal, J. M., M. I. Johnson, et al. (2006). Photoradiation in acute pain: a systematic review of possible mechanisms of action and clinical effects in randomized placebo-controlled trials. Photomed Laser Surg 24(2): 158-68***) looks at the effect of 'photoradiation' on acute pain following soft tissue injury.

The essential conclusion is there is strong evidence that photoradiation modifies the inflammatory process and achieves pain relief. The outcomes appear to be dose dependant (which is a subject dear to my own research and is the subject of a paper at the forthcoming WCPT). 19 of the 22 controlled studies provided quality evidence that inflammatory modulation resulted from the laser therapy with changes in a variety of cytokines and chemical mediators of the inflammatory events. The median dose applied in order to achieve these results was 7.5 J/cm² (sorry about no superscript for these doses, but I KNOW it is going to make electronic transmission a nightmare!!!) and furthermore, doses at under 5J applied at a single point over the tissue were not effective – dose dependency is a real issue. There are lots of other interesting bits in this review including some meta-analysis type work and trial data combinations, again showing significant benefit from treatment groups compared with control or sham groups.

The authors have clearly spent a lot of time and made a lot of effort to compile this review and I'll not do them any injustice by trying to condense their 10 journal pages into 2 paragraphs. If you use laser therapy as a part of your practice – soft tissue injury or pain management or both – this is a quality (and essential) read and I would strongly encourage you to get hold of the original and see what they have to say in the detail

Cold and Compression : Janwantanakul (2006)

Couple of papers on cold therapy – I know it is not electrotherapy, but usually gets bundled in with the 'electrophysical agents'. The first looks at the effect of compression on skin temperature when using cold application (**Janwantanakul, J. (2006). Cold pack/skin interface temperature during ice treatment with various levels of compression. *Physiotherapy* 92(4): 254-259.**

Janwantanakul has published on cold therapy previously, and this was a lab based study with 40 'normal' subjects having an ice pack applied to their anterior thigh (20 mins) with varying levels of compression ranging from 0 to 44mmHg using an elastic bandage (each subject therefore attending on 5 occasions with at least 1 day between). The primary outcome was skin surface temperature measurement and the results indicate that the addition of compression does significantly reduce the skin temperature, but it does not depend on the amount of compression applied. The time taken to reach the minimum temperature does however depend on the level of compression. In effect therefore, the skin temp decreases more with compression added, and the more compression that is applied, the faster the drop in temperature is achieved.

One has to be careful about the direct transfer of healthy subject laboratory based studies to the clinical environment. Much as one might assume that similar results are likely to be achieved clinically, there is no guarantee, and in a recent trial that we did on pulsed shortwave (coming out soon), we evaluated the difference between a lab study on normals and exactly the same intervention in a clinical group – and got different results! The results however do add to the all too limited knowledge that we have regarding cold therapy and physiological effects of changing treatment parameters

Cold, Microcirculation and Soft Tissue Injury : Schaser et al (2007)

The second cold therapy paper in this issue is from Germany (**Schaser, K. et al. (2007). Prolonged superficial local cryotherapy attenuates microcirculatory impairment, regional inflammation, and muscle necrosis after closed soft tissue injury in rats. *Am J Sports Med* 35(1): 93-102**) and looked at the effect of cold therapy on microcirculation in a lab based (animal model) study.

The animals were divided into two groups – treatment and control – and the closed soft tissue injury was generated using a pneumatic bolt fired against the muscle, providing a severe but no lethal injury. Animals were treated with 6 hours cryotherapy (at 10 degrees C – based on previous research) or not following injury. Various outcomes were recorded including intramuscular pressure, heart rate, blood pressure, local (muscle) microcirculation, oedema formation, tissue necrosis, leucocyte and macrophage numbers and behaviour.

The injury itself brought about significant capillary density and increased microvascular permeability, increased leucocyte adherence, oedema formation, increased cell counts for macrophages and tissue necrosis. The application of 6 hours cryotherapy significantly reduced the impact of these changes and the authors propose that the effect of the cold therapy is to reduce post traumatic microcirculatory dysfunction, reduce the inflammatory response and results in less structural damage in the tissue. These claims appear to be supported by the experimental data.

Following soft tissue injury (especially sports related injury) prolonged cooling is becoming a popular intervention (rather than the classic 10 minutes ice pack). It has been suggested for some time that the benefits of this type of intervention might relate more to microcirculatory and tissue hypoxia rather than simple pain relief and this animal experimental work would appear to support this intervention, though again, with the limits of transferring animal lab results to the clinical environment – though one can see the immediate problem of trying to run this study on the sports field with traumatic muscle injury of this type being somewhat unpopular with the volunteers!!

Heat and Supraspinatus Tendinopathy : Giombini et al (2006)

Quick change from cold therapy to heat therapy for the last one in this section. A paper from last year looking at hyperthermia (microwave or ultrasound) in supraspinatus tendinopathy (**Giombini, A, et al. (2006). Short-term Effectiveness of Hyperthermia for Supraspinatus Tendinopathy in Athletes: A Short-term Randomized Controlled Study. Am J Sports Med).**

The basic premise being tested was that there was no difference whether one used hyperthermia, ultrasound or exercise for patients with chronic (3-6 months) supraspinatus tendonopathy in an athlete population (n=37 subjects). The main outcome measures were pain (VAS score), pain or resisted movement, painful arc presentation and shoulder function on activity.

The three groups each received a different treatment approach. The hyperthermia group were treated with an electromagnetic device operating at 434MHz (microwave range) for 12 sessions (3 times a week for 4 weeks) for 30 minutes using an applied power of between 50 and 70Watts through a variable thickness water bag. The ultrasound group were treated with 1MHz US, continuous at 2.0 W/cm² with gel couplant for 15 minutes, 3 times weekly for 4 weeks. The exercise group were given a range of shoulder exercises to perform and attended the department on a weekly basis. No other interventions were employed nor allowed during the trial period. The hyperthermia (microwave) and ultrasound devices are detailed in the paper.

Results are based on the change from baseline to the end of treatment (4 weeks) and also at a 6 week post end treatment follow up. The microwave group showed significant reduction in pain by the end of the treatment period and by the end of the whole study (i.e. at follow up) 12 of the 14 patients in the microwave group had returned to their sport compared with 4 out of 12 in the ultrasound group and 4 out of 11 in the exercise group. The shoulder function assessment also showed significant improvement for the microwave group. Interestingly, not all the outcome measures that were employed were reported in the results. It was also interesting that the tissue temperatures achieved with the microwave treatment were calculated by the machines software, though I was unable to see from the report how this was achieved, and not having used or seen the machine, I am unable to pass on that information. If you are interested, you might like to contact the authors or manufacturers.

Essentially, the clinical trial supports the use of a heating based treatment for this clinical problem. Ultrasound was deemed to be ineffective, as was exercise, but as I have written numerous times, ultrasound is incredibly inefficient as a thermal modality, and microwave is very good at achieving real heating effects in the tissues – so much so, that several of the participants in this study identified discomfort during the treatment.

Heat therapy appears to be somewhat less fashionable than it was several years ago which is a bit of a shame really as it has significant physiological effects and useful clinical outcomes. Microwave based therapy is likely (based on the biophysics) to be one of the most efficient thermal treatment modalities, though many clinical departments that had one year ago have abandoned the modality (Pope et al, 1995; AlMandil and Watson 2006 for example). If heat based therapy is effective for chronic tendon problems, then microwave is likely to be useful. A more realistic clinical comparison I would have thought would be between it and say shortwave or high dose pulsed shortwave in order to determine relative efficacy. To compare it with an inefficient thermal modality is useful in some sense – it demonstrates that US is not good as a thermal modality – but not really a like for like comparison.

Magnetics and Osteopenia : Taniguchi et al (2007)

Two papers on magnetics one way or the other, couple on tissue repair and then that will be that for this issue. The first of the magnetics papers is by a research group in Japan (**Taniguchi, N. and S. Kanai (2007). Efficacy of Static Magnetic Field for Locomotor Activity of Experimental Osteopenia. Evid Based Complement Alternat Med 4(1): 99-105**).

This was an animal model (rat) experiment using a static magnetic field therapy for one group of ovariectomized animals, no magnetic therapy is a further group of ovariectomized animals, and neither magnetic therapy nor ovariectomy (true control) Clearly the effect of the ovariectomy is to reduce the bone mineral density (assessed by DEXA). The treatment group were managed for 12 weeks post operatively and the outcome was the reduction on bone mineral density (BMD) that was observed in the non treatment group. There were other observed advantages in the magnetic field treatment group, and the reason for including this paper in the newsletter is to raise the issue (again) that static magnetic fields have been expected not to have a significant physiological effect, and that dynamic magnetic fields have been thought more likely to be effective. It may be true that the dynamic fields have a greater effect (this paper did not set out to evaluate such an hypothesis), but there certainly appears to be a real and measurable effect of a static magnetic field – and I have reported some other trials along these lines in previous editions.

The potential benefits of magnetic type therapies are beginning to gain scientific credibility, and although there are clinical magnetic therapy devices already available on the market, the therapy based clinical research is still lacking in several areas. This at least adds to the evidence base and might provide a useful platform from which the next piece or work can start. A second static magnetic field paper looking at vascularisation in an rat bone ischaemia model might be of interest and can be found at : **Xu, S., N. Tomita, et al. (2007). "Recovery of Small-Sized Blood Vessels in Ischemic Bone Under Static Magnetic Field." *Evid Based Complement Alternat Med* 4(1): 59-63** though I will not review it in full here.

Magnetic Stim vs NMES for Quads : Han et al (2006)

The second of the magnetics papers links the therapy with electrical stimulation (**Han, T. R., H. I. Shin, et al. (2006). *Magnetic stimulation of the quadriceps femoris muscle: comparison of pain with electrical stimulation. Am J Phys Med Rehabil* 85(7): 593-9**) with the objective of comparing the pain experiences with magnetic stimulation versus NMES stimulation.

This was a lab based healthy volunteer study (n=17) and essentially, the stimulation mode was applied and gradually increased (in steps) whilst assessing the peak torque of the quads and an assessment of pain using a VAS scale. The main comparison was of the VAS scores of the 2 modalities when generating the same level of quads torque. Each subject recruited to the trial received both interventions with at least 24hr separation. A Cybex dynamometer was used for the torque measurements. The NMES was applied with 2 electrodes (upper V lateralis and lower V medialis) at 25Hz, 300 microsec duration using a ramped stimulation pattern and following a fixed protocol (described in the paper). The magnetic stimulation was also applied over the quadriceps using a frequency of 25Hz and with an approximately equivalent ramping and pattern procedure.

The results basically indicate that for an equivalent quads torque, the magnetic stimulation produced less discomfort/pain than the NMES stimulation and the authors go on to discuss the potential use and advantages of a magnetic based stimulation approach in the clinical setting – certainly seems to have some potential value. I have not tried this, though I did get used as a model a couple of times for magnetic stim to the motor cortex for muscle recruitment, and I can't say that I found it the most pleasant experience in the known universe. Clearly, this approach is quite the reverse, and we may end up using this in the future, especially for particular patient groups – pain sensitivity, adverse response to NMES or sensory nerve problems? Watch this space I guess. I will have a look around and see if I can find anything else along these lines and report back at some point.

Neovascularity and Tendon Problems : Fowble et al (2006)

Tissue repair to finish up with then. Angiogenic responses to injured and repairing tissues are attracting a deal of attention at the moment, and tendon pathology similarly is popular in the literature (certainly compared with a few years ago). This paper looks at neovascularity changes in tendon tissue from patients (not an animal model this time) who were undergoing surgery for posterior tibial tendon insufficiency (**Fowble, V, et al. (2006). *Neovascularity in chronic posterior tibial tendon insufficiency. Clin Orthop Relat Res* 450: 225-30**).

28 patients had samples taken and a range of parameters were evaluated including collagen orientation, vascularisation, cellularity (tenocytes) and other changes. Tissue from different zones was sampled – tenosynovial lining, subtenosynovial tissue and tendon proper. Samples were obtained from 19 patients undergoing a variety of surgical procedures, and from what I can see, all samples were taken from the 3cm proximal to the navicular insertion.

The gross findings included obvious thickening of the sheath and the tendon with tenocyte layer thickening, neovascularisation and angiogenic infiltration of the tissues. Microscopic evaluation showed neovascularisation in all specimens to varying degrees. The pictures in the paper are great if you can get hold of the pdf (some are in colour and clear changes visible).

It looks like the neovascular changes originated from the synovial lining and then infiltrated the tendon proper, resulting in collagen bundle leading to disruption of their alignment and orientation. Neovascularisation (angiogenesis) is an essential component of repair and in those circumstances appears to be both beneficial and essential to repair. In the cases described here, the process appears to be detrimental one way or another and there did not appear to be an inflammatory process present in the tested tissues from this sample. It may be that in this case the neovascular response is partly (?wholly) responsible for the degenerative changes, and therefore there appears to be good and possibly bad versions of the same soft tissue process. The discussion is a good read for those with an interest in tendon pathology and who have been aware of the tendonosis / tendonitis debate over recent years.

Effect of Stretching on Collagen and Muscle : Coutinho et al (2006)

The last one then (still over a hundred sitting in the pile though!) is looking at passive stretching and its effect on collagen and muscle fibres (*Coutinho, E, et al. (2006). Bouts of passive stretching after immobilization of the rat soleus muscle increase collagen macromolecular organization and muscle fiber area. Connect Tissue Res 47(5): 278-86*).

30 rats were studied and following immobilisation (soleus) and then the behaviour of the intramuscular connective tissue was evaluated together with muscle fibre area. The immobilisation involved fixation of the ankle in the fully plantar-flexed position for 4 weeks, and in group A, the tissue was evaluated at the end of this period. Group B had the immobilisation then 3 weeks free in their cage – with no stretching protocol. Group C had the immobilisation followed by passive stretching 3 times a week for 3 weeks. Group D got the immobilisation followed by daily stretching for 3 weeks and the final group (E) got no immobilisation and no stretching was were evaluated at the end of 7 weeks. The stretching was 10 stretching bouts, each maintained for 1 minute with 30 sec rest between (related to previous studies).

Results : The muscle weight reduced following the immobilisation, but increased in the stretching groups, though the 3 times a week stretching did not give significantly better results than 3 weeks free in the cage – daily stretching did. The muscle cross sectional area reduced significantly following immobilisation (as one might expect). The daily stretching group soleus muscle increased cross sectional area compared with the other groups. The three times a week group and those left free in the cage improved cross sectional area but there was no significant difference between them. The collagen orientation element of the study similarly demonstrated significant advantages with the daily stretching routine.

The authors conclude that short daily bouts of stretching after immobilization induced molecular reorganization of the collagen bundles and muscle fiber hypertrophy in the rat soleus and therefore there appears to be a distinct advantage to performing regular, short stretching and the patients who 'forget' may well get less good results (which we would all be aware of anecdotally) for very sound reasons. Interesting addition to the physiological evidence base relating to immobilised tissues and a paper that possibly lends additional evidence to the long held belief that active patient involvement in exercise and treatment post immobilisation is worthwhile.

OK, so I think that will do for now, or else it is going to be May before this reaches you and that is not the plan at all. Hope that something in here has grabbed your attention and that it might inspire some debate in your clinical environment. As ever, if you have spotted a good paper and I seem to have missed it, or if you have written a paper for that matter, do let me know. Keep the e mails coming in - can't promise to answer all of them immediately - there are simply too many to manage that I am afraid - but I will do my best. The next edition is due in June/July (or thereabouts)

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Regards

Tim



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