Electrotherapy News

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Hi there folks, and sincere apologies for the delay in getting this issue out to you. I had thought that I had time to put it together last month, but my real job somehow seemed to get in the way (!). Couple of new items first and then on with the recent papers.

NEWS : World Congress of Physical Therapy – Vancouver, June 2007

One of the initial reasons for the delay was that I was in Canada for the 2007 World Congress. It was a good conference and some impressive papers, presentations and posters. I have a couple of abstracts that I will include in the next issue, but in case you have any interest, I did have a presentation there myself relating to Dose Dependency in Electrotherapy, and the slides (pdf version) are up on the web site. Clearly, it will not be the same as being there, but the essential bits will be there plus the references etc. I have attached the abstract to the slides, so you can get the gist of it all. Once I have checked with the authors of some of the other papers and posters that I think might be of general interest, I will get them included in the next issue.

NEWS : International EPA Association

At the same congress, there was a proposal from 4 eminent Profs in the field (Goh, Robertson, Baxter and Kloth) to set up an International Electro Physical Agents Association come organisation. This is still in its infancy, and some details yet to be determined, but it is certainly an interesting development. It was argued that if EPA's continue to be used in practice and that they have a place in therapy, the some overarching organisation would help to support / promote / represent the interests of practitioners and researchers. There is a web site for ongoing discussions which can be found at : www.wcpt.org/smfforum/index.php/topic,208.0.html and I would strongly encourage you to have a look at what is going on and get your views heard. My thanks to Prof Goh (from Japan) for acting as the catalyst for this, and the other three for speaking so strongly in support. My personal view . . . . . is that it is a good thing and worthy of effort. Will keep you posted as news develops.

NEWS : Ibuprofen Gel Study

I mentioned in the last issue that we were running with the Ultrasound Therapy for Apomorphine Nodules study – thanks for all those that asked how we were getting on – but a bit early for results yet! Just to let you know that following our two ultrasound transmission studies from last year (refs on the web site and mentioned in previous editions of the Newsletter), we are about to start on a further ultrasound study concerning the transmission of ultrasound through

gel based coupling media which contain NSAID based drugs (mainly ibuprofen as they are the most commonly used). It is suggested that the inclusion of the drug material in the gel has the effect of reducing energy transmission, so we thought that it was about time that we put a currently used batch of gels to the test. Will let you know as soon as we have any results and will publish as soon as we can. If, in the meantime, you use such products, we would be grateful if you could let us know which one(s) so that we can test the most relevant and widely used products. We have scanned through all the major lists that we can find and have come up with a preliminary test list, but any information that you have would be gratefully received in order to ensure that we make our efforts worthwhile. If you have any information, please e mail either myself (usual address) or Amy Todd (a.todd@herts.ac.uk) who will be conducting the bulk of the research.

NEWS : TENS in Pregnancy

You have no idea how many e mails queries I get relating to the use of TENS during pregnancy and the confusion with regards the various different practices between professional groups and the conflicts in the different guidelines around out there. The Association of Chartered Physiotherapists in Women's Health (ACPWH) – a group within the Chartered Society in the UK have carried out a review of the issue and involved people with an interest in both electrotherapy and pain management. The newly published set of guidance notes concludes that although it might not be an ideal first line treatment for a patient who is pregnant, the evidence does not actually justify the inclusion of pregnancy on the TENS contraindication list. In effect, the guidance states that ' . . . . . if pain persists or is a hindrance to further improvement, then TENS may be beneficial especially where the alternative is medication that would cross the placental barrier . . . . . '

This is a very short abstract from the guidance which is evidenced, referenced and argued from a logical viewpoint. If this is a field of interest (or concern) then the organisation have put the guidance document up on their website (www.acpwh.org.uk/) and furthermore have agreed that I can make it available from the www.electrotherapy.org download page – so get a copy, read it carefully and see what it has to say.

Contents in this Issue

Therapeutic Ultrasound

Just before we start the usual round of ultrasound papers, if you want to catch up with a great review of ultrasound use in its broadest context, you could do a while lot worse than to seek out a special edition of ***Progress in Biophysics and Molecular Biology*** from this year. Volume 93 Issues 1-3 give you a 420 page resume of research evidence and a summary of where we are in numerous different ultrasound therapy areas. Many of you will have particular interest in the paper by Gail ter Haar (Therapeutic applications of ultrasound – pages 111 – 129), but you will find a wealth of useful bits in the other papers, so if this is your field or you are working / researching / reviewing in this area, check out the index and get copies of these papers. I will try and review some in the next edition.

Low Intensity Ultrasound and Bone Tendon Junction Healing

This was a paper from Lu and associates from last year ***(Lu, H. et al. (2006). Low-Intensity Pulsed Ultrasound Accelerates Bone-Tendon Junction Healing: A Partial Patellectomy Model in Rabbits. Am J Sports Med 34(8): 1287-1296)*** and is well worth a read. The authors argue that the use of low intensity pulsed ultrasound (LIPUS) in bone repair is becoming well established (there is material on this in just about every issue of the newsletter) and so they decided to look at the influence of the therapy at the bone to tendon junction. I know that it is an animal rather than a straight clinical study, but the work is informative and may have useful and important clinical implications of it transfers to the clinical environment.

Essentially 48 rabbits were subjected to a partial patellectomy and divided into 2 groups (treatment and control). The ultrasound (LIPUS) was with the Exogen device delivering the now predictable 1.5MHz, 0.03 W cm2, 20 minutes daily starting on day 3 post op. The treatment was delivered through a plaster window over the patellar osteotomy site. Animals were sacrificed at various stages between 2 and 16 weeks post op and assessments were made using radiographic, biomechanical and histological testing. There were significant differences between the groups on all outcomes tested, all in favour of the LIPUS treated group (plenty of detail in the paper) and the authors argue that this has the potential for beneficial inclusion in the treatment of patients who undergo this type of surgery as a means to enhance their recovery.

Ultrasound for Shoulder Pain

An RCT, clinical paper relating to ultrasound . . . . I can feel the excitement growing . . . and it is 2007 no less . . . . read on! Ainsworth et al (***Ainsworth, R. et al. (2007). A prospective double blind placebo-controlled randomized trial of ultrasound in the physiotherapy treatment of shoulder pain. Rheumatology 46(5): 815-20***) have published the results of a multicentre (around Birmingham) placebo controlled RCT with 221 patients with unilateral shoulder pain. Patients were divided into two groups (real and sham ultrasound) and all patients were additionally managed with advice, home exercises and manual therapy.

The inclusion criteria were broad (unilateral shoulder region pain, exacerbated by active or passive movement) and all patients were 'new' referrals from the general practitioner. Patients received a maximum of 8 treatment sessions, with 28 different therapists involved in the delivery. Standardised exercise and advice sheets were used, but so far as I can see from the published report, the manual therapy employed by the therapists is not detailed. The shoulder disability questionnaire (SDQ-UK) was employed as the primary outcome measure and is purported to reflect quality of life issues as well as shoulder disability. Details are apparently in the Appendix to the paper, but I couldn't see them – might just be me?? There were in addition, a raft of secondary outcome measures including global shoulder problem score, pain, health related quality of life (EuroQoL) and range of movement. Measures were made a baseline, 2 weeks, 6 weeks

and 6 months. The power calculation was designed to ensure 80% power and 5% significance and recruitment of 220 patients should have provided this whilst allowing for drop outs.

The biggest problem that I had with this study was the ultrasound treatments delivered. I struggled to find the data to start with, but eventually found it in the intervention section which describes the range of treatments and therefore one assumes that there was not a specific dose or US treatment rationale. The average and range data goes like this : US was delivered in 88% of the treatment visits (very strange for an ultrasound trial) and the mean power was at 0.5 W cm2 (range 0.2 – 1.0) with a mean duration of 4.5 minutes (range 3 – 7) delivered in pulsed mode for 95% of the sessions, usually at 1:4 ratio at either 1 or 3 MHz (39% at 3MHz, 46% at 1 MHz and no information for 15%. This is all a bit of a disappointment in that it is very difficult to make a decent decision about the use of ultrasound when the application was SO varied, and in some cases, apparently, unrecorded. The fact that is was not used in 12% of the treatments is strange, and if one takes the ranges cited, there is a big difference between 1 and 3MHz at powers between 0.2 and 1.0 W cm2, some pulsed and some continuous etc etc. Much as I appreciate that it is difficult to come up with a uniform dose that would work for everybody with a mixed bag of shoulder problems, one would have thought that a treatment dose algorithm would have at least been a useful start point. Treatment sessions averaged at 6 per patient, but ranged from 1 – 8, and therefore, I think that this is a pretty heterogeneous intervention, and together with the unreported 'manual therapy' makes it somewhat difficult to tell what has and what has not gone on. Interestingly, we have more detailed information regarding the ethnicity of the patients than we do for the treatment that they were given.

In terms of the results for the stated outcomes, there were significant differences in the primary outcome for all 3 time points (compared with baseline) but not a significant difference between treatment groups (real vs sham). The secondary outcomes are reported, and again there was an improvement over the intervention period with a trend for greater improvement in the real US group, but this was not statistically significant.

The authors (understandably) come to the conclusion that the addition of US to the manual therapy, exercise and advice demonstrates no significant benefit to the final outcome and was deemed to be of no additional benefit. It is argued (in the discussion) that the pragmatic decision with regards dose was a potential weakness of the work (ummmmmmmm) but that without other evidence , it was deemed to be the most appropriate design. I can see their point, but remain unconvinced as in fact the wide range of US applications may simply serve to weaken the conclusions reached (this is not an attempted defence of a modality that can not be defended, but a genuine critical review comment). The fact that in almost 40% of the treatments, the US was delivered at 3MHz which is unlikely to reach a high proportion of the intended tissues to be treated is a tad of a mystery.

I am fairly sure that this will generate a degree of discussion (and probably hate mail from those working with this study – bit it was actually carried out 6-8 years ago, so they might have forgotten by now!!!). It is an interesting clinical study. Well powered, carefully controlled in almost every way – apart from the ultrasound which is the bit they were aiming to evaluate. Have a read through and see what you think. Comments welcomed as ever

Ultrasound for Delayed and Non Union

It is becoming almost predictable that each edition of ElectroNews will include something on the use of LIPUS (Low Intensity Pulsed Ultrasound) in relation to fracture healing, and so as not to disappoint, here is one for this issue! A nice recent paper by Jingushi et al ***(Jingushi, S. et al. (2007). Low-intensity pulsed ultrasound treatment for postoperative delayed union or nonunion of long bone fractures. J Orthop Sci 12(1): 35-41***) which is a reanalysis of previously collected data looking at the relationship between the use of LIPUS and time since the last operative intervention in a series of 72 cases.

It has been shown that LIPUS is clinically effective in the stimulation of fracture healing, and in this instance, it was cases of delayed and non union that were evaluated, rather than fresh fractures. All the fractures were of the long bones (22 upper extremity and 50 lower extremity) and related to a study previously published in 2003 from this group in Japan.

The authors detail the fracture sites, number of operations prior to the commencement of LIPUS, fixation type an patient profiles. The LIPUS delivery was what many of you will recognise by now as the 'standard' intervention at 1.5MHz, 0.03W cm2 in pulsed mode. The overall healing rate was determined at 75%, though interestingly, there was a differential between the upper and lower limb responses, with the lower limb being the more favourable.

The main point of this work was to analyse the relationship between the use of LIPUS, the outcome, and the time between the last operative procedure and the commencement of the LIPUS intervention. Essentially, there were signifi-

cant relationships between the union rate and the time from the most recent operation, the time from the fracture to the start of the treatment and the time between the start of the treatment and the appearance of radiological improvement.

The shorter the time between the last operative intervention and the start of the LIPUS, the better the outcome, with the union rate at 90% when the LIPUS was started within 6 months of the last operation. There are some additional issues identified by the authors in their analysis, but they key features of this work appears to support the use of LIPUS in these circumstances, and that it is of clinical importance to commence LIPUS within 6 months of the last operation in order to achieve the best union results.

I get asked a lot about this area of ultrasound use, and I will try and update the web pages in the near future to summarise the many results that have emerged in the last couple of years. This is an interesting review of previously obtained results, and supports the early intervention with LIPUS in delayed and non union patients.

LIPUS for Tibial Non Union

As a direct follow on from the previous study, this paper comes from a research group in Holland and looks specifically at the use of LIPUS in tibial non union (***Rutten, S. et al. (2007). Use of low-intensity pulsed ultrasound for posttraumatic nonunions of the tibia: a review of patients treated in the Netherlands. J Trauma 62(4): 902-8***).

The study looked at the outcomes for 71 patients treated with LIPUS having experienced a tibial fracture and subsequent non union. The authors used the existing literature to estimate the maximal % of these that might be expected to go through to union by chance (at 30% which is, I reckon a generous rate estimate) and then compared the union rate with this LIPUS group which came out at 73% (incredibly close to the level attained in the previous study). The difference between the maximal expected spontaneous rate and the LIPUS achieved rate was highly significant, and it makes for an interesting paper for anybody with a continued interest in this field. There are, in addition to the main analysis, some interesting sub group analyses and discussion.

Ultrasound for Chronic Lateral Epicondylitis

This is a study that has evaluated the effect of LIPUS type ultrasound (as per the fracture healing studies above) but this time in relation to a musculoskeletal (soft tissue problem). This is an urgently needed area of investigation, and in fact I put it up as a funded PhD last year – though not taken up. If the LIPUS is as effective as has been demonstrated for bone lesions, then the key question is whether it is as effective for soft tissue work. There are several claims made in this regard (by the manufacturers) but as yet, not exactly a lot of clinical evidence to help the therapist to make a decision one way or the other.

This was a paper from last year by D'Vaz et al (***D'Vaz, A. P. et al. (2006). Pulsed low-intensity ultrasound therapy for chronic lateral epicondylitis: a randomized controlled trial. Rheumatology 45(5): 566-70***) in which a group of patients with 'chronic' lateral epicondylitis (more than 6 weeks duration) self treated with real or sham LIPUS (same settings as the fracture healing studies – again) daily for 20 minutes over a 3 month period (and the machine included a compliance monitor). The primary outcome was a 50% improvement from baseline in pain measured at 12 weeks (using VAS) and several secondary outcomes including a Forearm Evaluation Questionnaire and grip strength.

The active and placebo groups were pretty well matched (n = 29 and 30 respectively) and 48 patients completed the trial. The active group mean pain reduction was80% compared with 60% in the sham (placebo) group, and although there appeared to be a better effect in the real treatment group, this difference was not statistically significant. There were also improvements in the secondary outcomes, but again, even though the trend was in favour of the treatment group, they results were not significantly different between the groups. Compliance was deemed to be high with an overall compliance rate at just under 90%.

The authors conclude that LIPUS (as used in this study) was not significantly effective over a sham treatment for chronic lateral epicondylitis. It may well be that although this very low energy level is effective I terms of bone healing, it may be ineffective with some, or maybe all, soft tissue injuries and lesions. Clearly, a substantial amount of other similar work needs to be carried out, but this is a useful start and provides some insightful data.

Ultrasound and Blood Flow in Humans

There have been, over the years, a slow but steady trickle of research papers evaluating the effects of various modalities on changes in local blood flow. It is pretty widely claimed that many interventions include 'increased local blood flow' as one of their effects, and this paper (***Noble, J. G. et al. (2007). Therapeutic ultrasound: the effects upon cutaneous blood flow in humans. Ultrasound Med Biol 33(2): 279-85***) adds to the evidence stock. The research group, based in Nottingham, UK used laser Doppler flowmetry to measure blood flow changes in the skin in 10 healthy volunteers as a result of both pulsed and continuous ultrasound therapy.

Each subject was exposed to 4 different experimental conditions (control, placebo, pulsed and continuous Ultrasound) in a random order. All treatments were applied at 3MHz and at 1 W cm2 for 6 minutes. In addition to cutaneous blood flow, the ambient and skin temperatures were recorded.

Both the skin blood flow and the skin temperature were recorded from the anterior aspect of the forearm proximal to the wrist, but distal to the ultrasound application area (though interestingly, the abstract states that the intervention was to the lateral forearm – looking at the pictures and the description in the text – it was certainly anterior). The sampling method was detailed for the outcomes, and effectively, the blood flow and skin temp were sampled at 2 minute intervals following a baseline stabilisation period, and the blood flow (measured in BPU's (blood perfusion units) were subtracted from the baseline to give a relative change in perfusion rather than an absolute value. The laser Doppler device used effectively measures the flow at 1mm depth.

In the control condition, there was literally no intervention. In the placebo session, the treatment head was applied and moved, but with zero output. The pulsed mode had the machine set at a pulse ratio of 1:2 (33% duty cycle). The 'large' (5cm2) treatment head was used in all sessions, and a specified area of skin was treated, being an 8 x 5cm rectangular area, using a continuous movement pattern and standard gel coupling medium.

The results for the blood flow showed a significant difference (increase) between control and both pulsed and continuous US at 2, 4 and 6 minutes. The pulsed US had a stronger effect throughout (from Figure 2) and all blood flow measures pretty much came back to baseline by 2 minutes following the cessation of the treatment. The skin temperature changes in the control condition demonstrate a slow rise throughout the session, but in the treatment sessions, the rise was greater during the 6 minutes of treatment, with a return to baseline normally within a few minutes of treatment stopping. Continuous US was the exception in that the skin temp remained slightly elevated through to the end of the recording session. The statistical analysis indicated a significant effect with time, but no significant differences between interventions. The ambient (room) temperature did change over the duration of the sessions, but there were no significant differences between groups.

These results do demonstrate a significant increase in cutaneous blood flow during the application of US which were significantly different from the control condition, but interestingly, not significantly different from the placebo, though looking at the graphical results, the pulsed US had the greatest effect, with the continuous and placebo being much harder to separate. There is little doubt therefore that the 'rubbing' has a demonstrable effect, but that the US energy application does appear to have an effect over and above this – though the statistical analysis does not show significant results (though with a sample of n=10 that is not exactly surprising).

This combination of blood flow and thermal data does indicate that there is a change in blood flow as a result of the application of US, that part of this appears to be related to the movement of the treatment head, but looking at the differentials between the blood flow and thermal results, the application of pulsed US appears to have a blood flow effect that is not related to thermal change, and therefore contributes to the evidence for what is commonly referred to as the 'non thermal' effect.

There are two points that the authors do not appear to have raised that I will throw into the pot for good measure. Firstly, the amount of energy delivered in the continuous and pulsed applications was clearly not the same. The treatment time and intensity were standard throughout, and thus, when using the pulsed mode (at 1:2), there would have been significantly less energy going into the tissues (the machine was effectively only 'on' and therefore 'working' for 1/3 of the time) and therefore the pulsed US results were attained as a result of applying 1/3 of the energy of the continuous treatment. Not critical, but given that the pulsed US results appeared to provide the greater blood flow changes with

Seen any interesting papers?
Is there a paper that you have written and ought to be reviewed here?
E mail and let me know electronews@electrotherapyonline.co.uk

less energy, it is certainly interesting and (in my view anyway) ads to the growing body of evidence for the low energy (or non thermal) evidence for this modality. Secondly, the laser Doppler measurement system clearly only records blood flow changes from the very superficial tissues (effectively, the skin as the paper title suggests). This may (or may not) reflect the changes taking place in the deeper tissues which is the bit that is probably most interesting and that most of us struggle to measure, even though it is clinically the more fascinating element.

Nice paper, well presented and argued (though I would have liked to see all the stats results) and well worth a read.

LIPUS and Cell Level Effects

OK, so we are back to the bone repair theme here, and once again, it is Low Intensity Pulsed Ultrasound which is the key intervention. This paper from a Tokyo research group (***Takayama, T. et al. (2007). Low-intensity pulsed ultrasound stimulates osteogenic differentiation in ROS 17/2.8 cells. Life Sci 80(10): 965-71***) is a lab based study that evaluates the effect of LIPUS on osteogenic cells. I'll not go into a lot of detail on this one in that it is primarily concerned with periodontal disease (dentistry to the rest of us) but will include a summary as it appears to add weight to the knowledge base with regards the mechanism of action relating to the use of LIPUS as an intervention.

Osteoblastic cells (rat) were used to represent osteoblasts from the alveolar bone of the periodontium, and they were exposed (in culture) to the standard LIPUS (same as reported already in this issue). Various key outcome measures were assayed including ALPase, RNA, assessment of mineralised nodule formation, calcium content and some additional measures (like SDS-PAGE).

The results indicate that the use if LIPUS did not affect the rate of cell proliferation, whilst it did significantly increase the levels of ALPase activity, various RNA expressions that relate to osteoblastic activity, greater mineralised nodule formation which had higher calcium content in the LIPUS treated cultures.. At the end of the day, the LIPUS treated cultures demonstrated enhanced activity of the osteogenic cells, and for those who have a desire to explore the cell and subcellular mechanisms involved, this paper does provide some additional data. Getting to grips with the details of the various outcomes will be beyond many of us, mainly in that this is somewhat unfamiliar territory.

LIPUS and Cartilage / Interleukin Interaction

Another paper from this year (***Park, K. et al. (2007). Therapeutic ultrasound effects on interleukin-1beta stimulated cartilage construct in vitro. Ultrasound Med Biol 33(2): 286-95***) also takes a look at some specific effects of LIPUS, though this time in relation to degenerative (OA) cartilage and Interleukin activity. As with the Takayama et al paper previously, the details of this one are beyond that which most of us will be familiar with (after all this is not really supposed to be a cell biology newsletter), but in essence, it was argued that the mechanical effects of the modality could have an effect on chondrocytes in cartilage and therefore potentially offer a 'chondroprotective' function. The use of the interleukin in the cultures was to produce an OA type response to the chondrocytes in culture. The outcomes were related to cell number, cell viability, GAG content and various markers including TGF-Beta.

At the end of the day, the LIPUS did not make a significant difference to either cell numbers or cell viability but it did have a positive effect on GAG values (check out the web site – Tissue repair pages) if you are not sure of the role of GAG's, but they are important complex molecules that are partly responsible for the water binding properties of (in this case) cartilage, and this play an important role in maintaining the hydration of the tissue. This is a gross oversimplification of what they do, but it does convey the essentials. Less GAG's, less hydration, more prone to degeneration etc etc. The LIPUS served to bring about upregulation of the synthesis functions of the chondrocytes which in terms of OA pathology could be a useful contributor too cartilage protection. This is some way off clinical application, but certainly holds potential, and given the number of treatments that have been attempted to prevent / limit / stop cartilage degeneration, and have almost consistently failed to do so, it is showing promising signs. Just like the previous paper, I will not go into graphic details of the cellular assays, but it is all there in the original should you wish to go a hunting.

Low Frequency Ultrasound and Wound Debridement

Last of the Ultrasound papers for this issue looks another 'different' aspect of therapeutic ultrasound. Kavros (***Kavros, S. J. et al. (2007). Treatment of ischemic wounds with noncontact, low-frequency ultrasound: the Mayo clinic experience, 2004-2006. Adv Skin Wound Care 20(4): 221-6***) describes the use of ultrasound energy application to a

wound using a non contact technique – effectively transmitting the energy through a saline mist (hence the phrase MIST Therapy used in the paper). This low frequency US (at 40kHz) is actually similar to what will be recognised by therapists as 'longwave' ultrasound, though the machine being used here is different from the commonly encountered therapy device.

This was an RCT type design with two groups – one getting standardised wound care and one getting the standardised care plus the ultrasound (n=35 in each group). All patients presented with chronic wounds (more that 8 weeks) and 'critical' limb ischaemia. Treatment was for 12 weeks with the therapy group receiving the US 3 times a week. The standardised are (both groups) is detailed in the paper, as is the specific US device being used and treatment parameters.

The main outcome measure was the % of patients with more than 50% reduction in wound size from baseline by the end of the 12 week period. The results indicate that in the treatment group, there was a higher number of patients who met the 50% healing criteria than in the control group (63% vs 29%) which was statistically significant. This therapy has previously been shown to be effective in other patient groups (e.g. diabetic ulcers), and the demonstration that this therapy is clinically effective with a patient group with critical ischaemia (where the treatment options are getting limited) is a useful addition to the armoury of management protocols in such difficult to manage wounds.

Shock Wave Therapy for Chronic Patellar Tendinopathy

Shockwave therapy (in its various forms) makes an appearance in almost every newsletter, and I have a couple of papers for you in this edition. The first, from a research group in Taiwan follows a steady flow of clinical papers which evaluate the use of this therapy in a chronic tendon problem (***Wang, C. J. et al. (2007). Extracorporeal shockwave for chronic patellar tendinopathy. Am J Sports Med 35(6): 972-8***).

This RCT had 2 groups in a treatment (27 patients with 30 knee problems) and a control group (23 patients with 24 knee problems). The treatment group received a single treatment session of the therapy and the follow up period was certainly a long term on with measures taken at 1, 3, 6 and 12 months and then once a year thereafter (though some of the follow up assessments were by phone interview). Outcome measures included pain, a knee sports assessment score (VISA) and ultrasound scanning.

The control group were treated conservatively with NSAID's, physiotherapy, exercise and a knee strap (more details in the paper). The results show a significant improvement in the pain and VISA scores over time for the treatment group but not for the control group. The scores between the groups were not statistically significant at baseline, but were significantly different after treatment. Interestingly, the recurrence rate was 13% in the treatment group and 50% in the controls. The ultrasound scanning data showed changes in patellar tendon thickness and changes in tendon vascularity (which were significant between the two groups after treatment). There are extensive results tables in the paper which provide a lot more detail than I have alluded to here and are worth working through if this is your area (shockwave or chronic tendinopathy). Nice paper, easy read and makes a very useful contribution to the shockwave evidence pot.

Shockwave Therapy for Stress Fractures

The second shockwave paper is a case series (5) report from Japan (***Taki, M. et al. (2007). Extracorporeal shock wave therapy for resistant stress fracture in athletes: a report of 5 cases. Am J Sports Med 35(7): 1188-92***) from the same journal as the previous paper. I am aware of the discussions over hierarchy of evidence quality, and how people almost automatically write off case studies, but there is no doubt (personal opinion) that they make a useful contribution to the evidence base, and there are times when they provide better quality information than some rather dubious RCT's which are often 'automatically' regarded as being a higher level of evidence (though as we have already seen in this issue, that might not actually be the case).

Anyway, this report details 5 cases of athletes with stress fractures (2 x tibial, 1 x 5th metatarsal, 1 x pubic ramus and 1 x medial malleolus). The average time between stress fracture and the shockwave treatment was 12 months. The paper describes the clinical case in each instance, the treatment applied and the clinical outcomes using the assessment data that was available for each patient. The radiographic evidence in each patient is presented and the average time to union was just under 3 months and return to sport averaged 4 months.

There is an interesting and fundamental difference between the LIPUS approach to fracture healing and that of the shockwave approach – though both appear to be gaining a string evidence base. The shockwave therapy usually involves a single treatment session, though this can be regarded as a 'high intensity' intervention, and in the cases reported here, the patients were all treated under anaesthesia. The LIPUS approach involves a much lower energy application, but it needs to be applied daily for a significant period (usually 20 minutes). Going back to my previously documented 'hammer' and 'feather' approaches (Watson, 2006, 2007), it is unsurprising that both approaches are effective – it is essentially just a difference in mode of application – big blast of energy or a small dribble of energy. My own (personal) preference is for the slow dribble, but there is certainly evidence on both sides of the equation, and this particular paper adds weight to the one off, hit them hard type approach.

OK, so time to move away from various forms of ultrasound, and get into the electrical stimulation modalities. There are relatively few in this issue (coincidence, nothing else).

TENS for Neuropathic Pain

This is a paper from last year that I have only just got round to including, involving an animal (rat) model aiming to use TENS to reduce the development of allodynia. The use of TENS in Complex Regional Pain Syndrome is something that I get asked about a lot, and it is difficult to come up with anything strongly conclusive from the evidence base, and certainly anecdotally, I have seen some exceptionally good result for patients with this problem, yet sometimes it appears to seriously aggravate the condition. If we were better able to work out the patient groups for whom this treatment would be beneficial and the patients for whom it is likely to be aggravating, this it would enable a more discriminative clinical decision making process. Hopefully, this paper (***Somers, D. L. and F. R. Clemente (2006). Transcutaneous Electrical Nerve Stimulation for the Management of Neuropathic Pain: The Effects of Frequency and Electrode Position on Prevention of Allodynia in a Rat Model of Complex Regional Pain Syndrome Type II. Phys Ther 86(5): 698-709***) will make a contribution to this enhanced clinical choice.

The paper includes a useful review and rationale for the use of TENS in this type of problem and makes for a useful read in its own right (even if you are not interested in rat allodynia!). The basic investigation was designed to evaluate the relative efficacy of using TENS at both high and low frequency with electrodes placed ipsilaterally or contralaterally. The rat experimental model used here is well established and the onset and natural history of the symptoms are known, making the effect of the intervention easier to evaluate. The basic procedure involves applying sutures to the (R) sciatic nerve. The main outcomes involved assessment of the mechanical pain threshold (using monofilaments) and the thermal pain threshold (using a radiant heat source).

TENS was applied with surface electrodes at either 100Hz or 2Hz. The 100Hz stim was applied over the ipsilateral or contralateral nerve roots (paravertebral application) where as the 2Hz stim was applied to the knee and leg (right or left) with specific electrode positions being related to acupuncture points (fully described in the paper) and following previously published methods.

157 rats were used for this work divided into several groups to enable various combinations to be evaluated. I will try and summarise the grouping as simply as I can

- High frequency, paravertebral, ipsilateral (i.e. (R) side) [n=23]
- High frequency, paravertebral, contralateral (i.e. (L) side) [n=15]
- Low frequency, ipsilateral knee acupuncture points (i.e. (R) side) [n=23]
- Low frequency, contralateral knee acupuncture points (i.e. (L) side) [n=12]
- Control group (surgery but no stimulation) [n=28]

A further 3 groups of animals were not subjected to the nerve injury and acted as further control groups:

- no surgery and sham high frequency TENS) [n=20]
- no surgery and real high frequency TENS [n=24]
- no surgery and real low frequency TENS [n=12]

There were a few rats in other minimal groups, but if I carry on breaking down all the groups used, I might as well reproduce the whole paper!

Treatments were 60 minutes daily for the 11 days following nerve surgery. Mechanical and thermal thresholds were evaluated at various set points post nerve surgery. It is known that the mechanical allodynia reaches a maxima at 12 days post op in this rat model.

As you can imagine, the data analysis was long, but having taken you through the experimental set up, it would be daft not to tell you the outcome! The operated rats demonstrated a reduction of mechanical pain threshold (41%) and a reduction in thermal pain threshold (25%). With regards the mechanical threshold, the TENS applied at high frequency (100Hz) to the contralateral side of the spine got the most significant mechanical threshold effect. With the thermal threshold results, the most effective TENS was the low frequency (2Hz) delivered to the contralateral limb.

The TENS treatments applied to the rats who did not have the nerve injury had no significant effect on either threshold when applied at high frequency, but did change the thermal threshold at low frequency.

The discussion in this paper is extensive and well argued, and given that it is over three pages in the original, it would be daft to try and reproduce it here – go get the original – but goes like this : both low and high frequency TENS affects thresholds in a rat model of allodynia when applied to the contralateral side The thermal thresholds were only effectively managed with contralateral low frequency TENS and the mechanical threshold was only effectively changed with high frequency TENS. The high frequency was applied at spinal nerve (paravertebral) level and the low frequency was applied to the limb. The combination not tested that would further elucidate clinical application (high frequency to the limb and low frequency paravertebral) leaves that aspect unanswered which is a shame, but the various combinations were complex enough as it was, and I guess that further evaluation will be needed to answer the last bit of the question.

The authors argue that these finding could reasonably be expected to transfer to humans in the clinical environment, and I leave it for you to consider their rationale and discussion on that point. This is a comprehensive animal study which certainly brings up some interesting findings. The fact that the contralateral results were better than the ipsilateral ones is worthy of note and may provide a useful clinical pointer. It would be great to have the evaluation of the missing combinations (low frequency paravertebral and high frequency peripheral), but I guess we will just have to wait for that!

TENS for Hip Pain during Emergency Transport

This is the second paper along these lines from the same research group based in Eastern Europe (***Lang, T. et al. (2007). TENS relieves acute posttraumatic hip pain during emergency transport. J Trauma 62(1): 184-8***). The previous paper (2005) related to the use of TENS for pain relief in patients with back pain. The argument was that pain relief during emergency transport is an important aspect of case and TENS offers a potentially simply mechanism which might be clinically beneficial.

30 patients were treated with real TENS and 33 with sham TENS, and the key outcome measure was pain using a VAS measure. All patients included in the study had pain from a hip fracture and there were well described assessment and screening systems in place and no additional analgesia was employed for those in the study.

TENS was applied with a local electrode arrangement at 100Hz, 200 microsec pulse duration for 30 minutes at a set 2mA intensity. The results were that there was a significant difference in pain relief between the two groups, with an almost unchanged pain level in the sham group and a useful reduction in pain in the treatment group (mean reduction from 89 to 59 VAS in Rx group and 86 to 79 VAS in sham group). Anxiety and heart rate and blood pressure changes were also monitored (as secondary outcomes) and there was a significant reduction in anxiety and heart rate in the treatment group. There were no significant changes in blood pressure for either group.

Whilst it is appreciated that TENS during emergency transport is unlikely to be the clinical forte for most of you reading this newsletter, there are a couple of interesting points. Firstly, that TENS is effective as a modality to help manage acute pain – it is not only useful in chronic pain patients. This is an area I have been lecturing on for several years, and this paper certainly adds to the evidence that acute pain can be influenced by TENS. Secondly, this has possible application as a routine pain management intervention for patients in pain during emergency transport, and we are in the process of evaluating this in the UK with a clinical trial.

TENS Meta-analysis for Chronic Musculoskeletal Pain

Having already provided a long review of the TENS and allodynia paper, I could provide another long review of this one which is a well constructed meta-analysis which came out earlier this year (***Johnson, M. and M. Martinson (2007). Ef-***

ficacy of electrical nerve stimulation for chronic musculoskeletal pain: a meta-analysis of randomized controlled trials. Pain 130(1-2): 157-65).

This makes for an interesting read in that the authors used some very specific statistical techniques that allow one to combine the data and results from several different studies, thus giving a larger total sample and therefore the possibility of identifying effects that might otherwise be deemed insignificant on the basis of marginal effects in a small sample. Clinical RCT type studies were collected over a 10 year period and the results from the higher quality studies combined (you can read the details in the paper) such that a total of38 studies in 29 papers were combined giving 474 patients receiving stim, 335 receiving placebo and 418 receiving both stim and placebo in a crossover design. When these combined data sets were analysed, the results showed that there was a significant reduction in pain with the stimulation therapy. This was a heterogeneous sample in that the selected papers included mixed methodology and certainly included mixed treatments (some TENS, some PENS, some at low and some at high frequency). There is a useful consideration of these variables in the review, and given that Pain is a relatively easy journal for most people to access, I would very strongly encourage access of the full analysis. It is not a difficult read and well worth the effort.

The reason for mentioning it here is (a) to raise awareness in case you have not already seen this paper and (b) it raises an interesting issue with regards sample size and the benefits of combined data analysis.

TENS and Waveform Characteristics

This is an interesting paper that considers whether the various different TENS waveforms used by different manufacturers actually makes a significant difference to the outcome of the treatment. It has been argued (by myself and numerous others) that the shape of the wave is not an essential feature of the stimulation, and even if it does make a difference, there are other parameters that are more critically important.

This paper (***Hingne, P. M. and K. A. Sluka (2007). Differences in waveform characteristics have no effect on the anti-hyperalgesia produced by transcutaneous electrical nerve stimulation (TENS) in rats with joint inflammation. J Pain 8(3): 251-5***) evaluated the difference in effect of two different TENS waveforms (asymmetric biphasic and symmetric biphasic) on inflammatory hyperalgesia using a animal (rat) pain model. An inflammatory knee arthritis was induced using a well established method and the primary outcome measure was a paw withdrawal time in response to a heat stimulus. TENS was applied at 100Hz with a 100 microsec pulse duration with one of 2 different machines delivering these different waveforms. Treatment was for 20 minutes at a sensory intensity. The results showed that TENS with both waveforms significantly reduced secondary hyperalgesia but that there was not significant difference between the different applications.

There are some further issues raised, and some interesting ipsilateral vs contralateral comparisons (which link with the allodynia paper reviewed earlier). The key (take home) message from this work is that it does not appear to make a difference whether one employs a symmetric or asymmetric biphasic waveform, though there are arguments with regards patient comfort and acceptability of the stimulation with various waveforms. If one type of stim if preferable from the patients point of view, that is fine and can be taken into account when operating in the clinical environment, but it does not appear to alter the basic efficacy of the intervention.

TENS and Blood Flow in Human Subjects

Having had a look at a paper earlier considering the effect of ultrasound on human blood flow, this paper looks at a similar thing, but this time in response to TENS. Researchers from several UK universities, some of whose names I suspect will be well known to many of you have published a study looking at the effect of TENS on blood flow and skin temperature after that local blood flow has been increased with a heat application (***Chen, C. et al. (2007). The effect of transcutaneous electrical nerve stimulation on local and distal cutaneous blood flow following a prolonged heat stimulus in healthy subjects. Clin Physiol Funct Imaging 27(3): 154-61***).

A sizeable group of asymptomatic subjects (n=66) were divided into three groups – one control, one receiving low frequency TENS (4Hz) and one at high frequency TENS (110Hz). The TENS was applied for 15 minutes to the anterior forearm (over the median nerve) and the blood flow and skin temperature was recorded from the forearm and the finger tips. Previous studies from this and other groups have not demonstrated significant blood flow changes as a result of TENS, but it was argued that if there was an increased local flow (say, as a result of an inflammatory response) it might be that TENS has a previously undetected effect. The blood flow changes in this experimental set up were achieved

through a local heat application (15 minutes, infra red t flexor aspect of forearm, before electrode application) rather than through an inflammatory response.

The TENS was applied at just below the motor threshold (described in detail in the paper) and recordings of skin temperature and blood flow were made during the 15 minutes of TENS plus 15 minutes post stimulation. Skin temperature was recorded with skin thermistors and the blood flow with laser Doppler flowmetry providing a relative flow index in BPU's (as per the ultrasound paper previously reported).

The results show that there was a significant change in forearm blood flow with time but that there was no significant difference between groups (control, low and high TENS). The decrease in flow with time is to be expected as the limb had been preheated, and thus the startpoint was from an elevated local flow. The fingertip bloodflow data showed some different results in that the control group showed a steady decline in flow whilst the TENS groups demonstrated an increase in flow with time, most especially after the cessation of the TENS application. The high frequency TENS gave the bigger differences which were statistically significant for some time points post intervention.

The skin temperature changes showed a steady decline in forearm and index finger temperature in all groups and there were no significant differences between groups at any time point. The temperature of the middle fingertip however appeared to behave differently, with a significant decrease in skin temperature during the TENS application for both the low and the high frequency treatments.

There is an interesting discussion with regards these results, but the authors conclude the any changes that occur in blood flow and skin temperature are small and transient (i.e. short lived) and thus are unlikely to be of clinical significance. These results are consistent with the findings of a literature review we recently conducted looking for evidence of significant blood flow changes as a result of electrical stimulation. We were able to identify evidence of flow changes but only when the stimulation was above motor stimulation thresholds, and therefore we concluded that the involvement of muscle activity was an essential feature of the stimulation if an increased local blood flow was an intended outcome. The only exceptions that we were able to identify were those involving electroacupuncture in various forms, which is interesting, but I think it is a separate issue to the TENS described here.

Whether this particular research can be directly related to the clinical environment is unknown in that the increases in blood flow achieved prior to the TENS were always going to be transient (as a result of the superficial heating) and although they reflect an attempt to reflect altered blood flow as a result of pathology, were clearly not the same as the sustained flow changes as a result of local injury or sympathetic disturbance. Never the less, this is a well constructed trial and the results certainly add to our knowledge in this fascinating field.

Interferential for Constipation and Soiling in Children

Although I expect that most of you consult this journal on a regular basis (??), I have included this paper on the basis that you might have missed it, and for those working in the field of paediatric continence, it could be a very useful one. Chase et al (***Chase, J. et al. (2005). Pilot study using transcutaneous electrical stimulation (interferential current) to treat chronic treatment-resistant constipation and soiling in children. J Gastroenterol Hepatol 20(7): 1054-61***) published this paper a couple of years ago, and I am sorry for the delay in getting it to you – but I guess I missed it as well!

Whilst not a clinical problem that a large number of us encounter on a regular basis, it is clearly a significant problem for the children, their carers and the practitioners looking after them. Essentially this paper reports the outcomes of a small sample (n=8) of children who presented with chronic, treatment resistant constipation and soiling treated with interferential therapy in an attempt to improve their clinical status.

The children ranged in age from 7 to 16 years and all had at least a 4 year history of chronic constipation, soiling and limited or no response to traditional therapies. Interferential was employed with paravertebral and anterior abdominal electrode placements three times a week for 3-4 weeks. Outcomes were primarily related to a diary kept by the children and their carers. The interferential was applied (as mentioned above) with 2 paravertebral electrodes placed at T9 – L2 level and anterior electrodes placed left and right under the costal margins. A 4kHz carrier wave was set to generate a stimulating frequency on an automatic sweep of 80-120Hz (the beat frequency – though I am not especially keen on that particular term), and an intensity at the sensory but sub-motor level and a treatment duration of 20-30 minutes per session.

Unless you are directly involved in this patient group, some of the details of the different type of clinical problem described in the paper will not be especially meaningful, but for anybody who is interested, I suspect that you will fully un-

derstand the differentials when you get to the original. The results essentially showed that the stimulation stopped soiling in 7 out of the 8 children and increased the frequency of spontaneous defecations in 5 out of the 8. It would appear therefore that this type of interferential treatment could be of clinical usefulness in this specific group of treatment resistant chronic constipation with soiling, and although the authors acknowledge that further, larger scale studies are needed, it is a useful indicator of potential clinical benefit.

Electroacupuncture for Labour Pain

I know that electroacupuncture is not a mainstream topic for this newsletter, but there are no lack of papers in this area, and in fact I have just got hold of a copy of a great new electroacupuncture text by David Mayor which I will review in the next issue. In the meantime, here is an interesting 2007 paper by a research group in China (***Qu, F. and J. Zhou (2007). Electro-acupuncture in relieving labor pain. Evid Based Complement Alternat Med 4(1): 125-30***) which compared labour pain relief in a 2 group (treatment/control) design involving 36 women, comparing their beta endorphin and serotonin (5-HT) blood concentrations in addition to their pain intensity.

The women were all 'normal' deliveries and of the 39 recruited, 20 were in the electroacupuncture group (electroacupuncture and no other pain relief intervention) and 19 in the control group (assigned to receive no pain relief intervention). The final analysis included 18 from each group and the reasons for the drop outs are detailed in the paper.

The electroacupuncture points were Hegu (LI-4) and Sanyinjiao (SP-6) bilaterally. Treatment started at the first part of labour for 20 minutes and then repeated again at 7-8cm dilation. The control group (apparently) received no pain relief intervention. Pain was recorded on a VAS type scale hourly and blood samples were taken at the start of labour, at the end of stage 1 and were tested for endorphin and 5-HT levels.

The two groups were determined to be equivalent after randomisation. The treatment group had a significantly lower pain level and a greater degree of relaxation. The beta endorphin levels were equivalent at the initial measurement, but were significantly different at the later measurement (in favour of the treatment group) and the same findings were identified for the 5-HT levels.

Given that both the beta endorphin and the 5-HT have been shown in numerous studies to have a distinct influence on pain perception (usefully reviewed in the paper), the findings are consistent with the patient reported pain perception and relaxation ratings and provide a useful addition to the literature in this field.

Functional Electrical Stimulation in Children with Cerebral Palsy

The last of the electrical stimulation papers is a recent (2006) report of some experimentation relating to electrical stimulation to the posterior crural muscles in an attempt to improve gait in children with CP. Ho et al (***Ho, C. L. et al. (2006). Functional electrical stimulation changes dynamic resources in children with spastic cerebral palsy. Phys Ther 86(7): 987-1000***) are based at various institutions in the States employed a cross over design with 9 children (aged between 3 and 11 years and also 6 children of the same age range who were developing normally. The crossover design meant that the 9 children with CP were exposed to 15 walking trials of FES followed by 15 without FES. The normally developing children were assessed without FES. Various gait parameters, which were normalised for the comparison were recorded. This is a long and detailed paper, but at the end of the day, there were changes that were demonstrated in the gait parameters of the children with CP when measured walking with the FES active, but the changes that were achieved did not actually result in a more typical spatiotemporal gait and although there was a significant increase in some aspects (speed normalised dimensionless impulse), there was no change in stiffness, stride length and stride frequency.

The FES was delivered to the gastrocnemius muscle either in a bilateral (reciprocal) or unilateral mode, depending on the specific presentation of each child. The stimulation was controlled by pressure sensitive foot switches through a computer/software controlled device. Kinematic data was collected via an OptoTrak system and specific analysis software.

This is a detailed (14 page) paper with plenty of detail for those that need detailed information regarding the gait parameters, clinical presentations and gait changes that were recorded. It is beyond the scope (or remit) of this newsletter to go into that level of detail, especially as I have already dissected a couple of papers at considerable length already. I think that the important point here is that although significant changes were demonstrated, they, by the authors own ac-

knowledgement, did not make for a more typical gait pattern, though this intervention should not be written off at this stage, as there are a range of possible treatment intervention options that may prove to be more fruitful in future work.

Heading towards the last few papers now. Firstly a couple that relate to thermal interventions:

CryoCuff and tendoachilles microcirculation

Knobloch et al based in Germany published a couple of papers in 2006, and this particular one relates to the use of CryoCuff and TA microcirculation (***Knobloch, K. et al. (2006). Changes of Achilles Midportion Tendon Microcirculation After Repetitive Simultaneous Cryotherapy and Compression Using a Cryo/Cuff. Am J Sports Med 34(12): 1953-1959***).

This was a lab based study using healthy volunteers (26) in an attempt to evaluate the effect of intermittent cold and compression on the microcirculation of the midportion of the Achilles Tendon using a laser Doppler system (met that twice before in this issue alone). The detailed baseline data, including sport related activity are provided in the appropriate section of the main paper. The laser Doppler device used measures blood flow at 2 different depths (2mm and 8mm) and is reported in Arbitrary Units (AU's) for reasons detailed and explained. This is a complex measure, but I would suggest that if one was to view the AU as a relative index of local flow, it would be a reasonable assumption.

The CryoCuff device was applied using an ankle/boot system following the written recommendations of the manufacturers. The blood flow probe was fitted 4cm proximal to the TA insertion over the midpoint of the tendon prior to the application of the boot (I do worry to some extend that this simply can not have actually been the midpoint of the TA for everybody as it will depend on limb length, but some people would argue that any differences that will occur are minimal. I personally prefer a proportional measurement as it takes into account individual differences). The experimentation took 1 hr and the subject was lying supine throughout, and data were recorded every 20 seconds throughout. The sequence consisted of 10 mins application followed by 10 minutes rest (no cryocuff) which was repeated for a further 10 plus 10 minutes, and then again, thus giving 3 x 10 min applications of the cold and compression interspersed with 3 x 10 minutes rest periods.

The results :

The subjects impression of the discomfort associated with the cuff went up to around 7.something on the VAS rating, with there being a slight decrease with the repeated applications. The blood flow results (oxygen saturation) showed a significant decrease during each application period at the 2mm depth whilst at the 8mm depth, there was a significant decrease during the first application, and no further significant changes after that. There was a significant increase in saturation during each recovery phase at both depths.

Further microcirculation results are reported for the post capillary venous filling pressures with significant decreased pressures during the application periods with a reactive recovery during the recovery phases. The reactive increase in pressures during recovery were less marked at the deeper portions of the tendon.

The blood flow results showed a strong and significant decrease on the first and subsequent applications in both the superficial and deep layers, with relative flow changes decreasing by almost 95% at times. There appeared to be no reactive hyperaemia in the recovery phases.

There are a lot more detailed results in the paper and well worth a look but essentially, this work does demonstrate significant flow and oxygen saturation changes in the TA midportion with repeated cold (CryoCuff) applications. The onset of these changes was very swift (normally in a matter of seconds) but there were no real sustained effects following removal of the cuff. The authors discuss several reasons why these changes might be of benefit in the clinical environment both post injury and in cases of chronic tendinopathy. These seem very logical and I have no problem with them or their proposed benefit, but it must be remembered that these are proposed benefits rather than effects that were demonstrated in this particular study which involved asymptomatic subjects. Nice paper none the less and without doubt, adds to our understanding of TA microcirculation response to cold therapy.

Radio Frequency Fields and Metal Implants

Down to the last couple of papers now or else it is going to be time for the next issue to be coming out! There has been a long standing debate with regards the effects of various forms of electrotherapy on metallic implants, whether this ac-

tually constitutes a real contraindication or whether it is a historical 'pass it on' kind of perceived danger. We have looked at this in relation to the recently published CSP Electrotherapy Guidance (2006) and this paper might make a useful addition to the evidence for the debate. Virtanen et al published this work last year (***Virtanen, H. et al. (2006). Interaction of radio frequency electromagnetic fields and passive metallic implants--a brief review. Bioelectromagnetics 27(6): 431-9***) and although they indicate that it is a brief review, it is in fact pretty comprehensive and a great start point for anybody wanting pursue any of these issues further.

The paper provides a succinct but well argued background and goes on to review numerous papers that have evaluated these heating effects. The majority of these papers are computer simulation based as 'real' measurements are incredibly difficult to take. Rather than try and reproduce their review (which the copyright people would get a bit upset about if nothing else!), I will try and summarise their key findings : The material and shape of the implant make a difference to the temperature changes that will result (interestingly, gold implants are great in that they are efficient at dissipating the thermal energy compared with something like titanium – I know that gold hip replacements are an unreal proposition, but it makes for an interesting hypothetical argument . . . . .). The size of the implant will have an effect, but this relates to the wavelength of the applied EM radiation, and also the SAR (specific absorption rate) will be influenced by the nature and properties of the surrounding tissues. All of this (plus several other factors) adds up to a complex interaction between an EM field and body tissues containing a passive metallic implant – certainly not a simple relationship. The presence of a metallic implant in the RF field will cause an enhancement in the local field strength and affect the power absorption BUT interestingly, in the papers reviewed here, these changes have not been sufficient to cause damaging thermal changes – most of the changes were under 1 degree C. One would argue that with a continuous mode RF field, it would be prudent to leave passive metallic implants (e.g. pins, plates, wires, metal arthroplasty) on the contraindication list because if nothing else, it is known that they will influence the field strength and energy distribution which COULD have an adverse thermal effect on the local tissues. Unless one is prepared to do some fairly hairy calculations in each instance (based on body composition, size, shape and material of the implant, implant orientation relative to the field etc etc etc), it is easiest and safest to recognise an implant of this type as a local contraindication. Clearly more work is needed in this field, but even a superficial scan of this paper will provide the reader with some idea of the complexity of the problem, and although it affects clinicians, in terms of problem solving, it lies closer to the medical physics arena than the therapy world – unless you happen to have a medical physics and therapy dual qualifications – in which case, you can write the next newsletter . . . . . .

Last one then

Therapy for OA Knee Pain

Jan Bjordal (who gets numerous mentions in these newsletters) has recently published (along with several other esteemed colleagues, including Mark Johnson of TENS – Interferential – Pain fame) a review of several therapies and the efficacy in relation o to OA knee pain (***Bjordal, J. et al (2007) Short term efficacy of physical interventions in osteoarthritic knee pain. A systematic review and meta-analysis of randomised placebo controlled trials. BMC Musculoskeletal Disorders 8(1):51***).

This is not a short review, but it is well constructed and provides some great reading for a commonly encountered clinical problem. Essentially, the authors review 33 RCT's that met the selection criteria (based on a Jadad score) involving a total of almost 2500 patients of whom almost 1400 received active treatment (as opposed to placebo). Several modalities did not appear to offer significant benefit, including manual acupuncture, static magnet therapy and ultrasound. Pulsed shortwave type therapies appear to offer a marginal benefit whereas modalities like TENS, Interferential, electroacupuncture and laser therapy provide clinically significant reduction in pain, and the (limited) follow up effects seem to last for at least 4 weeks. There are dose dependency issues identified in the review (i.e. applying the modality at the 'optimal' dose provides additional advantage – which ties up neatly with the material I was mentioning earlier and presented at WCPT). There are lots of interesting issues raised in this work, and the authors are to be commended (again) for some quality review material. An interesting read for anybody dealing with OA as a clinical issue, for anybody with in interest in using various forms of electrotherapy in the clinical environment, and anybody wanting to read a well constructed review and meta-analysis all in one.

OK, so that is it for the moment. Just this morning I have come across a new paper on anti-inflammatories and tissue repair, but it is going to have to wait till the September issue as I MUST get this one out before you all give up. As ever, please do let me know if you have seen a paper and I appear to have completely missed it out (e mail : t.watson@herts.ac.uk) and don't forget that the website has some basic material on most, if not all of the key modalities (www.electrotherapy.org). There is still a very substantial pile awaiting review and comment, but all feedback welcomed. The next issue will have some more from the WCPT papers and presentations plus a raft of experimental and review mainstream papers.

Regards

Tim

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