Electrotherapy News

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OK folk, here is another edition of Electrotherapy News. Later than I had hoped, but I trust that the somewhat eclectic mix of papers in this edition will stir some interest. Couple of news items first and then on with the recent papers.

NEWS : Web Pages Update

Most of you will have seen by now I guess that I did manage to get the web pages completely restructured over the summer – new menu system and some additional pages. There are still some blanks in there, but hope to be filling them once I have caught up with the backlog of papers that need to be written. If you have any comments, please do let me know. I did try it out on as many different browsers as I could and think that I managed to iron out the problems that I could identify, but if there is anything that I appear to have missed, please do let me know (t.watson@herts.ac.uk).

NEWS : Electrotherapy : Evidence Based Practice

I am pleased (understatement) to say that the final editing and correcting has now been done for the next edition of this textbook. My thanks to all the authors who managed to turn around their chapters so efficiently. My current understanding from the publisher (Elsevier) is that we are looking for a February 2008 'on the shelf' date, so for those of you looking for an updated text with current evidence for (and against) the commonly used modalities, just hang on in there. I will let you know if any further developments . . . .

NEWS : Parkinsons Apomorphine Nodules and Ultrasound Study

I have mentioned this one previously, but just as an update, we have been running a pilot study using ultrasound on the problematic abdominal nodules that patients with Parkinsons tend to get where they inject their Apomorphine. Numerous therapists have used ultrasound over the years to try and reduce the tenderness, hardness and irritability of these nodules, and the anecdotal evidence is pretty supportive, BUT there was nothing published to show whether it actually worked or not. The pilot study involved a raft of nodule measurements, including ultrasound scanning to see what changes actually happened. We are just about done on the data collection and will be processing the results soon. Watch this space and we will let you know the outcome. The pilot work was sponsored by the Parkinsons Disease Society, and of course, our thanks to them for that opportunity to investigate.

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Therapeutic Ultrasound

Progress in Biophysics and Molecular Biology – Special Edition Papers

You may recall that at the start of the Ultrasound section in the last issue, I mentioned that there was a special issue of ***Progress in Biophysics and Molecular Biology (Volume 93 Issues 1-3)*** which included a great range of papers on ultrasound in its various applications. This is not an especially therapy focussed publication, but for those working in the field or looking for quality background information, there is some great stuff in here. There are 27 papers in all, and believe me, I am not going through the whole lot (you would get bored and I would run out of space) but there are a couple of titles that I would draw your attention to and a couple that I will briefly review.

The lead paper is by Leighton : What is Ultrasound? An 80 page read, but for those who want a decent review with some technical info, might be well worth a look (pages 3 – 83). The paper that I did mention last time was by Gail ter

Haar looking at 'Therapeutic Applications of Ultrasound' (comments below), and this is followed by a paper by Leventhall on Infrasound. I get plenty of e mails asking about infrasound – patients seem to be quite keen on purchasing their own units and them taking them to their therapist asking what it does, how it works etc etc. The paper by Leventhall looks at what this 'infrasound' or (low frequency sound) is, what we can and can not hear of it and clears up some of the commonly held misconceptions. Not sure that it would constitute an ideal reference for your patients to read, but if you are interested, then well worth a scan.

Francis Duck (from Bath, UK) has a paper in there on Medical and non medical protection standards – talking amongst other things about safe exposure levels (pages 176 – 191) and a paper by Humphrey (Southampton, UK) looking at the physical interactions between ultrasound energy and matter (pages 195-211). This (predictably) is mostly about the physics of ultrasound but does include sections on heating, streaming, cavitation and propogation – a brief read of which will illustrate the complexities of knowing what happens to ultrasound energy in the tissues.

The two papers that I will spend a bit of time on are the ter Haar paper (pages 111-129) on Therapeutic Applications and the paper on Bone Regeneration (pages 384-398) by Claes and Willie from Germany.

Firstly the ter Haar paper. Many of you will be familiar with the very significant publication history of Gail in relation to ultrasound (therapy and more recently HIFU as a potential treatment for cancer and tumours). Gail is based at the Royal Marsden Institute of Cancer Research, and takes time in this work to put into context the different therapeutic applications of ultrasound from the very low power to the very high power. Even if you are not involved in HIFU, the use of US for tumour 'ablation' and a range of ultrasound applications related to drug uptake, it does help to realise the range of applications that are out there outwith the therapy treatments with which we are most familiar. Whilst the section on therapy ultrasound is brief, the subsequent section on bone healing provides some useful references to go chasing, and makes for a useful introduction to the paper by Claes and Willie coming up next.

Lastly then 'The Enhancement of Bone Regeneration by Ultrasound (pages 384-398). Whenever I talk on post grad courses about the use of ultrasound for the enhancement of fracture healing, it seems to generate a level of interest that goes beyond the time available, and tracking the web site page hits, it looks like the Ultrasound for Fracture Healing pages do take a disproportionate hit rate. LIPUS (low intensity pulsed ultrasound) has been mentioned frequently in this newsletter – almost every issue in fact – but this review gives a useful overview of the cell, animal and clinical studies that have been published and in addition provides 3 pages of reference for those who want to go looking further. I have some more material to go up on the web pages on the use of US in normal and delayed fracture healing, but in the meantime, you might find this to be an exceptionally useful read.

Ultrasound and Scaphoid Non Union

A paper from last year by Ricardo, based in Cuba ***(Ricardo, M. (2006). "The effect of ultrasound on the healing of muscle-pediculated bone graft in scaphoid non-union." Int Orthop 30(2): 123-7)*** provides a useful follow on from the last review. The paper reports the outcome of a double blind trial, using LIPUS as part of the post surgical management of patients who have a problematic scaphoid fracture, requiring a bone graft. 21 patients were recruited to the trial and following standard surgical intervention, they were randomised into a LIPUS and a Placebo group with n = 10 in the active and n = 11 in the placebo groups. The LIPUS treatment parameters were entirely as one would predict from looking at previous work with non unions (30mW/cm2, 20 minutes daily). The design was double blind in that neither the patient nor the clinician knew whether the treatment unit was 'active' or a dummy. Outcome measures included pain, range of movement, radiographic and anatomic markers, and a good follow up (average over 2 years) was achieved. The results show a significantly decreased time to healing (clinical and radiographic) in the active compared with the placebo group (38 days difference on average). All the patients demonstrated union – but that is expected given that all were being followed up post op muscle pedicle graft. The key finding is that the use of LIPUS in the post surgical period results in an increased rate of union and no complications.

Ultrasound Use in Clinical Practice (plus a letter about it)

Wong et al published a paper in Physical Therapy earlier in the year ***(Wong, R. et al. (2007). "A survey of therapeutic ultrasound use by physical therapists who are orthopaedic certified specialists." Phys Ther 87(8): 986-94; discussion 995-100)*** in which the results of a questionnaire (just over 200 returned out of 450 distributed) are reported and discussed. The aim of the work was to evaluate the opinions of orthopaedic specialist practitioners with regards US use, importance and treatment parameters. The paper is a straightforward read, so I'll not go through the results one by one, but in essence the survey generated data along fairly predictable lines showing that soft tissue inflammatory lesions, tissue extensibility, scar remodelling, tissue repair were all fairly common reasons for the modality to be applied,

and this list was supplemented by pain relief, oedema reduction and phonophoresis (drug delivery). The respondents of the questionnaire were asked to report their use of US for a list of conditions (as above) and then there was space for them to include the proverbial 'other'. No problem with this, but one has to bear in mind that the majority of those replying to a questionnaire of this type will stick to the provided list rather than add copious amounts of 'other' categories, and therefore this might serve to skew the data somewhat. Furthermore, one has to be careful when taking these results outside the populationthat was surveyed – clinical specialist orthopaedic physical therapists in the USA – nothing wrong with that – don't get me wrong – but my understanding is that is one were to repeat the survey in the UK, elsewhere in Europe or on Australia for example, you would likely get a different useage profile and dose related comments.

The paper is followed by an invited commentary by Robertson (lead author of the new edition of Electrotherapy Explained) which does not hold the punches and generates some interesting debate – which is further addressed by an author response. If you don't normally bother with these 'additions' to the main paper, then I would encourage you to have a look and see what is there – fascinating and some interesting and valid points raised by both parties. That is not the end of the story, because in a later issue of the same journal (November 07 – how up to date is this newsletter????), there are a couple of letters (one from John Childs and one from Nurudeen Amusat (Vol 87 Issue 11 page 1558) which add two further 'differing' views – all great ammunition if you ever needed a provocative topic for a journal club, student assignment or just a good old staff room debate!! Enjoy

TENS and Blood Flow Changes

There has been considerable debate over the years relating to the effect of electrical stimulation and its effect on local blood flow. It is widely assumed that one of the effects of this intervention is to increase local blood flow, but whenever I have had a serious look through the literature, there is not a lot around out there to back it up. Electrical stimulation when applied at a motor threshold does demonstrate such effects, and so do some applications of electroacupuncture, but the routine application of sensory level TENS or other stimulation has failed (from what I can see) to show significant changes. This recent paper ny Sandberg et al (***Sandberg, M. L. et al. (2007). "Blood flow changes in the trapezius muscle and overlying skin following transcutaneous electrical nerve stimulation." Phys Ther 87(8): 1047-55***) has looked at this in an asymptomatic population and have not only looked at blood flow changes in the skin, but in the underlying muscle (in this case trapezius) too. Thirty three subjects (all women) were recruited and each received one of three different TENS applications : high frequency (80Hz) at a sensory level for 15 minutes : low frequency (2Hz) at a motor level for 15 minutes : high frequency TENS (80Hz) at a sub sensory (subliminal) level for 15 minutes – which was effectively the control condition.

Blood flow was recorded from the trapezius muscle and the skin overlying it before, during and after the intervention using a photoplethysmography technique. This is a widely used technique, but has been (appropriately) criticised for only effectively measuring superficial (skin) blood flow. Sandberg and colleagues describe a new modification of the instrumentation which enables a superficial and/or a deeper tissue flow to be recorded. In the detail of this paper, they describe some of the validation work conducted, though I would plan to follow this up in more depth as it would be great if it works with a non invasive technique.

The TENS at low frequency was actually a burst mode application (8 pulses per burst @ 180 microsec direction) applied at a strength (current intensity) sufficient to generate strong muscle activity around the shoulder region, but not sufficient to be painful. The 'control' application (at 0.5mA) was used so that a 'real' treatment could be applied with no anticipated therapeutic of physiological effect.

The blood flow probe and TENS electrodes were placed around the upper fibres of trapezius (a picture would have been useful here) and sensory and motor hresholds established (described in the paper). Blood flow measures were taken at intervals before, during (15 mins) and after (15 mins) the appropriate stimulation. The values were normalised for each subject (appropriate as we all have different flow levels – it is the change with treatment that is the important thing here). The statistics employed seem to fit the protocol and data just fine.

The results were interesting, though it is worth noting that the data for 5 subjects was excluded (for what appear to be very good reasons) thus the actual analysis is based on n = 28 not the n = 33 as indicated in the abstract. Looking at the summary data plots in the paper, it is not difficult to see that the low frequency TENS has a marked effect on the muscle blood flow, whereas the other TENS modes do not. The skin blood flow does not look significantly different between the three applications. The stats back this impression up, with statistically significant difference between the low frequency and the other two for muscle flow. These significant changes were not sustained for long after the cessation of the TENS. Looks like a few minutes from the plot, but there was nothing of statistical significance. The flow in the opposite shoulder (non stimulated) was also recorded and showed no change. The skin flow did not show any significant changes with any stimulation mode.

There is a substantial discussion, and I would not want to steal the thunder from Sandberg et al but giving you a line by line analysis, but there are some good points well made For example, some interesting points were recognised with regards the applied frequencies and stimulus strengths which give rise to several questions. If the high frequency TENS had been applied at the stronger stim level, would that have generated a flow change? – and all the other combinations for that matter? The authors acknowledge these issues, but you can imagine how many subjects would be needed to test groups that included both high and low frequencies at both sensory and motor stimulation levels – great experiment – but would take a lot of subjects and a lot of time. There were other problems – like the low frequency stim had to be turned off for a short while (20 sec) in order for the flow measures to be taken – may or may not be a critical issue – but there was not a lot of choice as the measures could not be taken during the muscle contraction itself.

There are some good further points made – well worth a read – but the overall finding of this study was that if you apply TENS at a motor level, and at a low frequency, there is a significant increase in local muscle blood flow during the stimulation period. This change is not apparent with other stimulation modes, and even with the low frequency, motor level stim, the increased flow is not sustained for very long at all after cessation of the treatment.

There are a lot of additional questions – how accurate is this new non invasive blood flow measurement for muscle? What about the non tested combinations? What about a longer carry over effect with a longer duration of stimulation etc etc etc, BUT that having been said, we know more now than we did before this was published and it gives a great platform for the next raft of experiments.

TENS and Post Thoracotomy Pain

OK, plenty of other electrical stimulation papers to report, so I had better not get so rambling or else you are going to lose the will to live and I am going to run out of time and space! The next one is also on TENS, but this time to a clinical application – for post thoracotomy pain by Solak et al based in Turkey (***Solak, O. et al. (2007). "Transcutaneous electric nerve stimulation for the treatment of postthoracotomy pain: a randomized prospective study." Thorac Cardiovasc Surg 55(3): 182-5***).

This was a two group study of 40 patients who were randomly allocated to a PCA or a TENS treatment for post operative pain with a variety of outcome measures taken for up to 60 days. VAS for pain (as expected), an additional pain scale (Prince Henry), lung function and medication record.

The TENS stimulation was at 3Hz for 30 minutes at a time with the electrodes (4) placed around the postrolateral thoracotomy scar. TENS was commenced at 4hrs post op and was continued for 10 days. The PCA application was a standard morphine based system,. Patients in both groups were offered additional pain relief should it be needed / required.

The results show that there was no significant difference in pain scores between the groups on days 1 and 2 post operatively. From the 4th through to the 60th day however, there was a statistically significant difference between the pain scores for the groups with the TENS group showing the better pain control. No patients in the TENS group reported pain after day 30 whereas some patients in the PCA group did. There were no statistically significant differences in the lung function outcomes between groups.

Clearly, there are more issues to follow up on, but the demonstration that post operative TENS was not only as effective as a morphine based PCA, but that from day ¾ onwards, it was actually more effective through to day 60 is an impressive result and might lead one to surmise the post thoracotomy TENS should be considered as a viable alternative to morphine based therapy.

Electrical Stimulation and Hypoalgesia

Val Robertson (one of the new authors of Electrotherapy Explained) has been mentioned already in this issue, so now it is the time for Alex Ward (the other new author of that text). He has co-authored a paper with Oliver on the hypoalgesic efficacy of different forms of electrical stimulation (***Ward, A. R. and W. G. Oliver (2007). "Comparison of the hypoalgesic efficacy of low-frequency and burst-modulated kilohertz frequency currents." Phys Ther 87(8): 1056-63***).

A group of 19 asymptomatic subjects (university students) were recruited for this study and each one was given two different forms of electrical stimulation (a monophasic pulsed current at 50Hz and 500 microsec duration and also an

alternating current – at 1kHz modulated at 50Hz giving a 4 millisecond burst duration – equivalent to a 20% duty cycle)) to evaluate differences in hypoalgesia between them. Pulsed currents and alternating currents (like Interferential and Russian stim) have been compared in some recent studies with regards the 'comfort' from the patient perspective and their efficacy in terms of pain relief (reported in previous editions of this news letter – see back issues if you want to check them out).

Having volunteered for the study, the subjects underwent a now familiar cold induced pain regime (previously reported by both Johnson & Tabasam in 1999 and Shanahan in 2006). The procedure is well described an explained in this paper, and the accompanying figure makes it as clear as you could want. Essentially, the test period consists of 60 minutes with a mix of warm (37 degrees C) and cold (0 degrees C) immersion periods. The pain threshold time was taken as the primary indicator (outcome measure) though unlike previous use of this cold pain test, the procedure of holding the hand in the cold water for an additional 30 seconds after having reported pain was no adopted (lucky volunteers!). The treatment was applied during cycles 3 and 4 and lasted 20 minutes. Electrodes were placed on the anterior and posterior forearm and one or the other forms of electrical stimulation was applied with the previously identified parameters – more details in the paper.. The intensity was controlled by the researcher, but the level of stimulation was determined by the subject – the classic 'strong but comfortable' level.

The data was analysed in order to answer two key questions – whether the different types of stim provided a significant increase in the pain threshold and whether there was any difference between the two treatments. There were big differences between individual subjects – as one might anticipate – but the detailed statistical testing showed that the time taken to reach the pain threshold during the stimulation phases was longer (by about 20%) than during the non stimulation periods. Looking at the charts in the paper, the average difference between the treatment phases are clear, BUT due to the wide variation in individual responses (large standard deviations) the group results did not demonstrate significant differences – i.e. that although both interventions generated an significant increase in the time taken to reach the pain threshold, there was no statistically significant difference between the interventions.

The lack of significant results might be attributable to several factors – individual differences being large, a big placebo effect, small sample or actually that there is not a difference in effect. The authors discuss these factors – interestingly, they have done a post hoc power calculation and reckon that they would need 100 or more subjects to run the trial at sufficient power – nightmare scenario in terms of recruitment! They raise some additional points which are on interest, but I would take the key take home messages from this that at the same frequency (50Hz) the pulsed current and the alternating current both provide a measurable hypoalgesic effect in non injured subjects. Trails on patients with real pain are still needed and repetition of this type of trial comparing further different current forms and application frequencies would be most welcomed.

NMES and Rheumatology

This is a relatively long paper from a group in Pittsburgh, USA which is a multiple patient case report looking at the use of NMES and exercise for patients with Rheumatoid Arthritis (***Piva, S. et al. (2007). "Neuromuscular electrical stimulation and volitional exercise for individuals with rheumatoid arthritis: a multiple-patient case report." Phys Ther 87(8): 1064-77***).

The NMES (neuromuscular electrical stimulation) was used on the quads in conjunction with an active exercise programme on a group of 7 rheumatoid patients. Unfortunately only 4 patients completed the programme (16 weeks), but the plan had been to evaluate the merits of including the NMES in the programme, looking at muscle mass, strength and function, and in addition considering the patient perspective including adherence to the treatment. With only 4 patients finishing enough of the programme to be 'counted', it was unlikely to come to any earth shattering conclusions, but that should not devalue the work, and it makes for a very useful read for anybody involved in the treatment and care of patients with Rheumatoid or those involved in NMES treatment programmes.

Pre treatment measures of quads muscle mass (CT scan) and quads strength, times chair rise test and two physical function scores (LEFS and HAQ) were taken – all of which are described in nice detail in the paper. The intervention was of 16 weeks, and consisted a combination of 'attended' (12) sessions and self management at home. Patient adherence to the sessions and the home programme was evaluated as a part of this research. The NMEs parameters are

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

summarised thus : large skin electrodes – proximal v. lateralis and distal v.medialis using a battery powered stimulator. Stim was applied at 75Hz @250 microsec pulse duration, 4 sec ramp up, 6 second stim and 4 sec ramp down (14 second cycle) followed by 50 seconds rest (based on previously published quads stim work). The current intensity was sufficient to produce a strong titanic muscle contraction, starting as 10 contractions per session and progressing up to 30 contractions by week 2. Self treatment period appear to have varied between about 10 minutes up to 1 hr daily.

There were measurable changes in quads mass (from CT scan) averaging some 7.5%, but there was a wide range and certainly there appears to be a relationship between changes recorded and treatment compliance (as we might expect). The muscle strength (quads torque) also demonstrated an overall (mean) improvement with wide variability. The times chair rise outcomes showed an 24% improvement for those that were compliant and the questionnaire results are briefly discussed in the paper on a patient by patient basis.

It certainly looks like the patients that adhered to the programme got useful changes in outcomes measured, and from the authors comments, it looks like those that were compliant for at least 1/3 of the schedule did alright. There was no evaluation of whether adding the NMES to an exercise programme is of benefit – but that would take a controlled trial of some kind, and that is not what the authors set out to achieve and report. The detailed discussion and suggestions contained therein make for an interesting read and will provide some insightful material which I would encourage you to go read – especially if you are involved with any aspect of NMES or care with Rheumatoid type patients. Researchers and clinicians who report on the basis of case studies sometimes get a hard time of it, which is a tad unfair in many respects because (a) they HAVE done something and reported it and (B) it is not an RCT, so there is no point applying RCT critical review systems. Case studies DO provide some useful insights an the outcome of this work should help to inform future research in both fields.

Nerve Stimulation and Pain

This is an interesting diversion for the standard 'therapy' type applications that I tend to report in the newsletter, but it might be of interest to some, hence its inclusion in this issue. Faraj and Mulholland, based in Nottingham (spinal surgery) have evaluated the potential benefit of using electrical stimulation to increase the effectiveness of nerve root infiltration for patients with a range of spinal problems. The paper was published last year (***Faraj, A. A. and R. C. Mulholland (2006). "The value of nerve root infiltration for leg pain when used with a nerve stimulator." Eur Spine J 15(10): 1495-9***) and a group of patients who were going to be treated with nerve root infiltration (96 of them) were either treated in the normal way (n=39) or with a normal nerve infiltrating, but using an electrical stimulator to help to identify the most appropriate nerve root to treat (n=57). The problem, as outlined by the authors) is that you have to get the right nerve root (bear with me) and that by increasing the accuracy of nerve root identification, the outcome could be improved. A quick summary of the outcome goes like this : overall, the pain response rate to the nerve block when using the electrical stimulation was 96% and the equivalent rate with no use of stimulation was 79%. The increase in efficacy of the intervention is a useful margin. The authors note (an interesting side issue to some extent) that some pathologies are less responsive to nerve infiltration as an intervention, but of the 'responders' in terms of pathology, the use of concomitant electrical stimulation in order to increase the accuracy of the infiltration has a beneficial effect.

Electrical Stimulation and Spinal Fusions

Another electrical stimulation paper now, and possibly another one that some of you might find somewhat peripheral to your main field of interest. Electrotherapy (in various forms) post spinal fusion is not new, and since the mid 70's, various different form of electrotherapy have been advocated in an attempt to reduce the sometimes worryingly high failure rate of this procedure (up to 40%). Despite the title of this paper, the review does not only consider electrical stimulation in the classic sense, but includes capacitive coupling, PEMF's and magnetic field therapies. It aims to evaluate the mechanisms through which these interventions might be working (very similar to the reviews on electrotherapy and fracture healing that I have included over the last year or so) with various growth factors and cytokines being the likely mechanisms. The clinically positive outcomes are documented, and although the paper covers both animal/experimental research in addition to clinical trials, the overall view is positive. If you want to know the final answer, I will summarise it here, but be warned, this is my one paragraph version of a 10 page review and almost 70 references! The various forms of 'electrical stimulation' as defined by the authors are effective, but they are not equally so. The direct current approaches appear to be in the lead at the moment (this would concur with reviews on electrical stimulation for bone and wound healing that I have come up with over the last 8 or 10 years). Inductive coupling and Capacitive coupling are not ineffective (sorry about another double negative), but the DC stim appears to have the edge. The mechanism of action is most likely to be related to an upregulation in various growth factors and cytokines (just like the fracture healing work) and it is suggested that this work has potential for other musculoskeletal problems and soft tissue work . . . . . ummmmmm think that we are already working on that one!!!

FES, Gait and Stroke

Here is one from a group in Brazil looking at gait training and Functional Electrical Stimulation (FES) post stroke – maybe a bit more clinically focussed for those of you who wonder where I am going in this issue! Lindquist et al (Lindquist, A. et al. (2007). "Gait training combining partial body-weight support, a treadmill, and functional electrical stimulation: effects on poststroke gait." Phys Ther 87(9): 1144-54) have published in Physical Therapy, so it should be reasonably easy for you to get access to the full paper should you want to follow this one up. It is essentially a case series report of 8 patients post stroke who went through an A-B-A type case study protocol. The A bits involved treadmill training combined with body weight support (BWS). The B bit in the middle (the different intervention) involved the addition of functional electrical stimulation (FES) in addition to the previously identified therapies.

There were a lot of outcome measures (9 for gait outcomes alone) but included spasticity (modified Ashworth), functional walking (Functional Ambulation Category Test), motor function (STREAM) and gait analysis using a 5 camera system with body markers. The body weight support was provided by an overhead gantry type system which allowed control over the % body weight actually supported by the technology, starting at around 30% and reducing with time, getting down to under 20%. The treadmill speed was also progressively increased session on session depending on the ability and progress of the patient. The FES was applied over the common peroneal nerve giving a tibialis anterior response – hence an electrically reinforced dorsiflexion – during the swing phase of gait. The A – B – A design was over a 9 week period, with 3 equal divisions – FES only being employed during the middle (B) period.

The overall results are like this : Motor recovery during the A period was 56% and during the FES (B) period, went up to 71% which (looking at the data plots) appears to have been maintained in the follow up A period (this is a gross simplification of the motor outcome results). Some gait parameters showed significant change after the combined BWS and FES period; for example single limb stance was better as was swing symmetry and cycle length symmetry. Cadence and gait speed also increased after the B phase. There is a lot more to it than that, but you will get the gist I am sure.

There is a good discussion (there have been several in this issue I am pleased to say) and the authors conclude that for patients in a chronic post stroke status are likely to benefit from a combined BWS and FES programme

Microcurrent therapy and Arthroplasty

We are currently doing some work on Microcurrent therapy (as I have mentioned in a previous edition), so it was nice to find a paper from earlier this year from a research group in Egypt, looking at the effect of microcurrent therapy (by means of skin patches) in post op knees as a means of pain control (***El-Husseini, T. et al. (2007). "Microcurrent skin patches for postoperative pain control in total knee arthroplasty: a pilot study." Int Orthop 31(2): 229-33***).

The basic trail design was a comparative study of a group of 24 patients divided into a pain medication only group (tramadol) and a group receiving tramadol and microcurrent therapy. Both groups were managed for 10 days post operatively (following TKA). Outcome measures were pain (with VAS), drug dose, wound fluid drained and wound healing status.

The microcurrent therapy (MCT) was applied using a commercially available patch application (Painmaster Patch) which was applied immediately post operatively and did not interfere with the operative site or the dressings. The tramadol was given IM up to 400 mg/day. Follow up was for 10 post operative days.

The VAS pain scored were better in the MCT + tramadol group than the tramadol alone group (throughout the 10 days). The MCT group also used significantly less tramadol than the 'control' group (drug only). The wound healing scores were better in the MCT group as was the wound draining volume.

These are interesting results. I have looked at microcurrent therapy in various forms over the last few years as it has all the potential to be an interesting therapy with great promise – already reasonably well established in areas like wound

healing (Watson 2002, 2008) and fracture healing. Its use in these circumstances, primarily as a pain relief modality, but also considering the gross signs of repair makes for an interesting combination. It is a shame that in this brief paper, there are almost no details about the MCT protocol, stimulation parameters etc, but the overall results were encouraging, and certainly paves the way for a larger scale trial for similar patient groups.

Iontophoresis and Antibiotics

Nearly there with the electrical stimulation batch in this issue – makes a change to have more in this section than in the Ultrasound bit! A paper on iontophoresis will make a change – though there has already been some mention of it in the last couple of Newsletters. This next paper by Khoo et al (***Khoo, P. P. et al. (2006). "Iontophoresis of antibiotics into segmental allografts." J Bone Joint Surg Br 88(9): 1149-57***) is published in a respected and widely available journal, so should not be too hard to get hold of a copy if you were inclined to do so.

This is not a routine therapy type use of iontophoresis, but is an interesting development. In fact, if you search the iontophoresis literature, there are many applications evidenced which are not classic therapy treatments, so in that respect, this fits the typical profile!

The use of cortical bone allografts for revision arthroplasty and for bone replacement following tumour resection is reasonably common, as is (unfortunately) the incidence of deep tissue infection in the post operative period. The aim of this study was to try and see if, by using an iontophoresis technique in the theatre, prior to implantation gave rise to a lower infection rate and hence magnitude of the complication for the surgery.

A total of 31 patients were involved in the study and the iontophoresis application was designed to 'load' the graft with both gentamycin and flucloxacillin. A total of 34 allografts were evaluated with a substantial follow up period of some 2 years. The iontophoresis procedure in the theatre is detailed, though I will not replicate it here as I doubt that any of you would get called upon to perform this technique. Some of the 'left over' bone was subjected to further lab studies to evaluate the efficacy of the gentamycin and fluclox treatments.

There were some data sets that were incomplete (fully explained in the paper) and a total of 24 patients (26 allografts) were available for the 2 year follow up schedule. Of these, there were no early infection identified, and even the late infections ran at under 8% (reported rates in the literature go up to almost 30%). The authors describe a range of 'events' and complications that were noted across the group and the outcomes of the lab studies. It was shown that the concentrations of the iontophoresed drugs around the allograft reached bactericidal concentrations. The authors argue that this method of treating the graft prior to operation gives a high level of the drugs dispersed throughout the graft.

Although not classic therapy in the terms that I would normally report research, I was just interested in this one, so though that I would pass it on in case you find it of value.

Electroacupuncture and Cancer Pain

OK, this is the last of the 'electrical stimulation' type papers for this issue – another paper from last year looking at electroacupuncture for cancer pain using an animal (mouse) model from a research group in China (Mao-Ying, Q. L. et al. (2006). "Stage-dependent analgesia of electro-acupuncture in a mouse model of cutaneous cancer pain." Eur J Pain 10(8): 689-94).

Male mice (6 weeks old) were inoculated with prepared melanoma cells into the hindpaw. Mice were then randomly divided into electroacupuncture (EA), sham EA and a non treatment group. The electroacupuncture was applied to the ST-36 point (behind the knee) and BL-60 (around the ankle) – plenty of detail in the description in the paper for those that want it. The needles in these 2 locations were connected to the stimulator and the current applied using alternating trains of 'dense-sparse' frequencies (100Hz for 1.05 sec and 4Hz for 2.85 sec alternately using bidirectional asymmetric pulses and 0.6ms pulse duration and a current intensity of approximately 1 milliamp and a stimulation duration of 30 minutes. EA was applied every other day.

The key outcomes were the paw withdrawal latency (to radiant heat stimulus) and a 'hot plate' test) both detailed in the paper). The melamoma size did not change significantly until 7 days post injection and became apparent by day 12. There are many results, so I will summarise the key EA findings and leave those with a specific interest in EA or cancer related pain to fully evaluate the results data. The EA was started on day 8 after inoculation and repeated every other day. There was an increase in the analgesic effect, with a significant rise after the second session and a peak after he

third session. The sham group (ame handling but no EA) showed no significant changes in pain scores and responses. The effects of the EA appeared to be more pronounced in the earlier stages, and when EA was not started until day 16 there was no significant therapeutic effect. (day 16 is a 'late stage' in this mouse cancer model). The EA on day 8 (first session) showed significant analgesic effect immediately after treatment, reaching its peak some 15-30 ,minutes after application and disappearing by about 50 minutes. When the same tests were repeated on day 20, they did not the same 'immediate' effect. The authors conclude that EA has a significant effect on early melanoma pain, but much less so with late stage. It is of course difficult to make a direct transfer between a mouse melanoma model and human patients with a wide variety of cancer types and pain behaviours. It is interesting however that such marked analgesic effects were achieved, especially in the earlier stages.

Ice Therapy and Migraine

Change of subject now, and time to wander down another 'different' route – looking at the potential value of ice therapy on migraine using a non controlled research design. This paper from Ucler et al (***Ucler, S. (2006) Cold therapy in migraine patients: Open-label, non controlled pilot study. eCAM 3(4);489-493***). A group of twenty eight patients with migraine were recruited, though the data from only 26 was evaluated (due to drop out). The cold was applied via a gel cap for 25 minutes and patients were investigated over a 2 migraine period using VAS scores and a diary. The VAS data was collected before the treatment and then at intervals post treatment though to 3 hours. The gel cap was otherwise stored in the freezer. The clinical change that being looked for was a 25% reduction in headache severity from pre treatment baseline.

The patients were all female with an average migraine incidence of some 3 a month. The results show that on the first treatment ice therapy after the first migraine episode) half of the patients reported a clinical benefit after the 25 minutes and some patients had a 'complete' response. The VAS scores throughout the follow up period were lower than the pre treatment level. A slightly higher response rate (57% occurred after the second intervention. For the most part, the patients that responded on the first occasion did so on the second occasion too (but not always).

The authors acknowledge that this is not an RCT, there is no control group, and an RCT is clearly needed, but it does demonstrate what appears to be significant clinical benefit for some patients at least (maybe around 60%)..

Radiofrequency intervention and nerve regeneration

A research group, based primarily in the States, but with some representation from Japan have published a paper relating to the use of radiofrequency energy as a treatment and the subsequent nerve ablation and regeneration. In this particular animal study (***Ochiai, N. et al. (2007). "Nerve regeneration after radiofrequency application." Am J Sports Med 35(11): 1940-4***) the authors reasoned that following radiofrequency treatment (for example, as an intervention for chronic tendinosis), the subsequent pain relief that is reported my in fact be partly related to sensory nerve fibre ablation or degeneration. The treatment applied here (not your standard type of shortwave or pulsed shortwave by the way) is a relatively new approach to chronic tendonosis (bipolar radiofrequecy microtenotomy) and early results suggest that in addition to the short term pain relief following the procedure, there can be pain relief / reduction for up to 2 years. This coupled with the proposal that part of the pathology of these lesions may involve overactivity of free nerve endings, means that if the intervention actually caused a nerve degeneration or ablation, it might be in part responsible for the pain outcome. Eighteen rats were treated (hindpaw) using the bipolar radiofrequecy technique and then followed up for between 30 and 90 days. The intervention was unilateral, and the opposite limb served as a control. In short, they identified a significant degeneration of sensory nerve fibres following treatment and that by 90 days post intervention, there was evidence of complete degeneration. The main point of identifying this particular paper was not to 'promote' the use of bipolar radiofrequency treatment (though it is interesting in the context of chronic tendinosis problems), but o raise awareness, that if you are participating the in the post op care and/or rehabilitation of one of these patients, that there is a real sensory nerve deficit post op which might have implications for treatment. That the patient reports marked pain relief is not surprising if the sensory nerves are in a degenerate state post op. Nerve recovery appears to take place (at least in this animal model), but the presence of a sensory nerve deficit would be expected to have an effect of proprioception, sensory feedback loops and thus, motor control : worth bearing in mind – especially if this becomes a more prevalent management tool for these chronic problems (currently used in Achilles problems, plantar fasciitis and tennis elbow). I know it might be of peripheral interest, but thought that I would throw it into the pot for your consideration.

Laser and Chronic Low Back Pain

OK, change of modality now and a more directly related clinical paper from a research group in Iran looking as laser therapy and clinical back pain management (Djavid, G. E. et al. (2007). "In chronic low back pain, low level laser therapy combined with exercise is more beneficial than exercise alone in the long term: a randomised trial." Aust J Physiother 53(3): 155-60). Reporting in the Australian Journal, the authors describe a 3 group clinical trial using a randomised allocation and assessor blinding. Patients with low back pain (at least 12 weeks duration) were either treated with laser therapy, laser therapy with exercise or placebo laser with exercise (2 x weekly for 6 weeks). Pain (VAS), Range of movement and disability (Oswestry Disability Index) were the key outcomes taken at baseline, at the end of the 6 weeks treatment and again 6 weeks post treatment.

From the initial 84 patients, 61 were recruited to the trial (2 groups of 20 and one of 21) with results on 58 at the end of the treatment period and on 53 at the end of the follow up period. The data analysis showed no difference between groups at the end of the treatment period for any of the outcomes. At the end of the 6 week follow up, there was no significant difference between the laser and placebo laser plus exercise groups, BUT there was a difference between the two groups undertaking exercise : laser with exercise group did significantly better than the placebo laser plus exercise group, thus bring the authors to their conclusion that the addition of laser therapy to the exercise programme makes a difference to the outcomes that they evaluated. Clearly, there was not an exercise only group (which would have been useful) but the comparison of an equivalent exercise programme for both groups, and the only difference being wither that had real or placebo laser therapy certainly indicates an effect. The changes in VAS, movement and Oswestry scores were not (in my view) massive, though I would not deny that they were significant. The difference between statistical significance and clinical significance is something that I have been on about for years, but nevertheless, changes were shown, the trial was of a randomised and blinded design, it was a clinical trial with real patients and an intention to treat analysis was conducted – all contributing to the overall quality of the research. The laser treatment ‘dose’ for those who might be wondering was : 810nm wavelength GaAlAs laser with 50mW power, continuous mode and just over 0.2cm2 spot giving 27J/cm2 over a 20 minute treatment period.

Electrodermal Activity and Trigger Points

Several people have asked me if I had seen this paper, and although I was going to include it anyway in this issue, thank you to those who took the trouble to bring it to my attention. Shultz et al (Shultz, S. P. et al. (2007). "The evaluation of electrodermal properties in the identification of myofascial trigger points." Arch Phys Med Rehabil 88(6): 780-4) has looked at whether there is a link between skin resistance measures and trigger point locations and also to see whether it is possible to differentiate between different trigger point ‘states’ by using this technique. Certainly the links between skin resistance and acupuncture points have been evaluated numerous times before – and numerous therapists use this phenomenon when locating acupuncture points with a ‘locator’ type device – it is looking for points of particularly low (or different) skin resistance. Myofascial pain and trigger point integration into therapy is widespread, and although I would not wan to wander off into some trigger point theory discussion here, I find it of some considerable interest, and from conversations with practicing therapists, I am aware that it is widely used in treatment.

For this trial, 49 volunteers were recruited and were assessed for their trigger point status (in trapezius). 21 of them fell into an ‘absent’ myofascial trigger point (MTP) group. 28 were allocated to an MTP treatment group which was then further subdivided into a ‘latent’ (n=16) or ‘active’ (n=12) MTP group.

Trigger points were identified using a clinical assessment technique, marked and then pain pressure thresholds and verbal pain ratings were taken. The electrodermal testing followed a particular protocol – fully described in the paper) involving a 16 point assessment grid which was fixed in a particular way over the area. Using skin resistance measurement equipment, and with an acromial ground (reference) electrode, the skin resistance was then measured in each of the 16 ‘holes’ in the grid surrounding the identified MTP. The reliability of this procedure is reported in the paper, and the environment was controlled (as it is known to make a difference). The results were an average of 3 readings for each of the 16 ‘holes’ which by design, were at different distances from the actual MTP (described by the authors in terms of ‘rings’). For the subjects in the ‘absent’ MTP group – in case you were wondering – the grid was located at a position over the trapezius that was a commonly identified MTP area.

The results showed that there was a statistically significant difference in resistance between locations on the grid. For those with an MTP, there was a significant difference between the skin resistance at the MTP pint, and both the first and second rings (whether the MTP was ‘latent’ or ‘active’.

There is a full representation of the mean data for all three groups in a table published as a part of this paper – well worth a look – and the discussion that following raises some good points and is worth a read (though I always say that – but it is true!). The skin resistance certainly was shown to increase with distance from the actual MTP point (derived from the clinical test) and the authors suggest that using a skin resistance method for identifying the location of an MTP is realistic and supported by this data. There is more to all this than just finding trigger points using a machine (in my view anyway). The fact that the tissue over an MTP (and for that matter, an acupuncture point) is 'different' from the surrounding skin and different from 'normal' means that something is going on. The authors of this paper include in their discussion what that might be, and I have some ideas in that my PhD studies (several years ago now) were concerned with measuring electrical changes in the skin, comparing normal and injured tissue states, and trying to track the repair process with some variation on this theme. That is a different story, but using skin electrical changes as an indicator of something happening in the deeper tissues is not as daft as it might sound – it is based on some good physiology, biophysics and real findings.

Balneotherapy, Mud Packs and OA

Another paper that might seem a bit peripheral to electrotherapy and tissue repair (the mainstay of this newsletter) was published earlier this year in Joint Bone Spine (***Evcik, D. et al. (2007). "The efficacy of balneotherapy and mud-pack therapy in patients with knee osteoarthritis." Joint Bone Spine 74(1): 60-5***) looking at a couple of therapies in relation to their effect on OA knee. I will keep the summary on this one brief, but it is easy enough to get hold of if you are interested. 80 patients with OA knee were divided into three treatment groups (Balneotherapy, n=25, Mud Pack therapy, n=29 and Hot pack therapy, n=26). Treatment for all groups was a daily 20 minute session (10 sessions in 2 weeks) and the outcomes included pain, quality of life and functional outcomes plus walking distance and other clinical assessments taken pre and up to 3 months post intervention.

The treatments resulted in improvements in almost all scores in all groups. The pain and WOMAC scores showed significant improvement in all groups. Quality of life scores improved for the balneotherapy and mud pack groups but not for the hot pack therapy group and the same was true for the walking assessment. The clinical (physician) assessment outcomes improved in all groups. The balneotherapy (in case you were wondering) involves bathing in mineral spring water (at 36 degrees C). The mud packs and the hot packs were both employed at a temperature of 42 degrees C.

Heat therapies – of various kinds – have been used in 'arthritic' conditions for many years, though in some parts of the world, the use of heat has become less popular as a formal 'treatment'. The evidence on the physiological effects of heat therapies is actually quite interesting (Tim's comment – not trying to attribute it to these authors) and in many ways, it is a shame that a therapy with know and significant physiological and therapeutic effects has become less widely used – but that is another story for another day! Many have argued that the effects of heat are only dealing with the symptoms of the problem – and to some extent, that is going to be true, but from the patient perspective, if they feel better – and in this study, have not only less pain, but also a better quality of life from their perspective – what is wrong with that??? The fact that these changes were demonstrable at the three month follow up is of interest – something beyond the immediate 'nice feeling of the warmth' appears to be going on and is worth pursuing I would reckon.

Magnetic Field Therapy – Review

The last of the 'electrotherapy' papers is a review – and a big one at that – published this year by Markov and relating to Magnetic Field Therapy (***Markov, M. S. (2007). "Magnetic field therapy: a review." Electromagn Biol Med 26(1): 1-23***). Markov has published several papers in this field, and this review aims to consider the physics, physiology and therapy (including dose issues) for magnetic and electromagnetic stimulation. There are 7 pages of references at the end of the work, so if you are hunting for a decent source or recent review information relating to magnetic therapy, this would be well worth a look.

I do get asked about this topic with some regularity as many therapists are using more and more of it with their patients and want to know what the evidence out there says. OK, so now you can have a look and see what Markov thinks. The one area where I see a significant issue with the magnetic type therapies is in the field of doseage. I find it difficult to see clear trends from the evidence about whether we should be using very low power fields for hours at a time (maybe all day), or use a stronger field, but for a shorter period each day or whether to go for a very strong field applied relatively infrequently – once a week or something like that. I am pleased to see that – if nothing else – Markov appears to have come to the same conclusion – that the dose issues do not appear to have been resolved and remain one of the most obvious factors that limit the current clinical application of magnetic energy as a therapeutic intervention. That is not to say – before I get a bucketload of hate mail – that it does not work, is useless and should not be used. I have a

lot of time for the potential value of magnetic energy – I would fully expect it to have effects which we should be able to harness – just that at the moment, and based on the evidence that I have seen – getting the right amount of energy into the tissues to do job A or job B is the tricky bit.

Anti Inflammatories and Tendon Healing

The last 3 papers in this issue relate to various aspects of tissue repair, and he first of these looks at the effect of various ant inflammatory drugs on the healing strength at the tendon-bone junction. Ferry et al writing in the Am J Sports Medicine earlier this year (Ferry, S. T. et al. (2007). "The Effects of Common Anti-Inflammatory Drugs on the Healing Rat Patellar Tendon." Am J Sports Med35 (8);1326-1333).

The trial evaluated the effect of several different non steroidal anti inflammatory drugs on healing using an animal (rat) model with a 7 sub group design. All animals were exposed to a surgical transaction of the patellar tendon at the inferior pole which was then repaired. The groups were given different analgesic drug therapies for 14 days – apart from group 7 who acted as controls. The drugs were : ibuprofen : acetaminophen : naproxen : piroxicam : celecoxib : valdecoxib.

A total of 215 rats were used for this work – not a small sample by any means – though in the first (main) run, 180 were used and data from 16 were excluded for a variety of reasons detailed in the paper – thus leaving 164 in the main analysis, and at the end of the 14 day period the mechanical and biochemical testing of the extensor mechanism was evaluated for each of the groups. There was a significant difference between the meachnical strength of the controls and three of the drug groups (celecoxib, valdecoxib and piroxicam) with the controls being the stronger in each case. The acetaminophen and ibuprofen groups were significantly stronger than the three previous drug groups, but not significantly different than the controls. There were failures of the suture in 23 cases divided between the following groups : naproxen (n=3), prioxicam (n=4), celecoxib (n=6) and valdecoxib (n=10) with a significant distribution of these failures (i.e. is was not a statistically 'random' distribution.

Collagen content and proteoglycan content at the repair site was assessed using standard techniques and the results followed a very similar pattern to the biomechanical testing identified about (details in the paper with data and plots)

The results demonstrate different responses to this drug intervention and mechanical testing model. Some drugs had a demonstrably detrimental effect whilst others were not significantly different from the controls. The COX-2 group drugs demonstrated the most significant effects. Ibuprofen came out with the least detrimental effects, and the authors were surprised by this outcome (based on previous work) and include in the discussion several reasons why this might be the case. The results are more complex than I have described in this summary and are worthy of a more detailed evaluation if you are involved in this field or have an ongoing interest in the influence of anti inflammatory drugs on tissue repair. Clearly with an animal experiment, one has to be cautious with regards direct transfer of results from rat to human. It is suggested that a 14 day drug programme in the rat might equate to a 2 month equivalent in humans.

At the end of the day, the COX-2 selective drugs demonstrated the most significant detrimental changes. Naproxen and piroxicam also showed changes, but they were not so marked, and ibuprofen showed no changes of significance compared with controls. Although one might query some of the findings, this paper does raise some pertinent issues and certainly adds to the debate about the use of anti inflammatories in the post operative period.

Achilles Tendinosis

A high proportion of the issues of electrotherapy news have some mention of Achilles Tendinosis – highlighting the ongoing debate over this particular clinical problem – whether it is (or is not) and inflammatory lesion, what the changes are in the tissue and what that might mean in terms of clinical intervention. A recent paper from a group in Holland (***de Mos, M. et al. (2007). "Achilles tendinosis: changes in biochemical composition and collagen turnover rate." Am J Sports Med 35(9): 1549-56***) have made the penultimate contribute to this issue with a paper looking at changes in biochemistry and collagen physiology in a group of patients who were undergoing surgery for Achilles tendinopathy. Analysis of water content, percentage of denatured collagen, various biochemical assays were analysed from biopsies taken from 10 patients. The samples were taken from the lesion site and from adjacent 'healthy' tendon and additional samples were taken from 3 subjects with asymptomatic tendons.

The obvious changes in the lesion samples included a higher than normal water content and a higher amount of denatured and damaged collagen. There were changes in almost all the biochemical and enzyme functions evaluated except for the enzymztic crosslink data. The patients all fell into a chronic Achilles tendinopathy group with a mean age of 46 years and a mean problem duration of almost 2 years. Eccentric exercise training had been undertaken by all pa-

tients for 3 months prior to the surgical Debridement (which is what they were going to surgery for when the biopsies were taken). The 3 healthy samples were taken from patients attend for other surgical procedures (described in the methods section).

There are a lot of results, and some of the biochemical data gets a bit complex (though still interesting). None of the samples fro the 10 patients showed evidence of tendon rupture or inflammatory lesions and on a rating scale, the majority of the sampled tendons fell into the mild-moderate tendinosis band. Interestingly, with the samples taken from the 'normal' tissue adjacent to the lesion, there were still signs of collagen degeneration. There were some pathological findings in the samples from 'normals'. But there was still a significant difference between the samples from normals and the 'normal samples' taken from tissue adjacent to the lesions (if you see what I mean). It appears that the 'wettest' samples were those from the TA lesions, followed by the samples from the adjacent tissue and the least wet were the samples from 'normal' samples. The percentage of denatured collagen was highest in the lesion samples and the cross link analysis showed its lowest level in the lesion samples.

The authors suggest that taken as a whole, these results show that the samples from the lesion proper demonstrate aberrant collagen structure and a high collagen turnover rate. There is an interesting element to the discussion in which these findings are compared with previously published findings from supraspinatus analysis. Overall, it is suggested that the chronic Achilles tendinosis lesions studied there is an increase in collagen turnover which is reckoned to be related to an exaggerated repair response. The surrounding tissue was also abnormal despite its apparently normal appearance. This may offer some indicators for therapy and intervention strategies, and bearing in mind that all these patients had been through a 3 month eccentric loading programme prior to the biopsies having been taken, it also raises some interesting questions along that line. Would be fascinating of course to have comparable data from a patient group who had not been exposed to the eccentric loading to see what the difference might be . . . . . another experiment coming along for somebody . . . .

Growth factors and Fracture Healing

The last one folks . . . . fracture healing and growth factors – a trendy research area and one that holds significant potential for developing treatment protocols in the future maybe? This paper is a review rather than a report of direct experimental evidence, and hence somebody (Simpson et al in this case) has done a lot of the donkey work and present their findings (***Simpson, A. et al. (2006). "The role of growth factors and related agents in accelerating fracture healing." J Bone Joint Surg Br 88(6): 701-5***).

I'll not steal all their thunder, and as the paper is pretty easy to get hold of being in a mainstream journal, it would be worth a look for yourself. The paper does not look at therapy for fractures in the classic mode, but does look at the role of various growth factors in the process of fracture healing – something that I have reported on more than one occasion in previous issues – and outlines how these might be employed as a fracture management therapy – delivery of the factors by direct or indirect means to enhance repair. The factors that generate the strongest part of this review are the BMP's (bone morphogenetic proteins) which are similar to TGF-beta (transforming growth factor)and the lab, animal and clinical evidence in this field is usefully, if briefly reviewed. As with the LIPUS type treatment that I have mentioned many times, it is unlikely that growth factor intervention is likely to become routine for every fracture – though I might be completely wrong here – but might be best targeted at the problem and potential problem fractures, thus staving off a longer term problem. Not mainstream therapy as yet, and may not even fall into the therapy realm at all, but of interest in relation to fracture healing, and a great review – quick and easy to read and digest.

OK, so that will do for the moment. The next edition is now scheduled for January 2008. It has taken too long to get this one put to bed to be able to squeeze another one out before Christmas. I will wish you a pleasant Christmas time (if you celebrate such things) and good New Year and look forward to hearing your views, comments and suggestions.

If you have any papers that I appear to have missed, please do let me know and I will get them included somehow if I can. Much as I scan a lot of journals and index systems, I have no doubt that I miss papers and my thanks to those people who e mail and make suggestions – at least I get the impression that somebody reads all this!

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).

Regards

Tim

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