Electrotherapy News

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News : Web Site Search option

Most of you will have received the 'extra' message that I sent out a short while ago about the new search function that has been incorporated into the web site. There is a LOT of material there now (increasing all the time) and a lot of the e mails that I get every week ask about stuff that I know is up there, but clearly was not found by the person asking. The advantage of this trick is that when you include your search term(s), it will also come up with previous entries in Electrotherapy News

News : Textbook – Electrotherapy : Evidence Based Practice

Last mention for this one (I almost promise!). The book is out and on the shelf. Thanks to those of you who have already e mailed with your comments (and suggestions). The details are up on the web site (on the Book page as you might expect). I have also done a chapter on Electrotherapy in the new edition of Tidy's Physiotherapy (edited by Stuart Porter and also published by Elsevier). It was a tad tricky getting the whole of the subject into a single chapter, and is therefore a bit superficial in places, but hopefully might serve a useful propose to some readership or another.

News : Ultrasound Gel study extension

You may recall some while ago we published the results of a couple of studies concerned with ultrasound transmission through various different gels and also through wound dressings. We are currently extending the work, and are now testing the transmissivity of different gel and cream products that contain drugs and special preparations. Ibuprofen and steroid are used in US gels by some therapists on the basis that the US energy will enhance the drug delivery to the tissues (phonophoresis). We are not looking (at this stage) at the phonophoresis effect per se, but we are looking to see if the incorporation of 'active ingredients' into the gel makes any measurable difference to the amount of US that would reach the tissues. The reason for mentioning it is that we are trying to make sure that we test the most widely used gels and creams (no point doing the work on gels that nobody uses), and therefore would welcome feedback with regards which of these you do use. We can't promise to test everything out there, but will do our best to make sure that the most popular ones do get on the list. You can either e mail me (t.watson@herts.ac.uk) or Amy Todd (a.todd@herts.ac.uk) who is running the lab work.

Keep you eye on the web site (if you are inclined to do so). There are new additions and updates going up there as often as I can get the time to write them. Recent additions include some extra material on US and fractures, the outcome of the US for Apomorphine nodule trial and some extra material on Shockwave therapy (still writing some more on that one). I still have a list of pages that I need to write (currently out of date or empty pages) but suggestions welcomed with regards material that you would find useful.

Contents

TENS and Hypoalgesia

The first paper in this issue is a systematic review considering the effect of pulse frequency on experimental pain in human subjects (***Chen, C. et al. (2008). Does the pulse frequency of transcutaneous electrical nerve stimulation (TENS) influence hypoalgesia? A systematic review of studies using experimental pain and healthy human participants. Physiotherapy 94(1): 11-20.)***

There remains a considerable variability in the opinion of researchers and practitioners with regards the importance of pulse frequency and the associated pain relief effects. The authors of this paper, well known in the field, have undertaken a serious review of the published literature (up to 2006). From the available literature, only the studies in which pulse frequency was the only manipulated variable were included (in other words, all other variables needed to remain constant) and 13 such studies were identified. Of these, 10 identified that there was no significant difference between the applied frequencies and 3 demonstrated a difference. Of the 3 where a difference was identified, one showed that 100pps (pulses per sec) was superior to 10pps and 1 showed that a lower frequency was more effective (4pps better than 100pps) and one mixed result (5pps and 80pps both better than 2pps). The authors comment that some of the studies were underpowered (usually due to small sample size) and thus false negative outcomes may have resulted.

There are many comments about this work, and what it might mean in clinical practice. Importantly, the authors were only looking at 'head to head' type trials – and in that sense, quite rightly. Comparing the effectiveness of a stim frequency against a control or placebo condition simply tells you whether the therapy 'works'. Comparing one of these trials against another which utilised a different frequency may not be the safest comparison to make. All the included trials in this work employed an experimental pain model, and the primary outcomes needed to be a change in pain threshold or pain intensity. There are obvious differences between an experimental pain model (where asymptomatic subjects are tested for their response) versus a clinical pain – much more variable, much less easy to control (in research terms) but much more 'real world'. We all know about the variability of clinical pain behaviour, and the authors allude to such issues in the discussion, and furthermore, some critical experimental and research methodology issues are raised and considered. Given that the literature considered dropped from over 2000 papers in the initial search, down to 20 once the criteria had been applied and then down to 13 once the details was considered says something about TENS research and quality. Sample size is a BIG issue here, and some very interesting points are raised about how big a sample would need to be to stand much chance of finding a real difference between groups – much larger than is routinely employed in most of these published reports. The other big issue for me is that due to the nature of the experimental model, it is almost impossible to apply the TENS electrodes close to, or at that site of the pain (clinically applicable) and therefore, not only is the pain experimental rather than clinical, the 'treatment' does not really reflect normal clinical practice. This is not an attempt by me to try and excuse the non significant findings of this review – it is all good stuff. One has to be wary of making an automatic leap from here to the clinical world and assume that pulse frequency is not an issue – indeed, it might not be, but on the other hand, it just might . . . . .

For anybody involved in pain research using electrical stimulation, this is an essential read. For anybody working with TENS clinically, I would strongly encourage you to give it a good look. It is well written, comprehensive and the discussion raises some excellent points which are worth further consideration

Implanted stimulation system and walking speed

The next 2 papers both relate to the use of electrical stimulation systems which employ implanted electrodes – this one on walking speed. It is a report of a randomised controlled trial with patients post stroke carried out in the Netherlands (***Kottink, A. et al. (2007). A randomised controlled trial of an implantable 2-channel peroneal nerve stimulator on walking speed and activity in poststroke hemiplegia. Arch Phys Med Rehabil 88(8): 971-8)***

Whilst I appreciate that implantable electrode systems are for the specialist centre rather than for the regular neuro rehab unit or out patient practice/department, there have been some very significant developments in this field in recent years and they certainly deserve a consideration.

The RCT based trial involved 29 (chronic) post stroke patients, one group of whom (n=15) continued with their normal walking aid and/or AFO and one group (n=14) had the implanted peroneal nerve stimulator. The recruitment was from a mixture of adverts in the local paper and via the standard medical route. The key outcomes were the 6 minute walk test, walking speed. Some patients were also assessed for their levels of physical activity. Details of the assessments and their intervals (basically baseline and then 4, 8, 12 and 26 weeks) are provided in the paper.

The stimulation system employs small electrodes attached around the peroneal nerve with a small receiver just under the skin. The stimulator is worn on a strap over the implant and a heel switch is used to provide stimulation timing. The stimulation was applied at 30Hz and used an asymmetric biphasic zero net CD waveform, with one set of electrodes going to each of the superficial and deep peroneal branches.

There were no significant differences between the groups at baseline (though the time since stroke was longer in the intervention group). Some patients in each group used other devices (walking aids and AFO's) which are detailed as are the n=4 drop outs. The results show that for the 6 minute walk test (assessed at baseline, 12 and 26 weeks), there was a significant difference between the groups, with the stimulation group continuing to show improvement at 26 weeks whereas the control group was demonstrating some loss of improvement by this time point. The walking speed also showed an improvement over time, and it is interesting that when the intervention group were assessed without their stimulator switched on, there was no significant difference between the groups. When activated, the stimulation group showed a clear improvement. The fact that there was no significant difference between the groups, even at 26 weeks, when the stimulator was not activated would imply that there is little or no carry over effect (though other trials I have heard of recently do seem to have identified a carry over effect). The activity measures did not show anything remarkable, and I am not entirely sure how many of the patients had this measure taken as there were a limited number of measurement systems available (n=4) – the results are presented, but not (as far as I could see) how many patients actually were involved in this part of the work (unless I just missed it).

The recruited patients were all 'chronic' stroke patients and therefore not on the road to recovery. Any changes demonstrated would be 'useful' as none would be clinically expected in this group. The 23% improvement in walking speed in the treatment group is impressive, and the authors include a good discussion and also a section on clinical relevance. There are an increasing number of implantable systems becoming available for 'chronic' post stroke patients, and one would suspect that it will become more 'normal' as an intervention as time goes by, the technology improves and relative costs decrease. The FES unit at Odstock Hospital in the UK have recently reported some impressive results in this area (www.odstockmedical.com) and I am sure that there are other groups around who are doing similar work (I know of a couple in the USA at least).

Implanted stimulation system and foot drop

The second study (***Kottink, A. et al. (2008). Therapeutic effect of an implantable peroneal nerve stimulator in subjects with chronic stroke and footdrop: a randomized controlled trial. Phys Ther 88(4): 437-48)*** is from the same research group and from what I can see, involves the same 29 patients in the same trial. Given that this is the case, I'll not go through all the same material again, but in this paper, the primary outcome measure related to changes in recorded EMG in the tibialis anterior, peroneus longus, gastrocnemius and soleus muscles. I mentioned in the previous review that there did not appear to be any carry over effect of the stimulation (when the patients in the stim group were assessed for walking speed, but with their stimulator not turned on). This study specifically reports the EMG changes in both groups over the same 26 week period and showed that there was a significant increase in tibialis anterior EMG (RMS max) after stimulation. Some changes were also demonstrated in the gastrocnemius muscle on testing. Although the results of the main trial had not showed an improvement in walking speed with the stimulator turned off, this report does suggest that there are some carry over effects and the story may not be as simple as would be implied from the report in Archives Phys Med. Worth a detailed read if you are into FES or muscle plasticity research.

Iontophoresis and drug absorption

I don't often include papers on iontophoresis, but there have been a couple over the last few issues, so here is another one for those of you with an interest in this area. A recent paper from the States (G***urney, A. B. and D. C. Wascher (2008). Absorption of Dexamethasone Sodium Phosphate in Human Connective Tissue Using Iontophoresis. Am J Sports Med 36(4); 753-759)*** looks at some key issues related to absorption of dexamethasone.

It was a human subject study (which is nicer than a lab animal model in this context) involving 31 patients all of whom were undergoing ACL reconstruction, divided into a treatment (n=16), a sham (n=13) and a control group (n=2). The idea of iontophoresis (for those of you not especially familiar with the treatment) is to use an electric current to enhance the absorption of a drug into the tissues, using a direct current, and placing the drug under an electrode of the same charge, and thus using the principle of like charges repel in order to promote drug transport. There is a bit more to it than that, but if you want to delve further, the standard electrotherapy texts will have a more comprehensive summary. The web pages on the www.electrotherapy.org site are currently (sorry about the pun) empty on this subject BUT I am trying to put together a review of the current state of the art (for those who have been asking!). The recent (2006) systematic review by Hamann et al (Phys Ther Rev 11;190-194) might be a useful place to look in the meantime.

OK, back to this trial . . . . the aim was to measure the amount of the drug – dexamethasone – a synthetic steroid and therefore not normally found in the tissues at all – which reached the dense collagenous tissues following an iontophoresis session. Previous measurements (not always well designed research one might add) has looked at drug transport through the skin and in some cases into the superficial soft tissues and muscle, but there is a paucity of work which has evaluated drug transport through to the dense, deeper tissues – which is what was intended here.

The ACL reconstruction patients were all undergoing surgery in which an autograft (semitendinosis/gracilis tendon) was going to be employed for the graft material. Immediately prior to surgery, an iontophoresis session was delivered (either real or sham – the patient was blind as to which) using a standardises treatment (detailed in the paper) using 40 mA/min treatment with 0.4% dexamethasone over the ST tendon. With this type of treatment, the real treatment group had the current turned up to tolerance (between 2 and 4 mA) and thus the treatment lasted for between 10 and 20 minutes. The sham group were told that they may or may not feel the current (the machine was not turned on) and therefore any dextamethasone which reached the tissues did so purely by 'passive' absorption through the skin. The two control subjects were not exposed to either the iontophoresis or the drug on the skin.

Surgery was undertaken in the 2 hours following the session, and at the operative event, a tissue sample was taken from the fascial band associated with the ST tendon and used for analysis. The surgeon was blinded as to group allocation, and no dexamethasone was used in any part of the surgery or anaesthesia (quite a neat model I reckon). The amount of the dexamethasone in the tissue sample was determined (details provided) with what appears to be a pretty sensitive method.

The results (in summary) showed that the groups were equivalent pre treatment, and that although skin thickness varied, it was not a determinant of the dexamethasone concentrations achieved. There was a significant difference in the dexa concentrations between the treatment and sham groups with 8 of the 16 Rx group samples had a measurable concentration of the drug whilst only 1 of the 13 sham group met the same criteria. The treatment group had almost 15 times more dexamethasone in the tissue sample than the sham group (on average). The two control samples did not show any measurable concentration (as one might expect – it does not occur naturally – but worth checking anyway).

These findings make a useful contribution to the literature in this field, but one of the important issues is whether the amount of the drug in the tissues is sufficient to be of clinical importance. The authors discuss this in some detail, comparing their results with those obtained from animal, lab and other clinical studies. The amount of the drug needed in the tissues to have a known effect is not well established, but it is estimated that dexamethasone is some 25-30 times more 'powerful' than hydrocortisone, and thus relatively low concentrations could be clinically effective. The tissue concentrations found in this study were approaching those thought to be effective, and the authors provide some interesting points in their discussion. They also usefully discuss the issue surrounding the responders (the 8 subjects in the treatment group who did have measurable levels of the drug) and the 8 non responders in the treatment group. I'll not repeat the points here, but makes for an interesting read I reckon.

All in all, this experimentation goes a lot further than pervious work in trying to identify whether the iontophoresis actually does what it claims to achieve, to what extent, and also considers why some people seem to respond and others not (a critical issue in many areas of electrotherapy).

Laser Therapy and Achilles Tendinopathy (x2)

I was going to include on paper on Laser for Achilles problems, then as I was tapping away, another reached the desk, so you are getting two of them! The first one is is from a group in New Zealand, where David Baxter, a well established researcher in this field is now located, and is on the author list. Tumilty et al (***Tumilty, S. et al. (2008). Laser therapy in the treatment of achilles tendinopathy: a pilot study. Photomed Laser***

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

Surg 26(1): 25-30) report the outcome of a pilot study using a combination of eccentric exercise plus either real or sham laser therapy employing a double blinded RCT design. There were 20 patients involved in the trial (10 + 10). The exercise programme was standardised (12 weeks) and all got laser (real or sham) 3 x a week for 4 weeks during that period. The laser dose totalled 18J per session delivered at 810nm wavelength @ 100mW at 6 set points over the TA for 30 sec a point giving 3J per point. The patients were recruited by local paper advert, all had a chronic Achilles tendinopathy and none had been treated within the last 3 months. The main purpose of the study was to evaluate the effect size of the intervention and therefore would enable a power calculation to inform how many people would be needed for the full RCT. The outcome measures were taken at baseline, at the end of the 12 laser sessions (4 weeks) and at the end of the 12 week follow up period. The measures included the VISA-A questionnaire (specifically developed for TA function problems), a VAS pain score and a standardised isokinetic strength test for plantarflexion.

The results demonstrate that there was no significant difference between the groups at baseline, and there were significant changes with time (i.e. baseline to 4 weeks to 12 weeks) for both groups. There were no significant differences between the groups, and the variability of responses were considerable (something that is common in these trials). The fact that there were not significant differences between the groups is neither surprising nor disappointing in that the main purpose of the study was to look at the treatment effect size and thus enable a power calculation to inform the group size necessary for a full blown RCT, which turns out to be 20 (i.e. twice as many as were actually recruited). There are some useful discussion points raised about statistical and clinical significant differences in relation to power calculations, and well worth a read for those concerned with RCT design or evaluation. There are also some informed discussion regarding the use of ANCOVA statistics using baseline as a co-variate, and why this is a preferred analytical tool – again, well worth a read through. One assumes that the full RCT is planned and I look forward to the results. Whether laser therapy should or should not be included as a component of the management of Achilles tendinopathy management has not been resolved by this work, but it is important to note that both groups made significant improvement over the trial period. It may well be that the addition of laser therapy to the eccentric training programme makes not clinically significant difference, but that is yet to be confirmed. Watch this space . . . . . .

The second paper is so hot off the press that the papers are still warm from the printer (not quite, but you know what I mean!) and looks at almost the same issue. ***Stergioulas, A. et al. (2008). Effects of Low-Level Laser Therapy and Eccentric Exercises in the Treatment of Recreational Athletes With Chronic Achilles Tendinopathy. Am J Sports Med 36(5): 881-887.*** The author list includes Bjordal (whose name, like Baxter) is well known in this field, and is somebody who gets a mention in more issues of the Electrotherapy News than not!

The study was also a blinded RCT design using a treatment and a sham treatment group (20 in each) and the basic idea was to see if the addition of a laser therapy intervention to an eccentric programme made for a more rapid recovery – not dissimilar at all to the previous study except that a different raft of outcomes were used – which in many ways is a shame as the numbers in this trial are what the Tumilty papers identified as being necessary to establish a clinical effect – hey ho – they were clearly happening at the same time and that is the way it goes.

The 52 patients recruited for this study were all recreational athletes, divided at random into 2 groups – both with eccentric exercise programme plus either real or sham laser (blinded) over an 8 week intervention period with 12 sessions. The treatment was similar (but not the same) as the previous study, using almost the same wavelength (820nm instead of 810), but overall a lower dose being applied (same 6 point treatment protocol, but with a total dose of 5.4J per session – was 18J per session in the other one). The power output of the device was 30mW and the power per point was 0.9J (Interestingly, I am not aware that anybody has identified what the really critical parameters are for laser therapy – is it the power, power density, energy, energy density, peak or mean power, delivery method etc etc – absolutely would love to know – many people claim to know what is the critical component, but needs a serious review). The treatments were twice weekly for the first 4 weeks and then once weekly for the next 4 weeks.

Outcomes were taken (by a blinded assessor) at baseline, at 4 weeks (mid Rx), 8 weeks (end of Rx) and then at the 12 week follow up. Pain intensity on activity was the primary outcome (VAS) with morning stiffness, tenderness and ankle range of motion as secondary outcomes. I might have some quibbles here as the method employed for the outcomes is critical, especially the ankle goniometry which is prone to some significant errors depending on how it is done (not reported here). The margin of error – which must exist – needs to be smaller than the observed changes before any of the results are meaningful. If the error level for the goniometry for example is plus or minus 5 degrees, and 'statistically significant' difference of 3 or 4 degrees is seen in the results, it could all be error – no way of telling. The reporting of the accuracy/sensitivity of outcomes is critical, but often omitted I am afraid to say.

The results : of the 52 patients recruited, 12 of them (6 in each group) dropped out at 4 weeks (due to lack of effect), leaving 40 patients to complete the full intervention. The CONSORT flow chart illustrates the patient recruitment and progress through the trial (like to see it). The statistics included ANCOVA (see previous trial) and was based on an intention to treat analysis (again, nicely done). There was a significant difference for the primary outcome (pain) between the groups, and the real laser group had results at 4 weeks that were comparable to the sham group at 12 weeks (suggesting that they got better faster). The secondary outcomes were also significantly better for the treatment group compared with the sham group at 4, 8 and 12 weeks.

Although the therapist delivering the treatment was not blinded, the patients were and so was the assessor of the outcomes. The authors also suggest that they could have used the VISA-A questionnaire (used by Tumilty) but the study was carried out in Greece, and no translated version was available. The authors reason why their outcomes include numerous aspects of the VISA-A questions, and suggest that the results can be considered in that light (see the discussion for a nice reflection on some of these points). The limited follow up (restricted to 12 weeks) also gets a mention, and it is suggested (from the literature) that some 70% of the benefit of eccentric exercise is achieved by the 12 week point, and that although a longer follow up would have been nice, it was not practical in this study. Point taken, but a 6 month or 1 year follow up would have provided some very useful extra data I would reckon.

The outcome of this work suggests that the addition of laser therapy (at this dose) to an eccentric exercise programme has significantly beneficial effects, and the authors note that even though there is stronger evidence for the use of laser in chronic tendinopathy, it is often not employed in routine clinical practice – one of the mysteries of the apparent use of evidence based practice – the evidence appears to be used selectively depending on the beliefs (or not) of the therapist. Some issues about the outcomes which I would like to know the answer to (just being nosey), but overall, a useful study and the results certainly contribute to our knowledge in the field.

EM Fields and Fracture Healing

There are three papers on different aspects of EM fields in this issue, and the first one is a report of an RCT using capacitively coupled electric fields for fracture healing (***Beck, B. R. et al. (2008). Do capacitively coupled electric fields accelerate tibial stress fracture healing? A randomized controlled trial. Am J Sports Med 36(3): 545-53)***

The use of these fields for this purpose is not new, and there have been many many reports over the years, my own collection goes back to the early 60's, though I suspect that it was not new even then – must find out one day! This particular study, from a group in Australia and the USA (the work was carried out in the States) was looking at the effectiveness of the therapy in tibial stress fractures in 44 people who were randomly assigned to real of placebo treatment groups. The numbers recruited were based on a power calculation, aiming to see if a 3 week difference in time to healing occurred. The final power of this study was over 95%, and a CONSORT flow diagram is included to enable the reader to follow the cohorts through the research process.

The placebo treated group were provided with an identical machine to the real treatment group other than the fact that it did not provide an output. The patient and the assessors were blinded to group allocation. The device was a 'bone growth stimulator' which delivered a sinusoidal wave output to 2 self adhesive (gel) pad electrodes at 60kHz and at 3-6V with a current flow of 5-10 mA (not sure if that was measured from the patient or the specifications of the machine) which was used for 15 hours a day and standard stress fracture 'advice' was also provided to all subjects. Limited exercise programmes (limited WB) were allowed but subjects were asked not to take NSAID's (for very obvious reasons!). A diary and compliance log was maintained throughout. In addition to the clinical assessment, imaging systems were also employed, and as the situation improved (monitored via telephone interviews), walking without pain, then running then hopping were allowed as progressive activity (details in the paper). When the subject could hop without pain (30 seconds) and confirmed, the treatment stopped and a follow up MRI was undertaken.

Results : Tae primary analysis was to compare the time to healing between the two groups and to compare the responses of men and women in the trial (using a 2 way ANOVA) with some additional sub-analyses also being used to try and refine the responses identified and confounding variables (detailed in the paper). Of the 50 patients recruited, 44 completed the study (20 M 24F) and the random allocation of treatment units meant that there were 23 active and 21 placebo devices between them. There were generally no significant differences between the groups at baseline, though some differences in lean body mass and percentage body fat were noted. There was no sig difference in injury severity or compliance between the groups or between sexes.

There were no significant differences between the treatment groups, though there was an identified slower healing in women compared with the men. On one of the sub group analyses, it was seen that there was a relationship between machine use compliance and healing rate (in the treatment, but not for the placebo group) – those that used the machine for more hours a day got better faster. Taking the 70% use as the cut off (i.e. those that used the device more than 12 hours or so a day) healed significantly faster than those who used it less than this.

In another split file analysis, it was seen that those with a real treatment device, and a more severe injury healed on average some 24 days faster than those with a placebo device, but as the authors note, the split file analysis, although interesting, provides under powered results and one therefore has to be careful to jump to conclusions, however tempting that might be!! This would need to be retested with a larger sample and specific fracture severity sub groups to make a firm conclusion.

I think that although the overall difference between treatment groups was not significant, there looks like a real relationship between compliance (hours a day used), fracture severity and fracture healing. The applied fields appear to work best in the patients who DO use the machine (as one might expect – or hope for) and that the more severe the fracture at the start, the more effect the treatment had. These specific finding would need further investigation to establish whether this is 'true' or a fluke finding, and the under powered sub group analysis does not enable this conclusion to be reached with any degree of certainty – which is a shame – but that is what happens in real world research! For those of you dealing with stress fractures, athletes or any associated field, I would suggest that there is a lot of excellent material included in the main paper that I have not included her, and a read through the original is strongly commended.

EM Fields and Headache management

The second paper on EM fields is a review (literature rather than systematic) of the use of Pulsed Electromagnetic Fields in the treatment of Headache by three American authors (***Wilson, V. et al. (2007). Headache Treatment with Pulsing Electromagnetic Fields: A Literature Review. Applied Psychophysiology and Biofeedback 32(3-4): 191-207.)***

This is a substantial review (16 journal pages) and it would be both daft and inappropriate for me to try and reproduce the whole thing here (let alone the copyright issues), but there are some fascinating elements included, though the final conclusion, almost inevitably was that further research is needed!

The authors consider some of the critical issues concerned with mechanism of action, noting the although several studies have identified clinical benefit, the exact mechanism of action has yet to be fully elucidated. Some interesting potential mechanisms are considered including neurophysiological, neurochemical and cardiovascular.

Nine studies are detailed and also presented in a useful tabular format, and there are 4 further pages (table) of various 'other' uses for PEMF's and then 3 pages of references. If nothing else then, this provides a comprehensive introduction to some of the key literature in the field, though is by no means complete as a quick scan through my own database shown some almost 4000 papers on this and related subjects! The main downfall of many of the studies reported are lack of control groups, failure to compare with 'standard' treatments and small samples (amongst other issues). The overall results are 'encouraging' rather than not identified, and the genuine conclusion that further studies are needed illustrates the commonly identified problem of research not always being especially well designed or the outcome measures not being especially useful in terms of validity, reliability, sensitivity etc (my comment, not theirs!)

The paper makes for an excellent read, not only for those concerned with using PEMF's for headache treatment, but for anybody wanting to consider a range of published literature in the PEMF area with a view to investigating further.

EM Fields and Wound Healing

The last of the three papers looking at electromagnetic fields reports an experimental study using a rat model from a research group in Greece (***Athanasiou, A. et al. (2007). The effect of pulsed electromagnetic fields on secondary skin wound healing: an experimental study. Bioelectromagnetics 28(5): 362-8.)***

There have been several reviews of different forms of 'electrotherapy' used for chronic wounds, including an updated version of electrical stimulation for this effect in the new edition of Electrotherapy : Evidence Based Practice. When you look through the mass of literature, there is a growing body of evidence that several different forms of intervention can be effective, including ultrasound, laser, pulsed shortwave and electrical stimulation in numerous guises (not to mention acupuncture, electroacupuncture, manual therapy, exercise and mechanical / pressure therapies). In fact a full review of this range of published studies would be very welcome, but would be a massive undertaking – quite fancy the idea myself – just need some time.

Anyway, this animal model used 24 rats in each of two groups in whom full thickness wounds were generated. The treatment group were exposed to 20 minutes a day of a Pulsed Electromagnetic Field (PEMF) whilst immobilised. The control group were similarly immobilised but not exposed to the energy. The PEMF was applied whilst the animals were restrained in a wooden cage, and the full specifications of the device were provided in the paper, but essentially, the field applied as at 12.5mT with a 35-80J/pulse and a 1 microsecond wave duration. The control animals were places in the same cage system, but the unit was not switched on.

Rats were sacrificed on a serial basis, with 4 animals from each group on each of days 3, 6, 9, 12, 18 and 22. The main outcomes were wound healing rate (using photography, wound tracing and planimeter) and then tissue samples were harvested. The wounds were expected to heal during the course of the work, and the repair stage achieved at each sample point were compared between groups. The wounds were not dressed and no antibiotics were used.

The results (abbreviated) shed that wound size was significantly smaller in the treatment group on days 3, 6 and 9, and smaller throughout all time points in the treatment group, though not reaching statistical significance. The histological findings are described fully in the paper, but they can be reasonably summarised in that the treatment group were histologically several days ahead of the control group, and by day 18, the experimental group wounds were healed and the control group wounds did not achieve this stage until day 22 – something like a 4 day advantage (though this is a bit of a simplistic analysis). The main effects of the PEMF as applied in this work appears to be in the first 9 days or so, and needless to say, the authors have identified that further work is necessary to evaluate dose related issues.

Shockwave Therapy

I am getting an increasing number of e mails every week about shockwave therapy (in its various guises) and have been working on the web pages to try and make some sense of the literature. There are lots of publications (I have almost 2000 so far), but the key issues are up on the web and also a downloadable list of recent references which might be of value for those investigating this area further – seems like there are a lot of you! There are 4 shockwave papers in this issue (though there are also several mentions and reviews in earlier editions if you want to look through the back issues – available from the web pages). The main clinical areas where the therapy is used, apparently with measurable benefit, is with the chronic tendon and ligament related problems (like plantar fascitis, tennis elbow, supraspinatus etc). The selection below pretty much reflects that emphasis with the exception of one paper on bone necrosis – just to be different.

Shockwave therapy and Achilles Tendinopathy

The first paper is concerned with shockwave for Achilles tendinopathy and the author has published in this area several times in the last few years (***Furia, J. P. (2008). "High-energy extracorporeal shock wave therapy as a treatment for chronic noninsertional Achilles tendinopathy." Am J Sports Med 36(3): 502-8).***
Shock wave therapy basically comes in two different kinds (simplistic division) – HIGH and LOW – and as the title suggests, this was a high energy application. With these treatments, the procedure is usually a 'one off' rather than a series of treatments and a local anaesthesia is used as it is a painful experience without it! This was a study involving a total of 68 patients with chronic Achilles tendinopathy, divided equally between a treatment and a control group, though it is described as a case control study rather than an RCT. The treatment group were given 3000 shocks with a total energy flux of 604mJ/mm2. The control group were offered non shockwave treatment, and although there were no significant age or problem duration differences between the treatment and control groups, there was one fundamental difference in that an unidentified number of the control group were not able to receive the shockwave treatment as it was declined by their health insurance company. All the patients were from the authors practice and were not (so far as I can see) randomised into the two groups.

The treatment was delivered with a shockwave device that operates with an electromagnetic coil generator. Details of the machine and the treatment procedure are described in the original paper for those with an interest in such things. The outcome measures employed were the VAS for pain and a Roles and Maudsley score (4 point score related to activity) taken pre treatment and then at 1, 3 and 12 months post intervention. The analysis proposed has an associated power of 0.9 with a significance level of 0.05.

The results (summarised) show that the treatment group demonstrated a significant decrease in the VAS score, whilst the control group did not. The VAS score in the treatment group decreased at all measures time points going from an average 8.2 pre treatment to 2.2 at 12 months. The Roles and Maudsley scores for the treatment group improved significantly over the 12 month period, and the results were significantly better than those for the control group at all time points. Given that all patients had at least a 6 month history of problems at the baseline, these are pretty impressive results, and although not an RCT and there are some concerns with regards potential differences between the groups (not in terms of baseline score, but in terms of potential inequality re insurance) and also a non specific non-shockwave management protocol, there is a strong indication that a single high intensity shockwave treatment at these parameters does have a significant effect on both pain and functional outcome for patients with chronic non-insertional Achilles tendinopathy.

Shockwave and Heel Pain

This research report is from the Hong Kong stable, with Gladys Cheing as co author – another name that comes up with some regularity in the newsletter (***Chow, I. H. and G. L. Cheing (2007). Comparison of different energy densities of extracorporeal shock wave therapy (ESWT) for the management of chronic heel pain. Clin Rehabil 21(2): 131-41).***

This gives some insight into dose issues when using the lower energy type of shockwave (whereas the previous paper was looking at high dose, single intervention approach. A group (n=57 to start with, but 8 dropped out) of patients with chronic heel pain were allocated to one of three groups, based on treatment dose. The authors suggest that in their classification, low energy is less than 0.1mJ/mm2, medium density lies between 0.1 and 0.2 mJ/mm2 and high density are doses greater than 0.2. The authors also note that in previous studies for ESWT for heel pain, which had demonstrated significant benefit, the dose delivered was variable, and in some cases, inadequately reported. The aim of this study was to compare a 'fixed' dose with a 'maximum tolerable' dose with a control (which was not actually a control with no intervention, but a dose that was not anticipated to be effective.

The 57 recruited patients were allocated equally and randomly between the 3 groups (n=19 each). After the drop outs were accounted for (flow diagram included) the fixed group were left with 17 at follow up, the maximum dose group with 18 and the control group at 14. All patients had a history of unilateral heel pain for at least 3 months. The therapy was delivered to the most tender point on the heel according to the protocol for their group summarised thus : The Fixed group were started at 0.05mJ/mm2 and the dose was progressively increased up to tolerance level which was noted. All subsequent treatments were delivered at this dose. The maximum tolerance group were also started at the same dose, which was increased to max tolerance every 200 impulses. For both of these groups, 1000 impulses were delivered at 3Hz (previously identified as being effective). The control group were treated with a minimal dose which was 0.03mJ/mm2 for 30 impulses total, repeated at each session. All treatment sessions were delivered weekly for 3 weeks and patients were assessed again at a further 3 week follow up (6 weeks from baseline).

The outcomes included heel pain on palpation and on tension (both using VAS), max walking/standing duration and a Foot Function Index score (all procedures detailed in the paper). Change was normalised against the baseline measure, and a % change recorded (though this has been criticised as an analysis by some statistical authorities).

The results showed no significant difference at baseline. The VAS scores reduced significantly over the 3 week treatment period and the 3 further weeks follow up. The control group scores did not change significantly. The max tolerance group scores reduced more rapidly than the fixed dose group scores (fully detailed in the paper). Similarly, the pain on tension scores reduced significantly, with the max tolerance group doing better. The Foot Function Index scores changed significantly in the treatment groups, but not the control group, with the max tolerance groups again doing better in all aspects of the scale. As one might anticipate by this stage of reading, the same pattern of results were seen when the walking and standing duration data was analysed.

So, the overall result was that both 'treatment' groups made significant improvement, but that the maximum tolerance group did better on all counts than the fixed dose group. The follow up to this study was relatively short (3 weeks after the end of treatment) and it would have been better if a longer follow through had been possible, but never the less, these results are clearly significant, and although both 'doses' were shown to be effective, there was a marked difference between them in favour of the maximum dose on this test protocol. The full paper is well worth a read in that there are some subtleties and complexities to the results that I have skimmed over to some extent (I have to leave you something to do!) and there are also some interesting points raised in the discussion. For anybody using shockwave therapy for heel pain, this is a 'must', and for anybody trying to read around the subject to get to grips with it, it should be on your list.

Shockwave and Bone Necrosis

This is a lab based animal (rabbit) study and I'll not go through the detail as I would for the clinical studies, but it combines some good evidence about the mechanism of action for shockwave and also ties in with other work that I have reported over the last couple of years on VEGF.

The research team from China set out to evaluate the effect of shockwave therapy on femoral head avascular necrosis (***Ma, H. et al. (2007). Upregulation of VEGF in subchondral bone of necrotic femoral heads in rabbits with use of extracorporeal shock waves. Calcif Tissue Int 81(2): 124-31)*** in which, it is suggested that shockwave has been previously demonstrated to be effective. This being the case, they were trying to identify the effect of the therapy on VEGF expression, given that this mediator is well known as a stimulant of tissue angiogenesis during repair (why I have mentioned it numerous times before), and might therefore go someway to explain how the therapy might achieve its effect.

In short, 30 rabbits had a femoral head avascular necrosis induced (detailed in the paper) bilaterally. One of the hips was subsequently treated with shockwave and the other remained as a control. The shockwave was a single application at a dose previously demonstrated to be effective for this condition (2000 impulses, 0.26mJ/mm2). Some animals were sacrificed at each time point (1, 2, 4, 8 and 12 weeks after the therapy) and femoral head tissue samples taken for evaluation together with VEGF assays (all procedures detailed in the original paper).

The results demonstrated that there was a significant upregulation of VEGF expression in the treated femoral head, reaching a peak between 2 and 4 weeks post treatment, and the on some tests, remaining high at 12 weeks. In terms of the histology, there were strong changes observed in the treated femoral heads, especially in the proliferation zone surrounding the necrosis. The MVD (microvessel density – a measure of angiogenesis) showed significant advantage in the treated bone compared with the controls from week 4 onwards.

There are other findings, though some are more complex than I would normally present here – I am sure that those of you who are interested can go to the original and have a look see. At the end of the day, the treated femoral heads showed marked and significant changes compared with the controls, and interestingly, it would appear that a single session of shockwave brings about an upregulation of VEGF expression and this is (in part at least) responsible for the angiogenic changes observed in the bone. Both angiogenesis and VEGF have been attracting a deal of research publication volume over the last few years, and there is a strong potential for different therapies to involve one or other mechanism. This study helps to elucidate further the role of single session shockwave in this process.

Shockwave and Plantar Fascia Pain

The last of the shockwave papers in this issue takes us back to the foot again, this time looking at plantar fascia (one of the most prolific sources of shockwave papers if you have had a look at the research publications list on the web pages). This group from Taiwan were looking at the previously reported thinning of the plantar fascia following shockwave therapy and its relations to pain reduction. This was a clinical study involving 53 patients (who presented 78 affected feet between them) treated for 3 sessions a week apart at a higher (n=25) or lower dose (n=28). Each session consisted of 2000 impulses at 0.12 or 0.56mJ/mm2 using a piezoelectric type device, and no anaesthetic was employed for the session. The outcomes included pain (VAS), Foot Function Index, SF36 and US measured thickness of the plantar fascia, with follow up at 3 and 6 months.

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

There were no significant differences in presenting features between the two groups at baseline, though there were 10 patients with bilateral pain in the low intensity group and 15 in the high intensity, and although not a significant difference, might be worthy of further mention in my book. All patients recruited had at least a 6 month history and any who had been treated with steroids previously had at least a 3 month period since the last injection. The group allocation was randomised.

The results showed pain reduction in both groups (bit more so in the high dose group) and the reduction was significant. At the 3 and 6 month follow ups the overall success rates were 60 and 63%, and although there were some variations in these overall results for the low and high dose groups, they do not appear to be significantly different from each other. The fascial thickness was normalised in a significant number of patients. The factors that appear to have been associated with the better results at follow up included being in the high intensity group, exercising regularly and thinner plantar fascia thickness (all significant). The authors present a useful discussion relating to the relationships between plantar fascial thickness, shockwave treatment and outcomes of treatment. There was no control or placebo group in this trial which is a shame as it potentially weakens the outcomes to some extent, and furthermore the apparent associations between shockwave dose, thickness of the fascia and improvement in pain and function are not proven even though the relationship were significant – you can't determine cause-effect using a correlation type analysis. The work does however highlight some issues not previously identified and is well worth a read if this is your area, or, as previously, you are trying to get to grips with what shockwave can and can not achieve clinically.

Vibration Therapy :

Although the new and somewhat trendy vibration based therapies are not what some people would call electrotherapy, the move away from that terminology towards to broader and more encompassing 'Electro Physical Agents' quite comfortably includes this genre, and thus I have included ***brief*** reviews of 5 papers that might whet your appetite if you have not had much to do with this form of therapy before. There are plenty more where these came from!

Vibration therapy and Muscle Strength

The first paper is from Physical Therapy earlier this year but comes from some work done by a group in Sydney, Australia (***Rees, S. S. et al. (2008). Effects of whole-body vibration exercise on lower-extremity muscle strength and power in an older population: a randomized clinical trial. Phys Ther 88(4): 462-70).*** As the title suggests, this work set out to evaluate the effect of vibration training in an older, but healthy population with a primary objective of looking at its effect on muscle strength, using an RCT design, and comparing a vibration group with an exercise only group.

I need to keep these reports brief, so you will have to go to the original to get much of the detail (at least Physical Therapy is one of the easiest journals to get access to). OK, so 30 people were allocated (randomly) to the vibration training or the exercise only group. The basic idea of vibration as a therapy is that it increases motor unit activity by reflex induced muscle contractions, and is incorporated with 'normal' body weight exercise (in this case squatting – a multi joint, weight bearing exercise which is potentially valuable for the population (mean age of just over 70 years).

The strength and power of the hip, knee and ankle flexors and extensors were considered, and the sample size was predetermined in order to achieve a power of 0.8 with an alpha of 0.05. Essentially both groups carried out the same exercise programme, on a platform, but for one group it was vibrating and not for the other, over an 8 week period, attending 3 times weekly with at least a 1 day gap between sessions. Outcomes were taken at baseline and then at 4 and 8 weeks (middle and end of training phase). The vibration was set at 26Hz with a displacement of 5-8mm. The specific exercise programme is detailed in the paper and was progressed according to set criteria.

Muscle strength and power were assessed with a Cybex isokinetic dynamometer (plenty of details in the method section). There were no significant differences between the groups at the baseline, no left right or gender differences in responses, so data was effectively pooled for the purposes of the analysis.
The hip strength and power increased, but there were no significant differences within or between groups. For the knee, there were significant increases in strength and power in both groups, but they were not significantly different from each other. At the ankle, there was a significant difference in the plantarflexor strength and power, with the vibration group providing the advantageous results.

There are plenty more details to the results than I have alluded to here, and there is an excellent discussion, well worthy of a read. Effectively, there were no significant strength gains at the hip. There were significant gains at the knee, but both groups made comparable gains. It was at the ankle that the differences between the groups showed, with the vibration group gaining significantly over the exercise only group. This was a well constructed RCT design with careful methodology and a useful set of results to start us of with.

Vibration therapy and Balance

The second paper, from last year was based on work carried out by a group in Hong Kong (***Cheung, W. H. et al. (2007). High-frequency whole-body vibration improves balancing ability in elderly women. Arch Phys Med Rehabil 88(7): 852-7).*** This too was an RCT design, and unusually in therapy research, the treatment group were compared with a genuine no intervention group. The participants were all otherwise fit elderly women (69 of them all aged over 60) – please don't have a go at me about being 60 not constituting 'elderly' – I know – but I am just reporting! – I like the WHO definition that I came across once when teaching care of the elderly – that old age doesn't even start till beyond 80 years – but that is another story for another day!

Whole Body Vibration (I'll just call it vibration from here onwards) has been evaluated for its effect on balance in younger people and shown not to do a whole lot. In the elderly, it was hypothesised that it might have some significant benefit, and some previous work would support this contention. After screening and baseline assessment, subjects were randomised into two groups, one of which continued with their 'normal' sedentary lifestyle, and one of which followed the vibration programme (of course it would have been great if there was a third group who had exercised, but not with the vibration – at least the benefits of vibration vs the benefits of exercise would have been evaluated – but we all know what would be nice to do!

The vibration was delivered in a community centre. The vibration was set at 20Hz, 3 minutes a day, 3 days a week for 3 months (the intervention of 3's!!). The outcomes included a 'limits of stability' test and a functional reach test - both are described in sufficient detail in the paper. The allocation to control and intervention groups was (deliberately) not equal, and on completion there were 24 in the control and 45 in the intervention arms of the study. The results showed some significant changes in the intervention group in terms of aspects of their balance tested with the limits of stability assessment. There were improvements too in the functional reach test, but these did not achieve statistical significance.

There are several unanswered questions – not least of which I raised earlier – what about the benefits of the vibration versus the benefits of just doing some exercise? Clearly, this was not part of the aim of this work, but would make a very useful follow up. I am not knocking the demonstrated benefits of vibration – I would just to know how much better (or not) it is compared with just doing something – as opposed to nothing. That notwithstanding, this was a useful trial and provides another level of evidence in this rapidly expanding area.

Vibration therapy, Performance and Flexibility

We move swiftly from the elderly in Hong Kong to younger female athletes in Italy – not much of a leap eh? This is a 2006 paper, but included here as part of the introductory literature in this area. (***Fagnani, F. et al. (2006). The effects of a whole-body vibration program on muscle performance and flexibility in female athletes. Am J Phys Med Rehabil 85(12): 956-62).***

A group of young (21-27 years) female athletes were recruited and participated in an 8 week programme in either a vibration or a control group. The key outcomes were performance and flexibility (but you could guess that bit from the title!). The vibration was delivered 3 times a week and the outcomes were taken at baseline and at the end of the intervention period. The recruits were from different sports (detailed) and half of the recruits from each sport were randomised to the vibration and control groups. The vibration in this study was set at 35Hz with a 4mm displacement, and the exercises performed whilst on the platform are described. The outcome measures included a specific jump test, a dynamometer test for hip, knee and ankle flexors and extensors and a sit and reach flexibility test. There were some dropouts from the study and 13 completed the experimental arm and 11 the control arm. The groups were comparable (not significantly different) at baseline, but by 8 weeks, there were significant differences in the jump test (advantage to the vibration group) and similarly for the flexibility and isokinetic results.

Again, it is possible to be critical of the study (as one could about almost all studies) BUT the results do demonstrate significant advantage for the vibration group compared with the control group.

Vibration therapy : Neuromuscular performance

As if by design, we slide gracefully (ish) from young female athletes to a test of vibration in healthy young men, looking at neuromuscular and hormonal responses. The work, done by a group in the UK is reported in a journal that you might not obviously scan if you were on the hunt for whole body vibration research papers, but there it is! (***Erskine, J. et al. (2007). Neuromuscular and hormonal responses to a single session of whole body vibration exercise in healthy young men. Clin Physiol Funct Imaging 27(4): 242-8).***

This was a randomised cross over design with a small group (n=7) of healthy young men. There was a 2 week wash out period between the test sessions. Saliva was collected pre and post the intervention for tests of cortisol and testosterone (pre, immediately post, 1 hr, 2hr and 24 hr post intervention) and the MVC was recorded using the Biodex testing leg extension on a set protocol, pre, immediately post, 1hr, 2hr and 24hr post. The exercise element (60 sec intermittent isometric half squat – detailed in the paper) were performed under vibration (set at 30Hz, 4mm displacement) and control conditions following a warm up period on a cycle ergometer.

The results in brief showed that the MVC was significantly reduced after the vibration condition compared with the baseline data but returned to 'normal' by 24 hours, whereas the control condition did not show any significant changes. There were no gross differences in testosterone or cortisol concentrations by group or by time. There was a trend for salivary cortisol to increase over time under the vibration condition. These results did not confirm the hypotheses (that cortisol would decrease, testosterone would increase and greater MVC would result from the vibration intervention. There were no significant cortisol or testosterone changes and there was a significant change in MVC for the first 2 hours post intervention, but it was a decrease rather than the anticipated increase! There are some interesting discussion points raised (as you might expect) and in fact several other researchers have reported a decrease in muscle strength/function immediately following vibration exposure.

This was a small study, using a cross over design, but assuming that the wash out period was long enough (it certainly should have been), then the results might challenge some of the commonly held beliefs that the effects of vibration include a short term as well as a long term increase in muscular function. Food for thought and certainly room for further investigation using larger cohorts if at all possible!

Vibration therapy and Circulation effects

The last of these brief reports is from a 2007 paper from a New Zealand group (this subject seems to be attracting attention from a widespread geography) looking at the effects of the therapy on the peripheral circulation (***Button, C. et al. (2007). The effect of multidirectional mechanical vibration on peripheral circulation of humans. Clin Physiol Funct Imaging 27(4): 211-6).***

This was not a whole body vibration research protocol – but a local application, comparing a real and a placebo mechanical vibration device, a with a raft of physiological outcomes tested under crossover design conditions. Twenty healthy subjects were recruited (10M, 10F) aged between 40 and 65. The tests were carried out in a controlled (lab) environment on two separate occasions. The procedures a re detailed in the paper, and bearing in mind that these were supposed to be brief reports, I will keep the description to a minimum.

One each occasion, two vibratory devices were applied – one under the gluteal region (they sat on it) and one under the right foot. The devices looked and sounded the same, but for the placebo devices, the plate was disconnected from the driver – though of course, it is impossible to say that there was 'no' vibration (which the authors acknowledge). The devices operated at 60Hz and the exposure was for 30 minutes
The results showed that there were no significant changes in BP, heart rate and skin temperature between the treatment and placebo conditions. Two key measures of lower limb blood flow (MEAN and PEAK flow), measured with occlusion plethymography, were compared for time effects (over the course of the intervention) and group effects (comparing treatment with placebo). There was an increase in mean blood flow from baseline to post intervention which was significant, but there was no significant difference between the real and placebo conditions (26% for the real and 12% for the placebo). One of the problems encountered when doing work with blood flow is the substantial variability between the responses of individuals (TW comment, not the authors). You can get stunning responses in some individuals and just about nothing happening in others – or even the reverse effect. This is something that I have seen over several years of experimenting with blood flow changes under several different experimentations. When you look at the overall results, the average effect might be nothing of importance but within that, there are responders and non responders – something we have just seen in the apomorphine/ultrasound trial. The authors of this paper have addressed this issue looking at a couple of additional analyses, one of which was to run a regression analysis with mean blood flow with time. This will clearly not identify a cause / effect relationship, but can provide some useful insight – which it does – there was a stronger (more significant) relationship between the mean blood flow and time for the treatment group compared with the placebo group. It look like there is a response associated with the intervention and some, but a smaller effect associated with the placebo, but with these numbers (only 7 subjects) and with the 'normal' variability of blood flow responses, it is actually not surprising that nothing was spectacular in terms of the statistical results. The peak blood flow results were very much along the same lines (detail in the original paper of course). There are some interesting further points raised

by the authors in the discussion, but the one I would be concerned about if I were looking at this further would be the disconnection of the vibrating plate in the placebo devices – the machine is still ON and the motor is humming (if you know what I mean). Detaching the plate is supposed to make the treatment not arrive at the tissues, but I would suggest that it is all but impossible for this to be a non treatment, and the addition of a control group, plus more numbers of course would be good. Anyway, makes for an interesting read and could be of interest to many of you out there with various and diverse interests.

That will do for the vibration research. There is some more out there, but this is not Vibration News, and its inclusion was on the basis of an increasing volume of literature in the field and an ever increasing number of queries about what the evidence does (and does not) say about effectiveness. I am sure that there will be some more to come in future issues, but in the meantime, into the last couple of sections.

Reviews :

Review papers are of absolutely fantastic value – if of course they happen to be reviewing something that you are interested in – taking the current literature, trawling through it, making evaluations and comparisons and reaching a conclusion (which might be of course that further work is needed before a conclusion can be reached)! I have included the reviews below as they might be useful in this context, and as they tend to be 'of some substance', I will briefly discuss their scope and essential findings rather than page by page analysis.

Magnetic Fields and Blood Flow

McKay, J. C. et al. (2007). A literature review: the effects of magnetic field exposure on blood flow and blood vessels in the microvasculature. Bioelectromagnetics 28(2): 81-98.

Instigating a change in local blood flow is something that I get to mention a lot, both in lectures and here in the Newsletter. It is one of the things that many treatments claim to be achieving, and this review looks specifically at the evidence out there on the effect of magnetic fields in this regard. The review group (from Canada) present some detailed analysis, and they try and deal with the apparent inconsistencies between different studies (some demonstrating one thing and others demonstrating something different). They are not, so far as I can see, trying to gloss over the issues or hide a mixed bag of results, but trying to accommodate the existing data into a framework – and in that respect, nicely done. The essential findings so like this : Vasodilation demonstrated in about 50% of the studies and in the others there were no report of a constriction only effect, but there were results the gave mixed dilation and constriction results and this appears to be dependent on the initial tone of the vessel. The review also considers the role of nitric oxide as a mediator (something we have looked at more than once in this Newsletter) and also some of the work on angiogenesis (again, something that has been mentioned lots of times here). At just under 20 pages, it is a substantial review, but if you happen to be looking for a review on the effects of magnetic therapy or vascular responses to therapy, might be just what you were wanting.

Complementary Therapies and Fracture Management

Sprague, S. et al. (2007). Complementary and alternative medicine use in patients with fractures. Clin Orthop Relat Res 463: 173-8.

This is a much shorter review looks at the use of CAM (complementary and alternative medicine) in patients with fractures (as I suppose you might have worked out from the title!). This is a Canadian based study, and in fact the data were collected from a limited number of institutions in Ontario – nothing wrong with that at all, just be careful about using the results out of context – clearly may or may not be true in Hong Kong, London or rural North Island, New Zealand! About 35% of the patients reviewed were using some form of CAM after their fracture, and the data presented is of considerable interest. One thing that I did note when scanning through was that although some 35% were going to a CAM practitioner, actually only just about 2% were doing so specifically for treatment for their fracture. Anyway, some fascination profile data and if you are involved in CAM based therapy, well worth a read.

Interventions for OA Knee Pain

Bjordal, J. M. 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 Musculoskelet Disord 8: 51.

There are some well known names on this review author listing, and as the title suggests, they have taken a systematic review approach and looked at RCT design research and included a meta analysis. This is all good stuff and with 36 RCT's identified, there is plenty of base material to be working with. Some treatments (like ultrasound, static magnets and manual acupuncture) did not appear to offer significant short term benefit over placebo. Pulsed shortwave gave some advantage and the modalities with the strongest effect were TENS, IFT, Electro acupuncture and laser therapy.

There is a lot more to it than that, but it is a nicely constructed meta analysis and one of the bonuses is that it is in BMC Musculoskeletal Disorders, and so at least you can get hold of it easily enough so long as you have web access – which if you are getting this newsletter by e mail, I guess you have!

Interventions for Rheumatoid Arthritis

Christie, A. et al. (2007). Effectiveness of nonpharmacological and nonsurgical interventions for patients with rheumatoid arthritis: an overview of systematic reviews. Phys Ther 87(12): 1697-715.

Another recent review which may be of some interest to you considers various non pharmacological / non surgical interventions for rheumatoid patients and, as the title suggests, is an overview of systematic reviews (28 of them).

Given that systematic reviews are supposed to provide the highest level of evidence, I am not sure how high up the rankings a review of reviews is supposed to be, but . . . . Clearly, there is a lot of material in here which does not relate to electrotherapy – or electrophysical agents (though still of major interest of course). I will confine my comments here to the type of interventions that I would normally cover in this newsletter. Of the 'electrotherapy' modalities, low level laser therapy gets the strongest support, and that is only 'medium' level evidence. The other 'classic' electrotherapy interventions, all of which were deemed to provide low level evidence and unclear findings included electrical stimulation, therapeutic ultrasound. Thermotherapy and TENS.

The electrical stimulation (what is TENS if it is not electrical stimulation – hey ho – was inconclusive as only one trial was evaluated, and there was absolutely no mention of the parameters, why the stim was being undertaken, and although a limited number of patients were involved, this is the (potential) problem with systematic reviews, and reviews of reviews – one gets so far away from the original data. If a trial was excluded from a previous review, it would not get into any others (for right or wrong reasons). My other major criticism (not just for the electrical stimulation bit) is that if a trial was conducted and identified 'no effect', and met the criteria for inclusion – RCT etc etc – BUT that the treatment parameters were wholly inappropriate – or not optimal – the conclusion could be inappropriate. If I published a trial for say ultrasound on chronic rheumatoid fingers, and it had all the hallmark 'points' that get an RCT into a systematic review, but that the dose I delivered was completely inappropriate, it would still get included in the review, and if that was the only trial in the evidence pot, then the review conclusion would be that there was strong evidence that US for chronic rheumatoid fingers was ineffective – not the same thing as saying delivery of the US at an inappropriate dose is ineffective. If there were two such trials, one at the right dose which was effective and one at the wrong dose which was not, the conclusion would be that there was no convincing evidence to support the use of US etc etc etc. Dose issues do not appear to come into the equation, and if they do, the most that gets lost is say 1 point out of a possible 20 for the measurement of trial quality. This, I know I have mentioned previously, but remains a significant concern. I have others, but will save them for another issue before you all fall asleep – though I would welcome any comments on this if you have a view!

Actually, that is not quite the end of it in that with ultrasound for example (in this review of reviews) there are 2 entries – one from the Ottawa panel (2004) and one from Casimiro (2002). The Ottawa panel conclusion was the evidence quality was 3/5 (Jadad) and the panel found 'good evidence' for US to the hand. The Casimiro review concluded that US in combination with other modalities can not be supported, but used alone in the rheumatoid hand, there was evidence of benefit (though they were more conservative about it). This (Christie) review came to the conclusion that there was low quality. I know that arguments, but one has to look at the detail to get the fuller picture.

This is an interesting review and raises several critical issues, some of which I have alluded to above. I am not saying that the conclusions reached by Christie et al are necessarily incorrect (I would be flooded by Norwegian hate mail!), but need to be read carefully – don't just scan the abstract – you might just jump to the wrong conclusion. Maybe some more on this methodology in a future issue, but for now, I had better get on with the final tissue repair section before I run out of time and pages (again)

Tissue Repair :

Eccentric exercise for chronic tendinopathy

This paper could, I guess, have been included in the previous section with the other reviews as it is a review rather than a report of experimental research. The authors, based in the research centre at Dunedin, NZ, have considered a wide range of literature relating to the effectiveness of eccentric exercise in cases of chronic tendinopathy, and present their review findings (***Woodley, B. L. et al. (2007). Chronic tendinopathy: effectiveness of eccentric exercise. Br J Sports Med 41(4): 188-98; discussion 199).***

The literature considered runs up to 2006, and a total of 11 papers are included covering several different clinical tendinopathy problems (patellar, Achilles and lateral elbow). To some extent, the quality of the literature is not good and several key methodological flaws are identified, together with the inevitable conclusion that more work is needed with larger and better quality trials. That notwithstanding, the authors did identify some key points which I will try and summarise as succinctly as I can. The overall undertaking was of some substantial size as some 450 papers were identified in the initial trawl (nice that the search terms are included in full – might help some of you), but that initial number came down (after much work one suspects) to the 11 actually reviewed. The reasons for the massive reject pile are made clear enough, and again, the detailed methodology followed was also provided. Of the 11 used studies, 4 were related to Achilles, 4 to patellar and 3 to lateral elbow. The eccentric programmes considered varied from 4 through to 12 weeks, and it looks like the majority went for the 12 week option (8 studies). The comparisons with other treatments gave some diversity as did the follow up periods which ranged from 6 weeks through to 1 year, though it was common for the main follow up to be at the 12 week, end of intervention point. Outcomes were predictably variable, but return to functional activity and pain were leaders by popularity, and a whole host of others (detailed) are reported.

The authors detail issues relating to methodological quality and then work their way through the key issues of pain, function and (briefly) return to activity. The overall conclusion was that there was a 'dearth of high quality research available to establish the effectiveness of eccentric exercise therapy . . . ' in these tendinopathies though the overall trend was for a positive effect. The points is well made that without some quality evidence, this popular intervention is rather less evidenced than many therapists assume. Reading through the abstract and maybe the conclusion to a study does not provide you with the information that you need. This is a well constructed review with quick reference tables for the 11 included studies, summary score tables and an outcome measure table which summarises a host of information, plus almost 80 references. For those with a specific interest in the use of eccentric exercise as an intervention for chronic tendon problems, this, I would suggest, is an essential read. It could easily form the basis of an interesting journal club or discussion meeting, and anybody wanting to see an example of evidence evaluation could do a whole lot worse than learn some lessons from this paper.

Stretching and Tendon Injuries

Staying with the tendon theme for just a bit longer, there was a short, but insightful paper in the same journal from a group based in Belgium and NZ (but different research group) (***Witvrouw, E. et al. (2007). The role of stretching in tendon injuries. Br J Sports Med 41(4): 224-6).***

The authors manage, in a pretty limited space to raise an issue which has been bubbling out there for a couple of years, maybe longer. Essentially they argue that due to the nature of the work that tendon has to deal with (especially in the sporting environment), there is a role for tendon elasticity, not just extensibility. It therefore follows that static stretches and ballistic stretches could both be beneficial, but that they achieve different intended outcomes. Tendon (and other 'passive' tissue extensibility is something that I have been fascinated with for some years and have published a couple of joint papers in recent times, and (with any luck) another one on its way with regards stretching and the application of ultrasound. This has all focused on long slow (static) stretching, and my understanding that this extensibility was the more important component of tendon function – elasticity was not. The authors manage to convey some impressive arguments in the space of only 3 pages which would certainly challenge my own understanding of these issues, and provide a reason to go and search the literature once again and re-evaluate. I am not saying that you should simply accept the arguments presented – but have a look, see what you think and maybe go and chase some of the literature yourself. I would expect to be coming back to this at some point in the future.

Tendon Rupture and Early Motion

Achilles and more Achilles in this issue – maybe we should call it the electrotherapy and Achilles newsletter – just coincidence – I am not that biased – just reporting what I come across. In some issues it is all OA and backs, but this time TA's rule. This paper in the American Journal of Sports Medicine (***Twaddle, B. and P. Poon (2007). Early motion for Achilles tendon ruptures: is surgery important? A randomized, prospective study. Am J Sports Med 35(12): 2033-8)*** report the outcome of a clinical trial with high level evidence (RCT style)and comes to the conclusion that early motion is an effective intervention whether surgery is employed or not (sorry if I have jumped to the last line too swiftly for you!). There must be something about the NZ output machine in 2007 as this one too comes from a research group based there (Auckland this time).

Basically a group (n=50) of acute TA rupture patients were randomised into operative (n=25) or non operative (n=25) groups, both of which were managed with controlled early motion using a removable orthosis, progressing to full WB over 8 weeks, and followed up for a year. The paper includes a clear CONSORT type flowchart which includes lost to follow ups, drop outs and re-ruptures. The final numbers in each group were 20 for the operative and 22 for the non operative groups. The outcome measures were primarily concerned with range of motion, calf circumference a MAFI outcome score,and re-ruptures/complications recording. The short version of the results is that there were no significant differences between the groups at any of the time points between 2 and 52 weeks. You might think from reading the abstract that the treatment was no good, but there were some big and impressive improvements – in both groups – and if anything, it is strongly advocating the benefits of early motion, and it is the surgery that could be called into question here – given that the non surgical intervention group did just as well.

There are some interesting and useful details included in the report, and for those dealing with TA rupture rehab, early motion programmes following injury or surgery, this would be an excellent paper to read through in its full detail.

Achilles Tendon Microcirculation

Knobloch et al, based in Germany have been mentioned in the newsletter previously, and this particular study fits with the theme running in this issue – reporting a cross sectional study looking at microcirculation changes in the Achilles (***Knobloch, K. et al. (2008). Superior Achilles tendon microcirculation in tendinopathy among symptomatic female versus male patients. Am J Sports Med 36(3): 509-14.)***

The basic aim of the work was to evaluate the differences between microcirculation changes in the TA between male and female patients, working from the hypothesis that the circulation in the female TA would be worse than that for the male. They collected a good number of these patients (total 139 of whom 58F and 81M) and basically tested their microcirculation in the tendon and paratendon areas (using 12 predefined points), identifying capillary flow, oxygen saturation, venous filling pressure. All patients recruited (full details available in the paper) had at least a 12 week history (mean of some 7 months). Patients who had been exposed to surgery were excluded (as were several other groups as one might expect). Outcomes included [ain (VAS), foot and ankle outcome score (FAOS) together with the microcirculation mapping at 2 different tissue depths (2mm and 8mm) using a specific device for the purpose. The 12 points used are detailed in the paper, but essentially, they worked from the distal attachment up to 6cm proximal in 2 cm intervals, with 2 paratendon and one tendon measurement at each level. Every patient was tested bilaterally, therefore giving 24 reading per subject.

The results are (predictably) pretty comprehensive but summarise thus : there was not statistically significant difference at any of the points tested between the male and female patients. The symptomatic tendons had a significantly higher blood flow than the asymptomatic tendons at the point of pain in both sexes. Both the symptomatic tendon and paratendon oxygen saturation was higher in the women than the men. The post capillary filling pressures were significantly reduced in about half of the test positions in symptomatic women compared with symptomatic men. There was no significant difference in pain between the genders, and there were some (but not marked) differences in FAOS scores.

There are lots of fine detail in the paper, together with a quality discussion which raises some interesting points – I'll not steal their thunder and go through them all – you need a reason to look at the original! The findings essentially show that the capillary blood flow is just about the same in symptomatic men and women, but that women have better oxygen saturation and reduced filling pressures, suggesting that the microcirculation in the women was better, and therefore rejecting their original hypothesis. The implications of these findings in relation to TA problems, pathology and recovery are considered.

Genetic Influence on Tendon and Ligament Injuries

This one takes a bit of a diversion from the routine papers in the newsletter, though, as it happens, we are sticking with ligament and tendon problems. The authors are from Cape Town, SA and present a fascinating paper (***September, A. V., et al. (2007). Tendon and ligament injuries: the genetic component. Br J Sports Med 41(4): 241-6; discussion 246).*** On the subject. Genetics is not my strong point I am afraid – if you happen to be a geneticist, then by all means provide me with some insight as I struggled with a couple of genetics modules that I followed at some point in the dim and distant past! The argument has been previously made that there might be a genetic 'predisposition' (for want of a better term) when it comes to Achilles, rotator cuff and anterior cruciate injuries. Genes have been identified which code for critical collagen (type V) regulation and another which regulates the response of the body tissues to mechanical load.

In all seriousness, the genetics detail is not my cup of tea, and I would genuinely welcome any insight that people have. I can get to grips with the basic tenets, and can follow the general argument – I just get a bit lost here and there in the detail. This has significant future potential in that if the gene identification turns out to be a 'real' phenomenon (no reason to doubt that) it might help to explain why some people experience TA etc problems whilst others in apparently identical circumstances do not. It clearly does not obviate the known intrinsic and extrinsic factors that contribute to injury, but it might make a difference in terms of screening and possibly more importantly to this readership, the treatment and rehabilitation process.

Effect of loading on the ACL

Last one for this issue – has been a bit of a marathon – but that is partly because I had to cut the last edition a bit short. I have been interested for some while in the mechanical properties of ligament and tendon (in particular) under various immobilisation and loading conditions. This study (***Chen, C. H. et al. (2007). Pathological changes of human ligament after complete mechanical unloading. Am J Phys Med Rehabil 86(4): 282-9)*** looks at the pathological changes over time in the human anterior cruciate when it is completely unloaded, and in addition, considers changes in the ACL from cadavers.

The arguments (and the evidence) for detrimental changes in ligament and tendon during immobilisation are well rehearsed, and the resulting weakening, degeneration and disorganisation will influence recovery and rehabilitation. Rehab and treatment is a compromise therefore between getting in there too early, and doing too much versus leaving the injured tissue immobilised for too long and getting a whole lot of detrimental outcomes (this is a GROSS oversimplification – mine of course, not the authors of the paper!).
The experimental model employed was a neat one in that they got access to 35 athletes who had complete ruptures of the ACL and who were going for reconstruction surgery. The ACL remnant was removed at the time of surgery (actually got 31 usable samples) and the time since rupture (between 1 and 14 weeks) was noted. Control ACL's were obtained from cadavers within 48 hours of death.

Several outcomes were evaluated, including my favourite – the crimp wave (amplitude and wavelength), fibroblast density, nuclear shape and single fibre microscopy were all evaluated. As with the previous study, there were lots of results, but the overall findings were that the histology changes were significantly different from the controls after some 5-6 weeks of unloading, following a time based sequence of events. The collagen fibre arrangements became irregular and the crimp angles changed after some 7-8 weeks, and fibroblast density increases were seen after 5-6 weeks. The crimp angles were reduced in the unloaded ACL's compared with the controls and there was a variable crimp wavelength response, with an increase in the early phases and a decrease in the latter phases.

Tension in the tissue is known to be an important part of maintaining its properties, and this demonstration that by unloading (removing tension) from those tissues has a significantly detrimental effect. The various changes identified come on after different time periods, but it looks to me like the most significant changes appear from 5-6 weeks onwards. These are fascinating outcomes and link in well with the already known detrimental effects of immobilisation and unloading. I have never seen a 'complete unloading' study in human ligament before (sorry if you have written one and I have missed it!), and the data is good fun and provides useful insight with regards potential rehab, optimal surgery and maybe could even influence immobilisation and activity time frames.

OK folks, that will do for the moment. Managed to collect another 8 papers whilst writing this lot, but if I try and cram any more in here, it will be late (again) and too big for any of you to want to read (assuming of course that you have even gotten this far!!!)

Next edition due in July (fingers crossed), but as ever, if in the meantime, you manage to find any papers, or indeed have published one, and I appear to have missed them, please do let me know. I do scan through as much literature as I can as often as I can, but still coming across papers all the time that seem to have slipped through my search strategies, so do please let me know (t.watson@herts.ac.uk).

Regards

Tim

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