Electrotherapy News

VOLUME 4	ISSUE 3	February 2009

NEWS – Electrotherapy News

This edition of Electrotherapy News has been delayed for several reasons, not least of which is that I seem to be collecting more papers than I can keep up with in terms of the reviewing and commenting process – and as a result, I have decided to try and get this issue out now rather than work through the other 6 papers that I was going to include in it. The main focus therefore of this one is a bit on ultrasound and laser followed by a substantial section on electrical stimulation. The papers that I had lined up on shockwave, magnetic therapy, vibration therapy and cold therapy will be carried over to the next issue, and I am planning for that to be out in a couple of months. Rather than 4 bigger issues a year, I am going to try for 6 bimonthly smaller issues and see if that makes the preparation and publication any more efficient?

NEWS - ISEPA

The International Society for Electro Physical Agents (ISEPA) had its first meeting in Las Vegas, USA earlier this month. There was a very packed programme with presentations covering a wide range of EPA interventions and in addition, the formation of the Society was taken forward, such that we now have a proposed Executive, objectives and basic structure. These have yet to be formalised as it is proposed that we will become a sub group under the auspices of WCPT, and until that time, we remain in a bit of flux BUT it will happen! At some point there will be a web site to serve the organisation, but in the meantime, I will try and host the ISEPA pages on the www.electrotherapy.org web site so that there is a short term solution. Once I have updated the system, I will put a straight link through to ISEPA from the main menu, so that everybody can keep up with the activities and progress of the group. Membership will be intentionally broad, and not confined to physiotherapists/physical therapists, though they are expected to constitute the majority of the membership (hence the WCPT sub group status).

This is an exciting development and should provide a forum for those with an interest in Electro Physical Agents and help to disseminate information and provide support. As ever, it is going to be reliant on voluntary activity (at least in the first instance), so progress might be limited by the availability of a small group of people who are already very busy, but it is a good move in the right direction. Watch the web site for developments, information and progress. The next planned meeting will be in association with the WCPT meeting in Holland in 2011 (http://www.wcpt.org/congress/amsterdam/index.php).

NEWS - Physiopedia

You will be aware of Wikipedia I am sure – it is amazing how often a Wikipedia definition seems to crop up in conference presentations these days! Anyway, there is a move to generate a physiotherapy / physical therapy specific version using the same concept – getting information from 'users' of the facility which can be updated and modified from a broad base. The paragraphs below are taken from the home page – it is better for them to tell you what it is all about rather than my interpretation!

> ***Physiopedia is an ambitious project which aims to eventually offer an evidence-based knowledge resource for rehabilitation professionals throughout the world. Through utilising collaborative wiki technology Physiopedia is a place where all physiotherapists can participate by contributing, sharing and building knowledge to develop a global understanding. For educators Physiopedia offers an opportunity to involve their students in this knowledge creation process as part of an educational program.***
>
> ***This project is currently being developed with universities from the UK and the USA and we are keen to establish further partnerships with other institutions from around the world. We are also seeking input from physiotherapy and physical therapy experts who will add great value to this resource.***

If this is going to work, it needs a broad and active user group, so why not have a look and link up to the site? The home page is found at : **http://www.physio-pedia.com**

News – Publications and Web Site Changes

There have been a number of papers which have come out recently with which I have had some involvement. Some are related to the now completed Apomorphine Nodules and Ultrasound Study (which I have mentioned previously) and some to the ongoing recalcitrant soft tissue lesions and microcurrent therapy research being carried out in conjunction with Leon Poltawski at the University. The current listings are up on the web pages (go to **PUBLICATIONS** from the main menu) and I will refrain from reviewing them in detail in this publication – just letting you know that they are out there. I have another 11 in preparation, so I had better get a move on and write some more!!!! On the web site, you may have also noticed a new section on **Frequently Asked Questions (FAQ's)** – available from the main menu. I get a lot of e mails every week about issues in electrotherapy – and I do my best to get them answered, but a lot of them relate to the same issues – there are about 8-10 issues that I end up covering week in and week out – so the FAQ page is an attempt to reduce the repetition and make the info easily available. I will try and keep them updated and will add some new pages as I get the chance – so it might be a useful place to have a look see if the answer is there before e mailing – just a thought.

News – Old Books

Many thanks (again) to those of you who have been helping me to complete my 'historical' collection of Electrotherapy Texts. I now have a reasonable collection of previous editions of some of the standard texts which will enable me to follow up on some of the 'retrospective' analyses I was hoping to carry out. I do still have a few 'editions' missing, so if there are any old electrotherapy books on your shelf at home or lying around in the department or clinic, gathering dust and not being widely accessed, please do drop me an e mail as it might just be that your unwanted book is the very one I need to complete the collection. I don't have a lot of financial resources to throw at the project BUT I am not trying to steal them from you! Before you put them in the bin or up on E Bay, I would appreciate first option (t.watson@herts.ac.uk). Thank you.

Contents

Ultrasound and Wound Management

Ultrasound has been used intermittently and with varying degrees of popularity over the years as part of the management of chronic and non responding wounds. This paper considers the use of ultrasound as an adjunct to the conventional therapy in wounds that have failed to respond. The paper is published in a recent issue of Physical Therapy, and is followed by an interesting invited commentary and subsequent discussion which are well worth a read in their own right. (***Bell, A. L. and J. Cavorsi (2008). "Noncontact ultrasound therapy for adjunctive treatment of nonhealing wounds: retrospective analysis." Physical Therapy 88(12): 1517-24; discussion 1524-8***). The authors are based in a specialist wound care centre in the USA, and the work is based on a retrospective study of 76 patients using wound area as a primary outcome measure. What the authors call 'non contact ultrasound therapy' – or NCUT – is not delivered at the traditional MHz frequency, but is in fact what is commonly referred to as 'longwave' ultrasound, operating in the kHz band (actually 40kHz in this case). The authors review several previous trials, some of which were controlled, some not, which together appear to support the use of US in this clinical problem. The aim of this work therefore appears to be to provide further evidence and numbers to the data that already exists by using the retrospective analysis of completed treatments in this centre.

The 'inclusion' criteria are identified – basically adults with non responding or minimally responding lesions (any aetiology). The US was delivered in a very specific way, using a saline mist as the coupling medium – hence the non-contact label. Treatment dose varied according to an algorithm employed at this facility, though only dose 'ranges' are identified rather than specific details.

A total of 76 patients records were included in the data set (collected over a 2 year period). The majority of the patients had a comorbidity (as one might expect, with both cardiovascular and vascular problems being popular, and diabetes also ranking highly in the conditions list.

The outcomes showed that treatment averages just over 5 minutes a session for an average 2.3 sessions a week over a median time of just over 4 weeks (but with a wide range of about 1 week through to almost 22 weeks). The authors note that although 18% of the wounds healed completely, the main reason for employing this modality was in fact as a debridement intervention. The median wound area reduction was 79% - which was a significant change from start to finish. A range of other outcomes are reported including wound appearance, surrounding skin appearance, exudates (nature and quantity), granulation tissue plus others. The reduction in pain (not available for all patients) was (statistically) significant at 1.8 points – though some would argue that whilst this might be statistically significant, 1.8 points may not in fact reach clinical significance.

There are several limitations to this work – some of which the authors themselves note in the final sections of the paper, and others are identified by Robertson in the commentary that follows. One of my main concerns was using wound closure rate as a primary outcome when in fact it is clear that the ultrasound as used in this clinic was in fact discontinued before wound closure – its primary purpose for application appears to be relating to wound debridement rather than continuing through to complete closure. It might have been prudent therefore to use an index of debridement rather than one of closure. Robertson (invited commentary) and the authors response between them raise a range of further issues – and I will not add to the list – but wound certainly encourage the reader to look at both extra 'sections' not just the main article. I would suggest that the paper does have some merit, though may not contribute as much as one might have hoped to the wound management debate. The lack of detail and what appear to be arbitrary clinical decisions serve to confound the findings – as does the apparent conflict already identified between wound closure as an outcome and wound debridement as a treatment objective.

For those of you with an interest in wound management, this would be a useful read. It does indentify the need for large prospective studies – with which I would agree – but preferably with clearer and non-conflicting objectives and sufficient treatment detail to be able to evaluate dose dependent effects in they are there.

Ultrasound Machine Accuracy

There have been numerous papers over the years which have highlighted issued related to the accuracy of ultrasound equipment, its calibration, the accuracy of the applied dose and ERA of the radiating device – several of which have been included in previous editions of Electrotherapy News. This latest paper from Australia (***Schabrun, S. M. et al. (2008). "The accuracy of therapeutic ultrasound equipment: a systematic review " Physical Therapy Reviews 13(6): 443-449)*** offers a review, based on the published literature , of several aspects of US machine accuracy. Schabrun has appeared previously in the Newsletter (paper on infection and US machines in 2006) and Chipchase is also known in terms of EPA research.

The basic standard on intensity states that the output on the machine itself should be within 20% of the actual output (measured by an independent calibrated meter) and that the timer should be accurate to within ±5%. The authors reviewed the literature to evaluate the pooled evidence in this field and to additionally consider any relationships between factors such as machine age, frequency of use, brand etc.

Database searching back to 1973 plus hand searching were employed to identify the relevant papers (search terms are identified in the paper – nice one) and papers were then tested against set criteria for methodological quality judgement (using 2 different quality scales – both identified and described in the review). A total of 47 papers were identified, the duplicates were removed (from the overlapping searches) and then a further 19 on the basis of the methodological quality judgements, they were left with 18 studies for inclusion in this review. The reasons for excluding studies were identified in the text. Interestingly, the majority of the identified papers were published in the early part of the period (1973-1988) though studies were included up to 2005. Only 4 of the considered studies were from within the last 9 years, and therefore one needs to take account of this – in that things ***should*** have improved over the years (!). The methodological quality of the papers was not stunningly high (detailed in the appropriate section).

Power outputs : this is a complex area in that machines offer pulsed and continuous outputs and often at more than one frequency (typically 1 and 3 MHz), but the overall pooled data showed that 13 out of the 18 studies reported the majority of tested machines were inaccurate, with the mean coming in at almost 65% (ranging from 14 to 100% of tested machines). This is no surprise if you have read any of the existing literature, but if you have not, then you might be in shock mode by now – this is not impressive, and most therapists are unimpressed with the high inaccuracy rate. There are some further detailed analyses of the data in the subsequent sections.

The timer accuracy data shows that between 22 and 30% of the timer functions were not accurate (tested over 5 and 10 minutes), and the digital timers appear to be more accurate than the mechanical ones.
The only other factor which was identified as worthy of note was the relationship between machine age and output inaccuracy – which I guess should be expected – though there was no significant relationship between service/calibration interval and inaccuracy – which was a bit of a surprise.

The discussion covers many useful areas, but at the end of the day, almost two thirds of therapists could be using machines which are not accurate in terms of the power output they are delivering – i.e. more than 20% out. Delivering a higher dose than you plan for is potentially damaging to the tissues (though I would argue with some of the 'danger' data included in the discussion. Delivering too low a dose could be a waste of time, and certainly will not result in an optimal dose delivery.

Machines are often calibrated at one power level only (typically top output – say 3 Watts $cm^{-}2$, and given that the power relationship is non linear, only doing the calibration at one power will almost certainly lead to inaccuracies. If you are getting your machine calibrated at the service, I would make sure that you get it done at more than one power level (TW comment – this is not something directly from the paper).

There are some other useful issues raised in the discussion which are well worth a read – some of which relate to clinical application techniques, and indeed, how they might contribute to the inaccuracy of the device. The limitations of the current study are also visited. The suggestion that machines only need to be calibrated every 2 years is not a universal figure – the 2006 Guidance suggests annual checks, and some have suggested that calibration should be even more frequent than that – every 6 months or even monthly. If dose is important (and I would argue that it is) then knowing that your therapy machine is operating at least within the 20% inaccuracy range could be important to the efficacy of the delivered treatment. Some useful material in here which confirms, if nothing else, that a lot of US machines in current use are probably not delivering the therapy dose that the practitioner thinks that they are. How disappointing is that?

Laser and Tennis Elbow (lateral epicondylitis)

Bjordal has produced many fine papers over the years related to various forms of electrotherapy, and the latest review (***Bjordal, J. M. et al. (2008). "A systematic review with procedural assessments and meta-analysis of low level laser therapy in lateral elbow tendinopathy (tennis elbow)." BMC Musculoskeletal***

Disorders 9: 75) should be on your 'to read' list and luckily, as it is in the BMC series, it is easy to access on line.

This is not a short paper – runs to 15 pages – and I am not going to attempt to reproduce all the issues raised here – that would mean rewriting the paper – and that would be an insult to the authors. I will pick out the most salient issues and strongly encourage you to get access to the original whether your interest is related to the laser therapy or related to the treatment and management of tennis elbow.

The advantage of this particular paper is that it takes both a systematic review and a meta-analysis approach (each of which would be of value in its own right), and the paper highlights the serious issue of dose related effects. I am still in the process of compiling a paper looking at dose related issues right across the EPA spectrum, but Bjordal et al have clearly identified just such an issue in this context. The 1300 articles originally identified was swiftly reduced to n=18 (explained and rationalised) which was further reduced to 13 papers which were adequately sound and met the inclusion criteria.

The introduction summarises the current position with regards the management and background laser issues, following which the methodology adopted is clearly and fully explained, including both primary and secondary outcome measures and a comprehensive section on publication bias.

Treatments reported were either direct (i.e. over the tennis elbow site) or indirect (using acupuncture points). 8 of the 13 included trials showed positive effects for laser over placebo (8 out of 11 for the direct method). The strongest combination that produced positive outcomes was when a 904nm laser was used with a direct treatment technique. Trials which used other wavelengths failed to establish a positive effect. The laser when applied to the acupuncture points appears not to be effective, and the one trial which employed a visible red laser (632nm) also produced positive outcomes.

There is a lot of detailed analysis and pooling of data for the meta-analysis, full details of which you can easily read through (and indeed you should!). In summary the pain relief data was available for 10 of the trials. The laser groups did significantly better than the placebo groups, and importantly, those receiving 904nm laser did better still than the average. There are pooled analyses for global improvement (also better with laser than for placebo), and again the 904nm group did better still. Painfree grip strength and time off work are also analysed further as secondary outcomes.

The discussion ranges over several areas, and comes up with the (disappointing) finding that there does appear to be a publication bias in this area, such that papers reporting a negative finding are more likely to get published than those with a positive outcome (the opposite of what most people considering publication bias would predict I guess). Without trying to replicate the discussion in full, the strongest (positive) outcomes came from a distinct subset of later treatments : using 904nm with low output (5-50mW) using a direct treatment over the affected area, using 2-6 treatment points and doses ranging between 0.25 – 1.2 Joules per point. The significant effects of this treatment were maintained at follow up (3-8 weeks). The one decent trial using 632nm was also effective, but at a higher dose.

A nice review and meta analysis, written by a team who fully report their method and take into account publication bias as well as methodological issues. Their conclusions include a dose based sub group analysis,

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

which in clinical terms actually comes to a conclusion other than the predictable ' . . . . .more research is needed in this area . . . . '. The therapy is both safe and effective (at the right dose), and although more work is needed, this review should make a significant contribution to the literature with direct relevance to clinicians working in this field. Read and enjoy!

TENS and Lung Function

There are a lot of papers in this issue relating to Electrical Stimulation in various forms, the first of which may be relating to an area where many people would not think of this as an obvious intervention. Lau and Jones from Hong Kong (***Lau, K. S. and A. Y. Jones (2008). "A single session of Acu-TENS increases FEV1 and reduces dyspnoea in patients with chronic obstructive pulmonary disease: a randomised, placebo-controlled trial." Australian Journal of Physiotherapy 54(3): 179-184)*** have evaluated a single low frequency TENS session (45 minutes – real or placebo), applied to an acupuncture point in a group of COPD patients (n=46), looking at the effect of the intervention on lung function and dyspnoea. The trial was an RCT design, and all patients were over 60 years of age with a confirmed COPD diagnosis, attending for pulmonary rehab. Both the patients and the assessors were blinded to group allocation. The TENS (real or placebo) was applied to the Ex-Bl points (either side of C7 vertebra – picture in the paper) which is a widely reported acupuncture point used in TCM for shortness of breath. The 'normal' electrodes were applied over a punctured plastic film in order to restrict stimulation to the specific point intended. Stimulation was for 45 minutes @ 4Hz with a 200 microsec pulse duration. Intensity was at max tolerance, short of discomfort. The placebo group stimulation followed the same procedure but without the punctures in the plastic film. The primary outcomes were lung function tests : FEV_1 and FVC via spirometry. Dyspnoea assessed with a shortness of breath VAS scale (previously used and validated).

The mean age of the 46 participants was 75 years (31M 15F) with a mix of ex smokers (n=15), current smokers (n=22) and non smokers (n=9)and predicted FEV_1 of 69% and FVC of 73%. There were no significant differences in essential baseline measures between the two groups. The results show a significant increase of FEV_1 in the treatment group by 0.12 litres and an increase in FVC of 0.05 litres (which was just non-significant). The treatment group also showed a change in dyspnoea score of 11 mm on the 100mm VAS scale more than the control group.

There is a detailed discussion of these results compared with others previously reported. The authors did not investigate the mechanism of the significant reduction in dyspnoea and improvement in FEV_1 but go on to suggest a number of possible mechanisms which may account for the changes measured. The key limitations of the study were noted, but given that many of the patients attending the clinic travelled by public transport (an interesting experience as I recall in Hong Kong!!), they were classified as moderate COPD patients, and needed a rest before testing commenced – the 30 minutes allowed may not have been enough to enable a return to their actual baseline.

The study presents an interesting management option for patients with moderate COPD, and raises some issues which deserve to be followed up. Although electrical stimulation and COPD may not sit together in an obvious way, this is a well presented study and well worth a read.

Electrical Stimulation and Spasticity

As it happens, here is another one from the Australian Journal – unlike them to have SO much electrotherapy material – and one following right on from the previous . . . . what has come over them?? (only joking before I get a raft of hate mail!). Anyway, this is a report of a controlled trial from a group in Iran (***Khalili, M. A. and A. Hajihassanie (2008). "Electrical simulation in addition to passive stretch has a small effect on spas-***

ticity and contracture in children with cerebral palsy: a randomised within-participant controlled trial." Australian Journal of Physiotherapy 54(3): 185-189) who, as you can guess from the title are looking at the effect of adding an electrical stimulation intervention to a passive stretch event in children with CP – looking at the effect on both spasticity and contracture.

It is argued that both spasticity and contractures are distinctly disadvantageous for any child, and certainly are common in children with cerebral palsy. Stretching and serial splinting are known to have some effect on these clinical issues, and it is proposed by the authors that the addition of electrical stimulation to the quadriceps might contribute a useful additional benefit – may be through an additional stretching stimulus or through a reciprocal inhibition route. The research question was only concerned with the short term changes in these problems – though of course, in the clinical world, if this is shown to be effective, one would think that a study with a longer follow up to look at carry over effects might be of value.

The research used a within subject design such that passive stretching was applied to both lower limbs, but only one limb was treated with electrical stimulation to the quadriceps. The leg to receive the electrical stimulation was determined at random. The stimulation was delivered to the quads for 30 minutes, 3 times a week, and the passive stretching was applied (to treatment and control limb) 5 times a week. The intervention period (both groups) was for 4 weeks. Measurements were taken at baseline and at the end of the 4 week treatment period. The stimulation was applied at 30Hz with a 400 microsecond pulse duration, 4 seconds on, 4 seconds off (50% duty cycle) and a ramp (up) of 0.5 seconds. The intensity was as high as could be tolerated with a visible contraction. The hamstrings were stretched immediately following the end of the quadriceps stimulation – technique described. The passive stretching was not really detailed other than stating how often (5 times a week for 4 weeks). Technique, who did it, what procedure, how long for on each occasion, rest periods, stop point etc are all absent from the report (which is a shame really).

The outcome measures are detailed, using a modified Ashworth scale for the spasticity and goniometry with passive knee extension for the contracture (techniques for both are described). The sample size was limited (n=11) with a mean age of just under 13 years. One patient dropped out during the study (detailed on flowchart), leaving 10 data sets for control and 10 for treatment.

The results : the analysis included both within and between group data. The treatment group showed a greater reduction in spasticity score (modified Ashworth) than the control group, and similarly, the increase in passive knee extension was greater in the treatment group. Both of these changes were statistically significant, but one might ask about the clinical significance of the changes – no doubt that they were there, no doubt that they were statistically significant, but the between group difference for ROM was 4 degrees and the difference in spasticity score (between groups) was 0.8 points.

The authors themselves raise the issue of clinical significance in their discussion. It was suggested that a 1 point change on the spasticity scale would be the minimum to be clinically significant – and with a 0.8 point difference between groups, it is a marginal change. Several suggestions are raised in the discussion both in relation to the limitations of the study and the clinical relevance of the findings. They suggest that with the magnitude of the changes observed, it is probably not worth the 'extra' treatment (compared with the passive stretching alone), but in fairness, this was a small scale study, and the change that they showed was 'real' and significant. It would be worth following up with a larger scale study or with a modified regime before discarding it altogether – the extra difference might just turn out to be worthwhile after all.

Electrical Stimulation of Muscle and Patellofemoral Pain

A recent paper in Archives Phys Med from a research group in Austria have evaluated the potential benefit of adding quadriceps electrical stimulation to a regular treatment programme for patients with patellofemoral pain (***Bily, W. et al. (2008). "Training program and additional electric muscle stimulation for patellofemoral pain syndrome: a pilot study." Arch Phys Med Rehabil 89(7): 1230-6).*** There were 38 patients in the trial (36 completed) with a 12 week treatment period and a 1 year follow up, using an RCT design. It was argued that the addition of electric muscle stimulation to a routine exercise based treatment programme might offer an advantageous outcome in terms of pain (VAS outcome) and several other key assessment measures. Including a functional score (Kujala patellofemoral score[KPS]) and a test of isometric quads strength.

The patients all had patellofemoral pain of at least 6 months duration and both inclusion and exclusion criteria or detailed in the report. The routine exercise programme was of 12 weeks duration and follows a specific protocol which is detailed I the paper. The electrical stimulation group received, in addition to the routine exercise schedule, a home based period of electrical stimulation using a 2 channel, battery powered stimulator. Four self adhesive electrodes were used with the machine set at 40Hz and a 260 microsec pulse duration, duty cycle of 5 sec ON and 10 sec OFF for 2 x 20 minute stimulation periods each day, followed for the whole 12 week period. A compliance record was maintained. There is insufficient details about the electrode placement in the published report – it just says that the electrodes were placed ' . . . on both ends of the quadriceps muscle . . . ' Given that the patients had bilateral patellofemoral pain, one assumes that one channel was used for each limb. The intensity was set to be as high as could be tolerated but with pain and patient discomfort being taken into account.

The 38 recruited patients were randomly allocated to exercise or stimulation groups. One was lost in each group (patient progress chart is included in the paper) leaving 18 in each group for data analysis at the end of the 3 month period and then 13 from the exercise group and 16 from the stimulation group followed up at 1 year.

The results show that there was a significant reduction in VAS pain score for both groups by the end of the treatment period, and this was maintained at the 1 year follow up. There was no significant difference BETWEEN the groups. Similarly, there was significant improvement in the KPS score for both groups, maintained at 1 year follow up, and again, no significant difference between groups. The data shows that there was a trend for the stimulation group to actually do better on both VAS and KPS scores even though it was not a significant difference (TW comment). The isometric strength scores did not show any significant change over the treatment period, nor was there any significant difference between the groups (though looking at the data, it would appear that there was a reduction in isometric strength for the exercise group and an increase in strength for the stimulation group – even though it was not a significant change).

The study shows that a supervised exercise programme over a 12 week period demonstrates a significant improvement in both pain and functional capacity for these patients. It also demonstrates that the addition of a home based electrical stimulation protocol makes no significant difference to this outcome. There are several limitations to the study – some of which the authors acknowledge. The study was described as a 'pilot' – which is fair enough – but as such it was underpowered (24%) and unlikely to identify some of the clinically relevant changes. I would also suggest that they missed an opportunity here to look at the VMO / Lateralis relationship in these patients. IF the patients patellofemoral pain was related to an imbalance between the quads components (a widely proposed aetiology) there was no way of knowing whether this was the case in these patients, and furthermore, I would have thought that 'general' quads stimulation might have been less effective than concentrating on the VMO activity. I appreciate that this is not what they set out to evaluate, but certainly it would have been useful to do the VL/VMO ratio analysis at baseline and at 3 months – would have added to the data available. The authors make no mention of the data from the compliance diary unless I missed it (I would personally be impressed if their patients had done 20 minutes stim twice a day for 12 weeks!!), and using a stimulator with a built in compliance monitor would have been exceptionally useful – worth considering if anybody is planning on going down this route in future.

All in all, this is an interesting study. It clearly demonstrates the benefit of a monitored exercise programme, and fails to demonstrate any additional benefit of a home based electrical stimulation schedule. It might be that the stim protocol was not fully appropriate to the problem presented, and there were missed opportunities (especially in relation to the VL/VMO ratio). Useful platform for anybody considering taking this one further.

Electrical stimulation of Muscle after Rotator Cuff Surgery

A sports medicine based group from Boston in the USA report an interesting study using NMES (muscle stimulation) to the infraspinatus muscle post rotator cuff surgery to see if it can result in an enhanced contraction when measured with a hand held dynamometer (***Reinold, M. et al. (2008). "The effect of neuromuscular electrical stimulation of the infraspinatus on shoulder external rotation force production after rotator cuff repair surgery." Am J Sports Med 36(12): 2317-21).***

Thirty nine patients were recruited post surgery (supraspinatus repair) with a mean age of 54 years. The majority (37/39) had been operated using an arthroscopic technique and 2 had a 'mini' open surgery. The stimulation was applied over the infraspinatus muscle belly (average of 10.5 days post surgery) using a 50Hz stim, 300 microsec pulse duration, using a symmetrical waveform (according to the abstract) and an asymmetrical waveform (according to the methods section!!!) with a 1 second ramp. The isometric strength of the muscle was evaluated with 3 maximal (5 second) contractions with, and 3 without the stimulation. The dynamometer was placed just proximal to the wrist (photos in the paper). The reliability of the testing technique was 'improved' by using the same tester on each occasion, though there is no indication of just how reliable this procedure was (not always known for being the most stable and repeatable outcome measure). The stimulation intensity applied was at maximal tolerance.

The results showed that there was a significantly greater force production when using the NMES compared with a maximal voluntary contraction without the stim (22% greater). Sub group analysis showed no correlation with age, time since surgery, gender, size of the repaired tear of the intensity of the applied stimulation. The study was not a 'treatment' study per se, in that it only evaluated the effect of a single stimulation period post operatively. The authors suggest that in other published work, when a similar intervention has been tried post ACL repair, the use of stimulation to the quadriceps has resulted in an improvement in gait and functional outcomes. It is proposed therefore that NMES to the shoulder complex muscles post rotator cuff repair may result in an enhanced outcome – fair enough as a suggestion, but clearly not something that they have actually evaluated in this study.

Several proposals are made with regards the potential application of this stimulation as a part of post operative rehabilitation programmes, and there are some useful ideas which deserve to be followed up with a patient population through a recovery programme – would make a nice project for anybody with an interest in this field, and who has access to enough rotator cuff repair patients to make the sample size large enough to be useful.

Electrical Stimulation to the Upper Limb Post Stroke

There have been numerous studies in recent years considering the potential value of electrical stimulation programmes for patients post stroke (several of which have been reported in previous editions of this newsletter). This study, from a combined group based in Southampton (UK) and California evaluated the impact of an implanted electrical stimulation system on upper limb function in a group of hemiparetic patients (***Turk, R. et al. (2008). "Therapeutic effectiveness of electric stimulation of the upper-limb poststroke using implanted microstimulators." Arch Phys Med Rehabil 89(10): 1913-22).***

Whilst I appreciate that implanted microstimulator systems may not be an everyday possibility for many therapists, this is likely to become more prevalent as a treatment option as time goes by (something that would have been VERY experimental and futuristic only a few years ago), and is worth bringing to the attention of those if you involved in stroke rehab.

The study was a straightforward before and after study on a small number of patients (n=7) who were all at least 6 months post stroke and will reduced upper limb function. The intention of this intervention was to use electrical stimulation to assist with elbow, wrist and finger extension during reach, grasp and release of an object. The introduction provides a well constructed summary and rationale for the work – a useful read in any case. Subjects responded to adverts in local newsletters and stroke club adverts. Patients needed to have sufficient passive joint range and sufficient flexion activity to make the implantable system worthwhile. Several screening assessments were carried out, including HADS, Mini Mental and Health Locus of control in addition to the physical review. The implanted stimulators were of a radiofrequency type such that the electrodes are implanted, but the power is supplied from an external source using an inductance coil (described in the paper). The stimulators were programmed to provide the stimulation pattern (timing, duration, ramp) which would be appropriate for the individual. The optimal position for the implanted electrodes was determined preoperatively using an EMG evaluation (again, this is adequately described in the appropriate section of the main paper). Between 5 and 7 microstimulators were implanted for each patient.

Stimulation commenced 2 weeks post implantation with a determination of the parameters that were appropriate for the individual patient, and these were frequently checked at intervals throughout the programme. Patients attended twice weekly for the first 3 weeks in order for the stimulation programmes to be refined and for the subjects to learn how to use the system, and once they were independent in this regard, they started the 12 week home use programme. Regular contact and refinement were continued throughout this period. Outcome measures were taken twice before implantation (4 weeks pre and 1 day pre) and again at the end of the 12 week home Rx period (though the first baseline assessment was for familiarisation purposes and was not used in the analysis).

The outcome measures (described and justified in the paper) included the ARAT and tracking a motor control activity. The ARAT test battery is well validated (possible score range 0 – 57) and a change in score of 5.7

is deemed to be a clinically relevant improvement. A specific test rig (detailed) was used to evaluate motor control, spasticity, active ROM and isometric force for both the flexors and extensors. Reliability of this measurement system is also reported and appears to be very good. A detailed and full explanation of these outcome measures is included in the script and again, is well worth a read to see how a well developed test battery can be worked up, justified and explained.

The results of the study are that the 7 subjects recruited ranged from 32 to 67 years old with a mean time post stroke of almost 4 years (range just over 1 year through to over 10 years). The compliance was shown to be good with an average of over an hour a day use over 2 months. Improvement in all scores was shown between baseline and the 12 week point. There are a lot of outcome measure and therefore a lot of data and analysis – which is great stuff, and not for me to reproduce here line by line – go to the original – it is well worth a read. The authors conclude (quite reasonably in my own opinion) that the study shows that for these patients (more than a year post stroke), the combined exercise programme and electrical stimulation schedule can result in a significant improvement in function and reduced impairment. The authors further differentiate changes in outcome measures related to specific impairments and those relating to functional change. Importantly, the data from this work show improvements in both areas. Three of the patients who made the greatest clinical improvement showed changes in ARAT score of almost 13 points – well above that deemed necessary to be clinically relevant.

I have no doubt that there is more to come from this group (and others I am aware are using this type of system). It is a well written and presented paper, and for those of you who are led to believe that only RCT's are worth reading might want to use this as an example of a pre-post non-controlled trial which none the less makes a significant contribution to our knowledge and understanding in this emerging therapy field.

Electrical Stimulation and Assisted Cycling Post Stroke

Another paper looking at electrical stimulation post stroke also appears in Archives Phys Med (***Janssen, T. et al. (2008). "Effects of electric stimulation-assisted cycling training in people with chronic stroke." Arch Phys Med Rehabil 89(3): 463-9)*** and is lead by a Dutch team with links to a clinical research unit in Manchester, UK.

The basic argument is that cycling post stroke has demonstrable benefits but can be problematic. Reducing the loss of muscle capacity might enhance recovery (less time needed to regain the lost muscle function) and electrical stimulation to the paretic limb alongside a cycling activity may provide a useful adjunct to a rehabilitation programme (the authors provide a stronger argument, but this is my brief summary). The basic proposition is that using electrical stimulation alongside a cycling programme might provide not only an improvement in cycling, but also in aerobic capacity, functional capacity and muscle activity. To evaluate this, a group of 16 chronic stroke patients were recruited against a range of inclusion and exclusion criteria. None of them were currently receiving therapy and all were reasonably able in terms of functional mobility. Patients were randomly assigned to one of two groups. Both were exposed to a cycling programme and electrical stimulation to the affected limb BUT one group were stimulated at motor levels and the others only at a sensory level – therefore producing a level of blinding. The cycle ergometer was a semirecumbent model which incorporated electrical stimulation to the glutei, quads and hamstrings. The stimulation was set at 60Hz with a symmetrical biphasic sine pulse with a pulse duration of 450 microsec. The intensity for the stimulated group was set as high as they were able to tolerate and for the 'exercise' group was set at a sensory level (sub motor).

There were a raft of outcome measures including functional tests (6 minute walk test, Berg Balance Scale and Rivermead Mobility Index). In addition, a graded exercise test and aerobic capacity assessment

(VO2max) and heart rate were recorded. Muscle strength (both limbs) was assessed using a custom built measurement system.

The training protocol was repeated twice weekly for 6 weeks. At each session, the aim was to achieve between 25 and 30 minutes of exercise in 3 bouts with adequate rest between bouts. The level of these exercises on the cycle ergometer was determined for each subject on an individual basis (details in the paper). The results showed (in brief) the 4 of the patients withdrew from the study (detailed) leaving 12 subjects for the final analysis. The aerobic capacity improved significantly for both groups over the training period, though there was no significant difference between groups. The lower limb strength changes were less remarkable, though demonstrable, with no significant differences between groups. The functional scores were also better at the end of the test period, with an improvement in the Borg Balance score (which tended to be larger in the stimulation group) and also an improvement in the 6 minute walk test which was not significantly different between groups. The overall outcome was that the activity improved cycling performance (predictable I guess) and also aerobic capacity and functional performance, but not muscle strength. The addition of the electrical stimulation (motor level) did not achieve any significant benefit over the group who received sensory stimulation.

The authors offer a useful, comprehensive and insightful discussion with regards their finding and those identified by previous research. They also acknowledge several limitations to their work, not least of which was the small sample size and therefore low powered study. It would appear that the addition of electrical stimulation to this cycle ergometry programme does not provide additional benefit – or if it does, then it has net been confirmed by this study. There were clear advantages for the patients in carrying out the cycle training, it is just that the stimulation offered nothing additional. There is more work to be done here and several obvious variations on a theme that would be worth following up if the opportunity arose.

Electrical Stimulation Post Hip Arthroplasty

Electrical stimulation to the quads and the calf for patients following total hip arthroplasty (THA) is reported by a research group in France. This was an RCT design following 32 elderly patients and compared a 'normal' treatment group and a normal plus electrical stimulation group (***Gremeaux, V. et al. (2008). "Low-frequency electric muscle stimulation combined with physical therapy after total hip arthroplasty for hip osteoarthritis in elderly patients: a randomized controlled trial." Arch Phys Med Rehabil 89(12): 2265-73).***

The authors argue that muscle weakness post THA can lead to functional and rehabilitation issues and also that electrical stimulation may be a useful adjunct to routine therapy in order to enhance recovery. This has been tried before with somewhat unequivocal outcomes, but the applied stimulation in this previous work was at low or moderate intensity, so the aim in this work was to try the stimulation at a higher intensity.
All recruited patients were 70 years or older and all had a THA for OA hip less than 2 weeks before the start of the trial. Twenty nine were recruited and allocated at random to either a routine treatment group or a routine treatment plus electrical stimulation group. The stimulation was applied to the quads and the calf, bilaterally using 2 battery powered, portable devices. The current was at 10Hz with a 200 millisecond pulse duration (though this might actually have been misreported as microsecond pulses are the more normal with this type of stimulation). The stim was cycles at 20sec on and 20 sec off. The full details of the pattern of stimulation is not included in the report (or else I missed it) i.e. were these alternating or co-contractions, no details of ramp settings etc. Stimulation, using surface electrodes (positions detailed) was for 1 hour a day for 5 days a week for 5 weeks and in addition, the stim group also received the same 2 hours a week of routine therapy that patients in the 'control' group received. The stimulation intensity was progressively increased to the maximum that the patient was able to tolerate.

The 'normal' therapy programme is described and would be considered an intense programme (2 hours a day, 5 days a week for 5 weeks) by the standards normally encountered in many units that I am aware of. The outcomes measures included maximal isometric quads strength using a (described) dynamometry system, a 6 minute walk test followed by a 200m walk test. The length of stay and a FIM assessment were also included in the testing raft.

29 patients completed the trial – details of the drop outs were included in the write up. There were no significant difference between the two groups at baseline. Quads strength (operated and non operated sides) were tested using an ANOVA and post hoc showed a significant increase in strength pre to post in the stimulation group (77% on the operated side and 15% on the non operated side). There was a significant increase in strength in the control group (23%) but only for the operated side. The increase in strength for the stim group was significantly greater than for the control group. There were no significant differences in the outcomes for the 2 walk tests between the two groups. The FIM scores improved significantly for the stimulation group only (per-post).

These results show some significant advantages for the stimulation group compared with the routine treatment group in this trial – there were strength improvements, a better balance of strength in operated and non operated limbs and also a greater independence (as per the FIM score) for the stimulation group patients. It is interesting that although these significant changes were demonstrated in the stimulation group, the walking scores and length of stay durations were not significantly different between the groups. The authors work their way through these and other issues in a long discussion which is worth a read. There are several limitations to the study (which the authors identify) and a couple of additional pieces of information that I (personally) would have liked to know the details – especially with regards the stimulation schedule. Knowing whether the stimulation was delivered in a synchronous or asynchronous pattern, the phasing and the ramping would all be useful if you were going to take this therapy and try it out on your own patients. It would appear that this 'high intensity' stimulation was well tolerated by the patients in this trial, and none of the drop outs were related to an intolerance of the stim. There are lots of avenues that could be followed up – and hopefully somebody, somewhere will do just that. In the meantime, the effect of strong low frequency stimulation post THA in elderly patients appears to have gained some additional evidence in its favour.

Electrical Stimulation and Force Preservation with Spinal Cord Injury

And another electrical stimulation study . . . and another from Archives Phys Med . . . . and no, it is not the only journal that I ever access – just a coincidence that there are a load of them in this particular issue of the newsletter! The use of electrical stimulation in patients with spinal cord injury is not new, and I have reported several previous papers on the subject. This particular report comes from a research group in the States (***Chou, L. W. et al. (2008). "The effectiveness of progressively increasing stimulation frequency and intensity to maintain paralyzed muscle force during repetitive activation in persons with spinal cord injury." Arch Phys Med Rehabil 89(5): 856-64).***

Functional Electric Stimulation (FES) is an effective means to provide some SCI patients with enhanced mobility and functional capacity. One of the problems associated with the intervention is muscle fatigue, and

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

trying to overcome this issue is a primary concern for some researchers in this field. The research reported here (and I will keep it brief) evaluates the effect of manipulating both the stimulation frequency and the stimulation intensity as a means to overcome the fatigue effects often seen. The implications of this work will (may) have an impact for those working with FES – which I appreciate is not the majority of you), and may result in more effective stimulation protocols in the future.

A total of 8 children (5M, 3F average age around 14 years) who presented with complete motor cord lesions were recruited – so this was an acknowledged small study. There were detailed inclusion and exclusion criteria plus a screening process to ensure suitability – all detailed in the paper. The volunteers were tested using a computer controlled dynamometer which recorded isometric quads strength during various electrical stimulation combinations. There were 2 different fatigue protocols, and each subject was tested twice, in random order with sufficient gap between sessions to allow wash out.

At each session, the subjects maximal twitch force was established using a 600 microsec pulse delivered every 10 seconds with increasing intensity. Once the plateau response had been achieved, stimulation was applied at 100Hz for one second, applying the voltage required to generate the same twitch force. Pulses were delivered at different frequencies (between 12.5 and 80Hz) and at what the authors refer to as varying intensities (which are actually described as varying pulse durations between 150 and 600 microsec) to determine force frequency and force intensity responses. The combinations were delivered in a random sequence. From this data, the frequency and intensity increments needed to be applied for each subject to maintain peak muscle force output was established (with me so far???). Stimulation was then delivered at 60Hz using a 600 microsec pulse duration with increasing amplitude until the maximal twitch force was obtained. This intensity was then kept constant.

This establishes the normal set of responses for the individual using a set protocol. Following this, one of the two fatigue protocols was employed with a 300ms pulse train at 30Hz delivered at a rate of 1 train every 1.1 seconds at an intensity to gave the same maximal twitch force. The different fatigue protocols either worked by altering the stimulation frequency or the intensity (which was actually the pulse duration). The test sequence is therefore not simple, and is explained in more detail in the original paper together with some additional materials in the appendix for those with a specific interest in this field.

The outcomes of the work (complex results) showed that stimulation that progressively increased both the stimulation frequency and intensity (pulse duration) produced more successful contractions that stim patterns that increased either frequency or intensity (duration) alone. The optimal sequence appears to be to increase intensity (duration) first, then the stimulation frequency. If this were employed in the FES clinical setting, it is suggested that the duration of the effective FES period could be extended.

This is a technical paper, and I probably have not done it full justice here. If you are involved in FES, and especially if using FES with SCI patients, this information might just be critical as it has the possibility of increasing the duration of the effective FES period. I am not sure that it has any immediate value outside this field – though I may be wrong there – just that I can't see any immediate transfer just at the moment.

Electrical Stimulation and Seating Pressures for Spinal Cord Injury

The last of the main electrical stimulation papers – there seems to have been a fair raft of them in this issue – this one is again looking at e stim issues for spinal cord injury (SCI) patients, but in this case, considering the effects of stimulating the gluteal muscles on seating pressures, based on work done by a research group in Holland (***van Londen, A. et al. (2008). "The effect of surface electric stimulation of the gluteal muscles on the interface pressure in seated people with spinal cord injury." Arch Phys Med Rehabil 89(9): 1724-32).*** The concept behind this work was fairly straightforward – SCI patients have problems with pressure sore formation in the gluteal region, and these are clearly problematic once developed. There are several reasons why they are more likely to develop in this patient group (nicely outlined in the intro). SCI patients have long duration relatively static postures and with the gluteal atrophy that will follow the SCI, there is a reduced muscle bulk between the seating surface and the bony pressure points, further reducing capillary flow and raising risk. The idea of delivering electrical stimulation to the gluteal muscles is not unique to this study and has been tried several times before but this study looked at a couple of different issues – mainly looking at the effect of the stim on the pressure areas and secondly, considering whether a simultaneous or alternating (left/right) stimulation pattern was more effective (it was considered that simultaneous should have the better effect on the pressure interface but that the alternating stim should bring on less muscle fatigue). The trial was fairly small (n=13) with SCI patients with no existing pressure lesions and all were screened to ensure that electrical stimulation, using surface electrodes would bring about a contraction of the gluteal muscles. Patients were tested at a single session with the 2 different protocols (simultaneous or alternating stimulation) using their normal wheelchair and cushion system. The pressures were recorded using a calibrated pressure (FSA) system and samples were taken at 10Hz. Stimulation was delivered using 2 single channel stimulators which were synchronised through a computer system, delivering rectangular monophasic pulses at 50Hz with an 80mA intensity. The electrodes were placed over the glutei with one electrode near the lower border and one over the main muscle belly.

The stimulation protocols summarise thus : Alternating pattern : 0.5 sec stim of one muscle followed by a 15 second rest followed by a 0.5 sec stim of the other muscle followed by 15 seconds rest., this cycle was repeated 60 times (taking 31 minutes). The simultaneous protocol was similar in that the stimulation was for 0.5 seconds followed by 15 seconds rest, resulting in 120 contraction periods in the same 31 minute intervention period. The 2 interventions were evaluated in a single session with a 15 minute rest period between the interventions.

There were several pressure measures taken (detailed in the paper) including the mean pressure under the ischeal tuberosities, the maximum pressure the pressure spread and the pressure gradient. There are some additional details for those of you who have a specific interest in pressure measures.

The results (in brief) show that there was a significant reduction in the mean interface pressures during both the alternating and the simultaneous stimulation. There was also a significant reduction in the maximum pressures with both protocols, and a significant change in the pressure gradient. The pressure spread (rest vs stimulation periods) did not change significantly in either protocol. There was no significant difference between the pressure changes between the protocols, though the authors not a tendency for the simultaneous stimulation to result in a larger decrease in interface pressure.

There are a number of implications from this work which the authors raise in the discussion. Some previous work has shown that when using implanted electrode systems, and using repeated stimulation periods, there is not only a reducing in the interface pressure during the stimulation periods, but there is also a more general reduction in pressure, presumably as a result on increased gluteal muscle bulk and resultant change in tissue shape.

It is not known whether the level of pressure changes demonstrated in this study would be sufficient to have a pressure area preventative role (also discussed), but that fact that there were no significant differences between the protocols should provide useful information for anybody working in this field, whether with implanted or surface electrode systems. It is still a logical argument that the simultaneous stimulation is likely to result in a larger pressure drop and that the alternating stimulation would result in less fatigue. More work clearly needs to be done in this area, and it might be that there are other protocols which are more effective still. The electrical stimulation has been shown to be effective, and given the limitations of the described study, there did not appear to be any significant difference between the two test protocols employed in this instance. Despite several technical and methodological limitations, this data is useful and should provide a platform from which the next research can move forward.

Electrical Stimulation and Inflammation

The last paper in the current issue looks at the relationship between some aspects of electrical stimulation and the inflammatory state in tissues (***Odell, R. H., and R. E. Sorgnard (2008). "Anti-inflammatory Effects of Electronic Signal Treatment." Pain Physician 11(6): 891-907)*** and is available for free download from the journal web page (www.painphysicianjournal.com).

This is a review paper rather than reporting the results of a specific research activity and aims to consider the potential role of EST (electronic signal treatment) in relation to inflammatory events and pain mechanisms. This area is large, and draws upon several fields including biochemistry, cell biology, pharmacology/kinetics and a whole host of others. Many of the basic arguments presented are not new – by the authors own admission – and many overlap with areas in which I have been interested for many years. That there is an endogenous bioelectrical activity in the body is well established. A wide range of tissue, cellular and subcellular activity has a link to the electrical signals present in the tissues. These are not the same thing as the 'electrical' signals associated with classic nerve and muscle activity – and in fact, it was these endogenous signals that I was trying to measure and evaluate during my own doctoral research back in the late 80's and early 90's. OK, so if one accepts that (a) these bioelectrical signals exist and (b) that they have an influence on physiological activity (both of which I think are well evidenced), then monitoring them might tell you something about what the body is doing, and changing them might have the potential to change what the body is doing.

Odell and Sorgnard work though a range of such possibilities in this paper, drawing upon a variety of sources to support their argument. I will not try and work through the whole thesis – in fact they leave out some of the difficult bits (. . . till a later paper . . . .), but the areas they cover go like this : basic issues of inflammation and aspects of drug based anti inflammatory action – steroids and NSAID's). They then step back and look at the use of electricity in medicine (albeit briefly) and then move on to try and link these areas, though the most frustrating thing (for me) is that when they talk about EST, they remain particularly vague about the electric stim they are talking about – they mention frequency and amplitude effects and mention modulation effects (all good stuff), but it is difficult to see whether they are actually talking about the application of existing technologies/modalities or whether they are going to come up with a 'new' stimulation system that fulfils these requirements (I suspect the latter!).

The potential anti inflammatory effects of EST are presented – with some nice arguments (if a bit difficult to follow at times) and despite several assumptions and hypotheses integrated into the evidence based text – that is fine – we all do this, but you have to tease out what is 'known' and what is hypothetical in order to get the best out of the review. They talk through the potential for an EST type treatment to influence the inflammatory events and identify a number of mechanisms by which this might happen, including filtration/diffusion effects, pH normalisation, cAMP mechanisms, membrane repair options and metabolic activity. One of the interesting areas (for me) is the consideration of antidromic nerve activity – the conduction of impulses the 'wrong way' along a verve fibre – something that I have been playing with for some time as a potential therapy option. They then add it material about sustained polarisation (as opposed to immediate depolarisation/repolarisation events) and immune system changes.

You can see what a range of physiological 'stuff' is covered here, and even if you have an interest in this field, you are probably going to have to read it more than once to get to grips with the proposition being made. The basic tenet is that there is a role for EST (their term) as a part of the management of inflammatory events and processes. Their proposal is that the treatment is primarily anti-inflammatory in nature (which I am not totally convinced about – I would look at it is an inflammatory modulator – playing with the normality of inflammation rather than 'stopping' it – but that might just be a matter of semantics) and they propose that this might just be a better way of 'managing' the inflammatory events than relying on a drug based approach – which I have no problem with.

I suspect that there is some more to come from this group and maybe it will serve to reactiviate this bioelectrics field where intermittent research and publication activity seems to be the name of the game. I would certainly commend the paper to you and a consideration of an interesting area, and one that certainly has the potential (sorry about the pun) to impact on therapy if we can just get it right. It may be that we need a whole 'new' approach to make the most of the endogenous bioelectric system,. It may be that the therapies that we already deliver already play this game one way or another, but the possibility remains, and if bioelectrics, inflammation, pain or neurogenic inflammation are anywhere close to your interest zones, get a (free) copy and see what you make of it.

OK, so that will suffice for this issue. I have another 10 electrical stimulation papers sitting in the pile (2008 ones) plus a wide range of papers involving other interventions. These will go into the next issue, which as I mentioned in the opening news, should be out in a month or so. If I have missed any papers, please let me know (as ever). If you have published a paper and I seem to have missed it out – it might just be on the 'to do' pile, or I might have missed it – so let me know and I will try and get it included (t.watson@herts.ac.uk).

Keep you eye out for the ISEPA developments – some exciting stuff should be happening in the near future.

Regards

Tim