A survey of the “surgical and research” articles in the *European Spine Journal*, 2007

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Abstract Over the last couple of years the *European Spine Journal* has become truly international with papers from all over the world, and at the same time it has increased its size. Professor Mulholland has selected and reviewed some 40 papers from over 200 published in 2007 and that he felt were of particular interest to practicing surgeons and would influence their management of patients, or papers that challenged established beliefs. Papers dealing with back pain, spondylolyses, tumors, spinal stenosis, spinal infection, clinical examination, lumbar disc herniation, spinal fractures, etc. are reviewed and their significance assessed. The aim of the review is to encourage readers to read the papers themselves, hopefully stimulated by the trenchant comments of the reviewer, both critical and laudatory.

Keywords Thoracolumbar fractures · Spinal stenosis · Cervical spondylosis · Spinal cord monitoring · Scoliosis

Introduction

The enormous variety of paper in this years' journal is a reflection of the enlargement of Eurospine, the Spine Society of Europe, to embrace so many other specialist societies, both in Europe and outside Europe, and the many non-member attendees of the annual meeting from all over the world. The *European Spine Journal* is truly international and its contents reflect the same. Pressure of space inevitably prevents me from discussing many very important papers, and inevitably those I select reflect my own interests to some degree.

Back pain

Back pain is a near universal human experience, and it is only of significance for two reasons, either the cause will lead to serious injury to the spine, or the pain is seriously disabling itself although the “cause” is in itself benign. A paper and a special poster (the latter winning the Special Poster Prize at the Eurospine Meeting in Brussels) dealt with the important role of sensitization of the nociceptive system. The special poster by Jensen et al. [25] and the paper by Schenk et al. [45] both deal with this. Both authors carried out tests referred to as “dolorimetry”; a psychophysical method of assessing forces required to produce pain in distinct locations, the thumb nail and other soft tissue points. The Jensen group found that in their 239 patients, a high dolorimetry score (25% of patients) was

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negatively associated with disc degeneration or root entrapment, that is, a high score meant that organic disease was unlikely. The Schenk group dealt with 106 female workers between 45 and 62, 38 had suffered from recurrent low-back pain, 68 had not. They found that there was no relation between the degree of pain sensitivity and liability to attacks of low-back pain. Despite this negative result the paper explores in some depth, the whole issue of pain sensitivity and the various confounding factors. Fibromyalgia is now an accepted diagnosis in rheumatology and if we recognize this as an extreme disorder of the nociceptive system it is a somewhat more acceptable and understandable disorder.

Patients with chronic back pain dislike sitting, but is prolonged sitting a cause of back pain? The paper by Lis et al. [28] reports an extensive literature review, (24 studies) which indicated that if sitting is associated with awkward postures or whole body vibration; as for example in Helicopter pilots, then it does increase the likelihood of low-back pain. Sitting comfortably for more than half of the working day is not a factor in producing low-back pain or sciatica if it is not associated with the above factors.

Intradiscal steroid has been used in the treatment of low-back pain intermittently over the years, with disappointing results. The paper by Fayad et al. [17] is of interest as it gives some basis for its success in some patients for a limited period. A study in which they compared its success in patients with Modic changes 1 (inflammatory) with those with Modic changes 2 (fatty degeneration), it achieved short-term success in the former. This paper partially confirms that there is an inflammatory element in patients with Modic 1 changes, and also explains the random short-term success in some patients before the advent of MRI.

Anyone treating back pain wishes to know the tissue source of the pain, although we are aware that this cannot be discovered with certainty in the majority of patients. The review article by Hancock et al. [21] dealt with the value of tests to identify pain from the disc, the facet joint, or the SIJ by assessing from a literature review the likelihood ratios of various tests. A positive likelihood test >2 , or a negative likelihood test of <0.5 was considered informative. Absence of degeneration on MRI scan was the only test that reduced the likelihood of the disc being a pain source, no tests for facet pain were found to be informative using these criteria, and manual tests of the SIJ were only informative if combined with other tests. They point out the low positive rates for discography, if strict criteria are used for a positive result. As they conclude the usefulness of these tests in clinical practice for guiding patient selection, remains unclear.

It is always of interest when “obvious truths” are challenged. Most of us would hold the view that people in the nursing profession would get more back pain than

health administrators (secretaries). But it seems not to be the case. The paper by Schenk et al. [44] is a complex paper, but has a number of important conclusions. The physical demands of nursing, clearly different from those for a secretary, are not causative of back pain, back pain occurs equally in both groups. Hence we cannot give generalizable recommendations for the prevention and therapy of non-specific low-back pain in the two professions. The other conclusion of interest was the occurrence of back pain was strongly associated with complaints in other body regions.

In dealing with chronic back disorders, amenable to surgery, the time of intervention is a difficult decision, operate too soon, and one may operate on patients whose disability would have improved without surgery, but if one delays too long then the result will be compromised. The paper by Braybrooke et al. [5] and the comments on the paper by Freeman [18] should both be carefully studied by the excessively cautious surgeon, or the therapist who persists in conservative treatment for overlong, for a disorder that has a surgical solution. The recommendation in the European Guidelines is that one should delay some 2 years before doing a fusion for back pain, is challenged by Cost [10].

Cost effectiveness

At this time when the supposed greater cost-effectiveness of various procedures may determine their use, the paper by Soegaard et al. [48] makes some very important points concerning this subject. They point out that unless allocation is random to the operations being compared, then the supposedly greater cost effectiveness of one procedure, may merely be a reflection that it was in that selected group of patients, the more appropriate procedure. They also make the point that focussing on subgroups is important insofar that patient characteristics that may be associated with greater cost effectiveness of a procedure may be modified at considerably less expense than the extra cost of the surgical technique.

Adjacent segment degeneration

As the early results of total disc replacement are similar to those of fusion, disc replacement is often advocated on the ground that it may not lead to adjacent segment degeneration said to be produced by fusion.

The paper by Schulte et al. [46] effectively casts doubt on this reason for a total disc replacement. It is the case that there is an adjacent disc space narrowing, indicating degeneration in some 20% of patients, but it did not

correlate with clinical outcome at 10 years. The authors conclude that the loss of adjacent disc height is a consequence of three factors which alter biomechanical loading, ongoing degenerative disease, and advancing age.

Clinical examination

How much can we learn about the function of the lumbar spine by examination? The paper by Qvistgaard et al. [41] reported the results of two skilled specialists in manual medicine examining for segmental disturbances in the lumbar spine. Both were blinded to any diagnosis. In that part of the study that dealt with intra-observer agreement, there was a 70% chance of diagnosing the same segment, and 82%, if one included the adjacent segment. The agreement between examiners was even worse, some 42%, the same segment, and 75%, if the adjacent segment was counted as agreement. What was even more disconcerting was that in the ten normal patients, nine had a segmental dysfunction diagnosed. It was emphasised that this study involved two examiners with more than 20 years experience in the examination of the lower back and with years of coordination between them in a specialized clinic. My conclusion was that this paper demonstrated how valueless were claims that clinically relevant segmental dysfunction could be diagnosed by physical examination. Physiotherapists and therapists who claim to be able to do so should repeat this type of study.

On the other hand the paper by Quack et al. [39] looked at the correlation between MRI abnormalities and restriction of lumbar movement, looked at spinal movement as a whole and its relation to MRI. Their finding that only occasional significant correlations could be found between MRI findings and standard mobility tests was disappointing. However, this was more a criticism of the tests themselves, (finger-tip to floor distance and modified Schober) as expert observation of the spinal movement, rather than just measurement of range was able to detect altered patterns of movement. Expert observation in this paper did confirm degenerative changes at L2-3 did affect lateral bending significantly, but this effect was not so obvious on standard measurement. The important message of the paper was that careful observation of spinal movement was more useful than a blanket measurement in assessing movement differences between upper lumbar and lower lumbar spine, whereas the previous paper suggests identifying segmental differences is much more questionable. Clinically identifying in a patient with low-back pain that the movement loss is upper lumbar is, I believe, of value in identifying an upper lumbar disc protrusion, where the radicular signs are much less obvious than at lower levels.

Spinal stenosis

In 1990, Ooi et al. [35] showed some very graphic myeloscopic pictures of the effect of calcitonin on the perineural vessels, shrinking them dramatically, in patients with spinal stenosis walking on a treadmill, fitting in with the concept that congestion of these vessels was an important factor in the symptomatology of spinal stenosis. Porter and Miller [38], in a prospective randomized study, identified a non-significant clinical effect of its use. The excellent paper by Tafazal et al. [53] is short and damning as regards the value of salmon calcitonin used as a spray in treating spinal stenosis. One presumes that the very dramatic effect shown by Ooi when the drug is injected is too transient to be of clinical value. Is it possible that a different method of giving the drug may be more effective, perhaps allowing it to be longer acting?

The paper by Cavusoglu et al. [8] is of interest reporting as it does a very minimal surgical technique to deal with bilateral stenosis, through a unilateral approach using the microscope. They reported on 100 patients, with 267 levels decompressed, and compared unilateral laminectomy and unilateral interlaminar approach; the latter was as good as the former in terms of result. The procedure is a very minimal one, and perhaps it is this type of surgery that should be compared with the use of devices such as the x-stop [55]. Many more spinal surgeons are now competent with a microscope, especially in the cervical spine, and this technique illustrates its value in the lumbar spine.

Lumbar disc protrusion

Surgeons who have the reputation for being conservative are highly regarded, we in Europe were proud of our conservatism in the management of lumbar disc protrusion as compared with the US. However, the paper by Hansson and Hansson [22] establishes very clearly that although surgical treatment has higher medical costs, the total costs, including disability costs were much lower than in those treated conservatively. This paper should be read in conjunction with the paper by Luijsterburg et al. [29]. Their systematic review included 30 randomly controlled trials, and dealt with a total of 2,780 patients. They looked at injections, traction, physical therapy, bed rest, manipulation medications and acupuncture. Short-term pain relief was achieved by radiographically controlled root injections, and surprisingly pain medication did not affect overall improvement and sick leave. They looked at one study comparing physical therapy to surgery, and at the 1-year-period, surgery was superior. Their conclusion is that at present there is no evidence that one type of conservative

treatment is clearly superior to others, including no treatment, for patients with a lumbosacral radicular syndrome. The conclusion from this finding is that if there is no evidence that conservative therapy alters the natural history of recovery that prolonged conservative therapy, if that therapy itself may be preventing return to work, is wrong.

In the UK, at present it is NHS Policy, that all back pains, including radicular syndromes, have to be referred to a Triage system, and from there they have a period of physiotherapy before any review by a surgeon. The two papers above show the dangers of this practice.

Post-operative care after disc surgery

One fear that patients have if they agree to surgery for a lumbar disc protrusion is that their back is irredeemably weakened, and certainly in the past much inappropriate advice was given to patients concerning their recovery from such surgery. The paper by McGregor et al. [34] conducted a systematic literature search, and reviewed the evidence on post-operative management, and found that there was a little evidence for any post-operative restrictions, and a strong case for an early active approach. This message was extracted and developed into a patient centred message in an educational booklet. They highlight the fact that surgeons often advised quite unnecessary restrictions post-operatively, and emphasised how important early return to full activity was in producing a rapid recovery and relief of pain. Not only patients should take on this message, but many surgeons and physiotherapists. The value of the booklet is currently being tested in a RCT of rehabilitation; presumably comparing “usual advice” to “booklet advice (surgeon and patient)”. We will await the result of this study with interest.

Cauda equina lesions

In the UK, delay in treating cauda equina lesions is a frequent reason for allegations of medical negligence, and the damages awarded depend very much on whether the outcome was affected by delay. The paper by Qureshi and Sell [40] reported a prospective longitudinal inception cohort study of 33 patients undergoing surgery for a cauda equina syndrome due to a PID to determine what factors influence spine and urinary outcome measures at 3 months and 1 year with regard to the timing of onset of symptoms and the timing of surgical decompression. There was no statistically significant difference in outcome with respect to length of time from symptom onset to surgery, <24 h, between 24 and 48 h and >48 h. A significantly better outcome was found in patients who were continent of urine

at presentation compared with those that were incontinent. The paper confirms the findings of McCarthy et al. [31], although they found that urinary disturbance at time of presentation did not affect outcome. Unfortunately both papers deal with delay before being seen in hospital, so the evolution of symptoms during this time is poorly documented, and there is no clear record as to whether during the period of delay the neurological picture changed. The Qureshi paper, which found the poor results if incontinence was present at presentation to hospital, would suggest that if during the waiting period, whether in hospital or prior to admission there was a documented progression of neurology, especially from a mild bladder disturbance to an established retention then delay in decompression was causative.

Tumors

In the spinal cord ependymomas are the most common neuroepithelial tumors, accounting for some 50–60% of adult spinal tumors. The paper by Gavin Quigley et al. [19] whilst only dealing with outcome, does show that the result of treatment, be it surgery alone, or surgery followed by radiotherapy is influenced very much by the pre-operative neurological state. They are slow growing tumors and their presence can be established long before there is any neurology by MRI imaging. The possibility of such tumors must make us consider whether we should ever discharge a patient with non-specific back pain without doing an MRI. It was of interest in this paper that partial excision (partial because of the extent of the tumor) combined with radiotherapy, was as good as total excision. Clearly it is not justifiable in pursuing the laudable aim of complete excision if this is likely to produce neurological injury.

Scoliosis

The papers by Hempling et al. [23] including the comment on it by Arlet [3] and Suk et al. [49] are of considerable importance. With the development of thoracoscopic surgery there has been much discussion as to whether anterior release prior to posterior correction of adolescent scoliotic curve should be done by open operation or thoracoscopically. These papers question whether it is necessary at all. Hempling shows by the use of pre-operative and post-operative traction films that the effect of an anterior release was small, and that a posterior release with osteotomy of the concave ribs and an opening of the facet joints had a much greater effect on spinal flexibility. Suk shows that in a group of 35 patients treated for posterior correction alone

using pedicle screw fixation, satisfactory deformity correction was achieved, and obviated the need for anterior release. These papers will have a significant effect on the surgical management of adolescent deformity.

The very comprehensive paper by Koroivessis et al. [27], which explores the effect of bracing on the perceived health status of treated adolescents, compared with controls with no deformity, and inevitably the effect of this on compliance. The significant psychological effects of bracing, and the high incidence of back pain in those braced, and the sleep disturbances experienced, must be factors that should influence a decision to brace rather than surgically correct. The authors recommend psychological support and physical training to improve compliance, especially in the older adolescent.

One of the most exciting papers was that by Izatt et al. [24] dealing with the use of a physical model of the deformed spine, created from three dimensional CT images, called stereolithographic biomodelling, and used to help the surgeon plan and execute surgery. Twenty-eight sequential complex deformity patients were modelled, 22 models in 21 patients with deformity, and 6 models in 5 patients with tumors. A post-operative utility survey was completed by the surgeons, which they assessed by answering a questionnaire the value of the model. They found it to be the most useful visual modality in the pre-operative planning stage in 70% of patients and the most useful intraoperative visual modality in 89% of patients. Surgical times were reduced in 89% of cases by a mean of 63 min/case. In all cases, the surgeons' view was that the model had a very positive effect on the outcome of surgery, and they would order a bio-model again in similar cases. Although clearly, such assessment would have much personal bias, but the details of the questions asked, and the illustrations were very persuasive that this development is one that could be increasingly used in such complex surgery. The idea of using such models for teaching the nature of common deformities was attractive, and also the fact, that the cost of such models if used for surgery was covered by savings in surgical time.

Cervical spine

Surgery for cervical radiculopathy is less certain of success than surgery for a lumbar radiculopathy. The paper by Alrawi et al. [1] examined the reasons for this and prospectively evaluated the value of pre-operative neurophysiological studies (nerve conduction studies and concentric needle EMG). They demonstrate that when these are positive, and identify clearly the root involved, then surgical results are better. Whilst they would not necessarily deny surgery to a patient with a negative study,

the patient should be made aware that surgical success would be less certain.

The paper by Grob et al. [20] examined the correlation between the presence of neck pain and alterations in the normal cervical lordosis of the cervical spine, comparing patients without neck pain and those with. Their finding was that there was no significant difference between controls and those with neck pain, and hence that the presence of such structural abnormalities in the neck, in a patient with neck pain was coincidental, and therefore not necessarily indicative of the cause of the pain. There is a little evidence that the presence of altered cervical curvatures is of prognostic or diagnostic significance.

They do not comment on the logical conclusion of their findings, whether extension exercises and the creation of a "normal" lordosis should continue to be standard physiotherapy advice in patients with neck pain.

The paper by Rubinstein et al. [42], which reviewed the literature to establish the value of four tests used in the diagnosis of cervical radiculopathy, ULTT, Spurlings test, tract/neck distraction test, and Valsava manoeuvre was therefore of some interest, especially as their conclusion was that none of them had both specificity and sensitivity, and the number of studies evaluating their use were few, and their quality was poor. More high-quality studies are required to determine the diagnostic accuracy of these tests especially in a primary care setting. One of their criticisms was that the description of how the tests should be done was not clearly defined in the papers they reviewed, but they themselves did not describe how the tests should be done, but referred one to a paper in another journal, in which the tests were described in an appendix. The educational value and readability of the paper was reduced by this simple omission.

The brief paper by Kasai et al. [26], which showed that people with developmental spinal stenosis also had narrow faces (reduced inner canthal distance) was an interesting observation for the embryologist, but the authors suggestion that "a glance of the outpatients' eyes one may make a good speculation on the patients cervical spinal stenosis" is I suspect a wistful and entertaining thought only.

The paper by Persson et al. [37] addresses the problem of headache in patients with cervical degenerative change. They asked the question as to whether nerve root compression in the lower cervical spine can produce headache. They reported in a consecutive series of 275 patients with cervical radiculopathy, that 161 suffered from daily or recurrent headaches, located most often unilaterally on the same side. After selective root block, 59% of patients with headache, reported 50% or more reduction and of these patients, 69% had total relief. Reduction of headache was associated with reduced pain in the shoulder, neck and arm. If a patient had a root entrapment (fourth to seventh), there

was a 50% chance that they would have unilateral headache, less likely with the eighth root.

Spine infection

The review paper by Chen et al. [9] and the comments on it by Sell [47] must be read together. The first paper establishes on a reasonably referenced base, inevitably of cohort studies, that internal fixation which provides stability encourages resolution of infection, and instrumentation in the presence of infection is not contra-indicated, as was established in the case of the axial skeleton many years ago. However, in case this message implies that instrumentation is a part of treating infection, the comments by Sell give a very valuable message concerning the many other important factors that play a role in the management of vertebral infection, and pointing out that in one series Carragee [7] even in those undergoing surgery for such infection, 42 out of 111, only 14 required instrumentation. The fact, instrumentation is safe, does not imply that it is always indicated.

Spondylolysis

Although the paper by Debusscher and Troussel [11] deals with the use of a particular device, it has some important messages concerning repair of a spondylolysis in the presence of disc degeneration. The presence of a spondylolysis leads to degenerative change in the disc at that level [52]. The less satisfactory result of lysis in the older patient is attributed to the fact that with increasing age, discogenic pain is more important than lysis pain. In this series patients with what was considered on MRI scanning to have degeneration grade 3 or less on the Pfirrmann scale were offered a repair alone. Of eight patients with a Pfirrmann grade of 3, five had good or excellent results, and three had fair results, with failure of the lysis to heal in two of these patients. It is a shame that infiltration of the pars before surgery was not also used in the pre-operative work-up, as this might have further defined, which of the older patients (over 30) could benefit from repair alone. All patients under 30 with no disc degeneration had good or excellent results and all consolidated.

Thoracolumbar fractures

In 1994, McCormack et al. [32] on the basis of plane X-rays and CT scans developed a classification fracture severity, concentrating on the progressive loss of the ability to bear load of the vertebral body. The paper by Altay et al. [2] demonstrated the practical value of this classification. They compared the results of short segment fixation of

Magerl type A fractures, (fixation of one vertebra above and one below), with long segment fixation (two levels above and two below). They found that short segment fixation achieved adequate fixation, without implant failure or correction loss if the fracture load sharing classification (LSC) was six or less, and the Magerl type was A3.3. However, if it was A3.3 and the LSC was seven or more, or between eight and nine, in Magerl A3.1 and A3.2, then long segment fixation was more beneficial. This paper is a great argument for proper classification of the fracture before treatment, allowing a proper selection of the most appropriate length of fixation, hence preserving motion, in those where short segment fixation is appropriate, which is of great importance in those patients with significant concomitant neurological injury.

Two other papers dealing with thoracolumbar fractures must be considered with the above paper. The paper by Defino and Canto [12] reports on just 20 patients, but all had short segment fixation and loss of correction was in those with a poor load sharing classification, two of whom required further surgery from the front. However, at 2 years, the loss of disc height of the intervertebral disc above the fractures vertebrae was the only radiological parameter that showed correlation with the clinical result. This supports the view that clinical outcome is related more to the degree of disc degeneration produced by the fracture than deformity.

The paper by Briem et al. [6] looks at some 906 patients with thoracolumbar fractures treated over a 10-years-period, with a follow up of 5 ± 1.7 years, looking at their quality of life. They showed that all patients had a mild reduced quality of life, not related to deformity, but related to the severity of the injury. Unlike patients with low-back pain, psychosocial or pain regulation disturbances did not play a role in this disability.

With the increasing use of vertebroplasty and kyphoplasty in the treatment of osteoporotic fractures, it is an attractive concept to use these techniques in traumatic fractures in normal bone. The paper by Maestretti et al. [30] in which a calcium phosphate material is used, with the hope that it will in time be replaced with bone, certainly demonstrates a very effective clinical success, and it is certainly an attractive technique in the multiply injured patient. However, clearly at 3 years, it is not converted to bone, and one must be concerned at the presence of a foreign body, with somewhat unsatisfactory mechanical properties compared with bone, yet occupying a significant part of the vertebral body in some one who may be young. The comments on the paper by Boszczyk [4] are very pertinent, and my initial enthusiasm for the paper was tempered after reading them.

There were two supplements this year, one of the annual meeting proceedings, and one dealing with spinal monitoring. Publishing the papers and posters of the

annual meeting in a supplement is an excellent idea, as it very much facilitates reference and recall; the meeting becomes integrated into the academic advances recorded in the journal in the year. The second supplement is a required reading for all spinal surgeons. It has a wealth of information, logically arranged, so that reading the whole supplement is easier than one might anticipate and richly rewarding. The opening editorial and the history set the scene, by Dvorak et al. [13] and Tamaki and Kubota [54], about monitoring in the past, and monitoring now and in the future. We are introduced to the concept of multimodal intraoperative monitoring, and that it consists of intraoperative spinal and cortical-evoked potentials combined with continuous EMG and motor evoked potentials of muscles [50]. We are provided with an American view, by a neurologist, anesthesiologist and spine surgeon [36], and then a number of papers dealing with its use and results in various types of spinal surgery, principally from the Schulthess Clinic Zurich, Switzerland [14–16, 51], from Italy [43] and Saudi Arabia [33]. These papers cover the use of MIOM in the thoracolumbar spine, the thoracic spine, the cervical spine, dealing with stenosis and deformity, and also intramedullary tumors [43, 51]. I particularly enjoyed the paper by Sala, as it indicated the particular value of each monitoring system as one dissected into the cord in removing a tumor, allowing one to understand the necessity for a multimodal system. In over 1,200 patients reported in various papers, there was no permanent spinal cord injury. This litigious age, major neurological deficit after spinal surgery nearly always leads to litigation, and it is clear that the effective use of MIOM is an effective means of preventing such disasters. This supplement must not only be read by spinal surgeons, but also by administrators whose responsibility is to provide the equipment, and who in most cases will have to pay compensation to those cases where MIOM has not been used, and disastrous neurological consequences have occurred.

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