The Michel Benoist and Robert Mulholland yearly European Spine Journal Review: a survey of the “surgical and research” articles in the European Spine Journal, 2010

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Introduction

The philosophy of EuroSpine, the Spine Society of Europe and the Journal, is the furtherance both of knowledge, and education concerning disorders of the spine, for those in training, for trainers, and all practicing surgeons. Developments in the Journal throughout the year are evidence of this.

The first editorial of the year by Aebi [2] drew attention to the plans throughout the year for an education plan for spine specialists in Europe, which is to be continued annually. Such a development may create a degree of uniformity of practice in Europe that may facilitate the development of a recognized separate Spinal Speciality in Europe.

The Ganga operative spine Course [4] was an excellent example of this educational program, as it detailed

operative techniques for 12 operative procedures, recently developed, and increasingly used, which would not be in any textbook as yet. Such is the pace of development of spinal surgery.

Supplements are of particular educational value, as they focus on a particular subject, and present fully up-to-date information of clinical relevance.

Supplement 1 (March) dealt with vertebral fractures. The issue of classification was dealt by Aebi [1], who reviewed briefly various classifications, and then described concisely and clearly the AO Group (Magerl) Classification [25]. Central to this classification is the identification of a posterior lesion, to identify B and C fractures. Unfortunately, if the lesion is soft tissue alone, then X-rays and CT will not identify it unless associated with anatomical displacement. Clearly, clinical examination, identifying a gap between the spinous processes, and acute local tenderness, is available to the clinician, but not to the radiologist. However, the paper by Parizel [34] on imaging deals with the value of MRI in identifying soft tissue injuries, and clearly plain films, CT and MRI should be the usual imaging of a thoracolumbar fracture.

It was useful to read together the papers by Le Huec [22] who presented the results of operating on 50 patients, and Rajasekaran [39] who reviewed the literature on conservative treatment of such fractures. In the Le Huec series of 50, 49 were all A fractures, presumably stable (the only one other was C1) and none had neurological injury. Yet all had major surgery, involving posterior fixation and a major implant anteriorly. Admittedly, this approach was very successful in preventing kyphosis, better than some other surgical techniques, but how important is residual kyphosis clinically? Many of their patients had kyphosis well below the 20° suggested by Munting as justifying correction, the range being 6°–30°. I was concerned that the authors

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quoted as justification for their approach a paper by McAfee [7] from 1982, reporting 12 patients all with neurological injury. I am surprised that Le Huec et al. did not consider the extensive literature on canal remodelling, especially in those patients with no initial neurology [11, 46, 55] in considering their indications for such an aggressive approach. Their operation is technically demanding but very effective, and less invasive than other combined techniques, but surely should be reserved for the unstable fractures.

The paper dealing with management of cervical spine trauma by O'Dowd [32] was delightful, clear and concise, of particular value both to the trainee and the practicing surgeon.

The degree of kyphosis after a thoracolumbar fracture that we should correct is open to debate. The paper by Munting [29] suggests that a 20° kyphosis merits correction if accompanied by poor functional tolerance. However, he makes some interesting general points concerning the overall influence on the sagittal balance of the spine, and this influence is much affected by the level of the deformity; a kyphosis in the lumbar spine is of much greater significance than in the dorsal spine. This paper is very valuable to anyone dealing with posttraumatic kyphosis, both because of the advice it gives concerning patient selection and the surgery. Perhaps, one of its more important messages is that despite careful selection and appropriate surgery, only 60–70% of the patients are significantly improved as regards pain. Minor deformity with a lot of pain not explained by objective facts is a contra-indication to correction.

The *Supplement 2* (July) is a collection of case reports. In the past, these were scattered throughout the year in the monthly journal, now they are all in one supplement. I suspect that the likelihood of them being read is reduced. However, those that not only give the case report, but also carry out a review of the literature are of interest and of value. I particularly enjoyed the paper dealing with treatment of a cervical osteoblastoma [43], and the report dealing with iatrogenic injury of the thoracic aorta by an implant [21]. Both had excellent literature discussions. The two case reports dealing with hydatid disease were of interest, one dealing with total excision of an intra-dural extra-medullary hydatid cyst, and one dealing with aspiration of a cyst within muscle and injection with albendazole [5, 8]. In view of the absence of any bone involvement in the first case, one wonders whether the technique of aspiration used with such success in the second case might have been applicable.

Supplement 3 (September) contains all the papers and posters presented at the EuroSpine Meeting in Vienna. Although those attending the meeting get a copy, this copy provided to journal subscribers (all members of the Spine

Society of Europe) is most valuable for members who are unable to get to the annual meeting. It repays careful study. Many of the podium presentations will be expanded and make their way into the general spinal literature and can be easily accessed on the net. However, some are important because of their immediate relevance to current practice. An example of this is the paper by Sears et al. (Presentation 61) concerning the risk of adjacent segment surgery after a fusion. It reports that in a review of some 912 patients who had fusions, after 10 years, 10% of patients with one level fusion would have surgery to an adjacent disc. The fact that this included patients with degenerative disease and lytic spondylolysis, and that the incidence in the latter was even lower, suggests that degeneration is a more important factor than excessive stresses above a fusion. The much greater incidence of adjacent segment failure above a three-level fusion may be more indicative of a biomechanical cause, but equally would be the case if the patient had generalized degenerative disease of the discs. This is an important contribution to the continuing debate on the importance of adjacent segment disease—a disorder that has only come into prominence as a justification for the use of a disc replacement, it never concerned us when fusion was the standard treatment of low back pain.

Disc herniation

Surgical treatment of lumbar disc herniation is one of the more successful operations done by spinal surgeons. During the year, there were some important papers in this field. Silverplats et al. [45] looked at results of surgery at 2 and 10 years, and noted that they were very similar, that progressive deterioration did not occur, some 70% being excellent or good. However, the important observation was made that results were less satisfactory in a number of ways, if prior to surgery the patient had been on sick leave for more than 3 months. This has serious implications for the view that we lose nothing by delaying surgery, because according to Peul [35], results at 2 years are the same in patients treated conservatively or by surgery. If it is decided to treat conservatively, then we must be certain that the patients are not off work whilst they recover. The paper by Mariconda et al. [27] looks at the frequency and clinical significance of the long-term degenerative changes in the disc 10 years after discectomy. Although those who had had a disc operation had more disc space narrowing, facet arthritis and endplate changes than a control group, there was little clinical difference between them. These changes might be related to the surgery, but equally well as the authors point out, they may be related to the natural history of a degenerated disc presenting with a herniation.

The paper by Mysliwiec [30] dealing with the classification of the position and size of a herniated disc on MRI scanning and relating this to criteria for surgical treatment tabulates what I suspect most of us do, but it is of value for the purposes of clinical record. The poorer results of foraminal protrusions are of interest. This was a type of protrusion often dealt with very satisfactorily by Chymopain when that was available.

Orthopaedic surgeons deal with thoracic discs through the chest, because of the serious risk of cord injury if dealt with by laminectomy and an extra-dural approach, necessitating displacement of the cord. The value of a neurological perspective is clear in the paper by Moon et al. [28] dealing with the transdural approach. Many spinal surgeons from an orthopaedic background are being exposed to a neurosurgical module in their training, and this approach is attractive, as it does not have the morbidity of an approach through the chest, and because going transdurally allows one to divide the dentate ligament within the dura, allowing safe and minimal cord retraction to gain access to the herniation.

The paper by Lotan et al. [24] dealing with conjoined nerve roots is a valuable reminder that this abnormality should always be thought of when dealing with a lumbosacral herniation, especially if it is not very large, and the patient has claudication and minimal restriction of self leg raising, yet is very symptomatic. MRI now allows the diagnosis of a conjoined root to be made before surgery, and thus not only avoid the risk of nerve injury, but also the surgery involved will be a generous decompression, not necessarily involving removal of the hernia.

The two papers on dural tears during surgery are a valuable contribution to the subject. The one by Strömqvist [47] reports a prospective study from the Swedish Spine Register, dealing with 4,173 patients operated upon for a disc herniation, of whom 2.7% had tears. Although those patients who had dural tears tended to have had previous surgery, the fact that they had a tear did not have any negative implications in the long-term outcome for the patient at 1 year. Some useful advice is given in the paper on the role of preoperative planning to reduce the risk of a dural injury, by carefully studying the preoperative MRI, especially when there has been previous surgery.

The concept that access related morbidity will be reduced by minimal intervention techniques in dealing with a herniated disc has encouraged the development of various endoscopic techniques.

The paper by Teli et al. [49] points out the higher risk of dural tears and indeed recurrent disc herniation with lumbar micro-endoscopic discectomy. With this new technique, the results are the same at 2 years compared with conventional surgery, but costs and complications are higher, including dural tears, root injuries, and recurrent protrusion. This was a prospective randomized trial of 240

patients, randomized into micro-endoscopic, micro (use of a microscope) and standard discectomy. The use of a microscope added some extra costs, but was otherwise the same as a standard operation. There were no infections in the endoscopic group, and although dural tears were more common, none developed a meningocoele. It is of interest that hospital stay was few hours longer than the other two techniques. The review article by Nellensteijn [31] dealing with transforaminal endoscopic surgery looked at 1 randomized controlled trial, 7 non-randomized trials, and 31 observational studies, and concluded that there was insufficient evidence to show that it had any advantage over open micro-discectomy. They draw attention to the existence of a significant learning curve, so that patients at the beginning of this curve had a worse outcome. Will these papers influence the use of these techniques?

Spinal stenosis

With an ageing population, operations for spinal stenosis will continue to be a significant part of spinal surgery. The paper by Mannion et al. [26] dealing with the 5-year outcome of surgical decompression of the lumbar spine without fusion gives useful information to the clinician when a patient is being counselled preoperatively. About a quarter of the patients had further operations, and those who did have further operations did less well despite the further operation. The clinical situation at 5 months did not change significantly at 5 years, apart from some increase in back pain, which was considered clinically non-relevant. Patients at 5 years experienced moderate levels of disability in everyday's activities similar to patients with non-specific back pain, which the authors suggest is not an indication for further intervention, but is a degree of disability that patients have to live with.

The paper by Leonardi et al. [23] deals with the interpretation of postoperative MRI. They compared the appearances of MRI in patients with a postoperative cauda equina lesion, with MRI done in postoperative patients who had no evidence of neural compromise. The size of haematoma and the degree of dural compression was significantly larger in the patients with cauda equina symptoms. Although the indication for surgical intervention must be the clinical situation, this paper indicates that MRI can make a significant contribution to the decision, although intervention when clinically indicated should not be delayed unduly to obtain an MRI.

Vertebroplasty

The paper in the New England Journal of Medicine last year by Buchbinder et al. [10] which, based on what were

described as two randomized controlled trials, suggested that vertebroplasty was little more than a placebo, created some dismay in the media, and amongst many spinal surgeons. The paper was strongly criticized by Aebi [3] last year, and a further valid criticism of the paper and the trial it was based on is provided by Boszczyk [9] this year. Essentially, the amount of cement that was injected (2.8 ± 1.2) in the two trials that were reported was significantly less than would be required to effectively fill a lower thoracic vertebra. Essentially, the technical information provided by the NEJM publications was insufficient to prove or disprove the clinical efficacy of vertebroplasty.

Vertebroplasty remains an established method of treatment of osteoporotic fractures, but its role in trauma still remains somewhat controversial. The paper by Fuentes [16] presents a series of 18 patients with thoracolumbar burst fractures (Magerl A), treated by vertebroplasty of the fracture and then posterior fixation short segment screw fixation done percutaneously. The height recovery and kyphosis correction were similar to open surgery, but was less traumatic with a shorter hospital stay. The loss of correction—which occurred in the first 5 months—was only 2° , less than that reported by Palmisani [33] who reported minimal percutaneous fixation of these fractures without cement. It is of interest to compare this paper with that of Farrokhi et al. [14] referred to below. The paper by Pneumáticos et al. [36] asks the question as to whether when we do a vertebroplasty should we routinely do a needle biopsy to establish a tissue diagnosis. The answer he gives is a no, on the basis that preoperative investigations should establish whether the collapse is due to a malignancy or not. As the reason not to do it is cost, I am rather uncertain as to whether the cost of various investigations to fully exclude malignancy might not be as expensive as combining doing a biopsy with vertebroplasty.

Disc replacement

The management of severely disabling low back pain has been dominated in the last decade by the issue whether if surgery is carried out, it should be fusion surgery or total disc replacement. Some very responsible surgeons feel that disc replacement is the better treatment, and many equally responsible surgeons feel it is a step too far. We have had a number of systematic reviews of the literature to assess the role of disc replacement [15, 53]. The first by de Kleuver 2003 suggests that it is still experimental, the second by Freeman 2006 suggests that prospective randomized trials are still required to justify its use. This year we have a further and even more extensive review by van de Eerenbeemt et al. [52]. Their review of the literature is up to 2008. Their paper is very comprehensive, and their

conclusion is still that the existing evidence specifically regarding the long-term effectiveness and/or safety is insufficient to justify the widespread use of TDR over fusion for single level degenerative disease. Their review dealt with those devices available up to 2003, and the question must be whether the principle is misconceived, or the implants are at fault. It would certainly appear at present to be the case that those surgeons doing disc replacements should make their patients aware that its superiority to fusion is still a matter for debate. New devices may be more certain, but if used they should be part of a carefully controlled study.

The paper by Aunoble et al. [6] is a cohort study of 42 patients who had what is termed a “hybrid construction”, a fusion at L5/S1 and a disc replacement at L4/5. The results were good, and the authors recommend it as an alternative to a two-level disc replacement or two-level fusions, to avoid problems of sagittal balance and adjacent segment degeneration. The paper they quote to justify this approach by Jang [19] actually deals with the problem by doing a fusion to correct the sagittal imbalance produced by the first operation, with excellent results. The important message is to achieve correct sagittal balance when doing a fusion, and their unit is perhaps one of the most experienced in achieving this. It is a shame that they did not have a control group of two-level fusions, done with their particular expertise of establishing a correct sagittal profile, to allow them demonstrate the hybrid construction was indeed superior to a two-level fusion.

Fusion

Another “hybrid” concept was dealt with in the papers by Putzier et al. [38] showing no clinical benefit to “soft stabilization” of a degenerated segment adjacent to a fusion. This procedure was becoming a popular intervention, based on no scientific data, and one hopes that this paper and the associated biomechanical paper by Strube et al. [48] persuade surgeons that what was popularly called “topping off” has no value.

Since the introduction of disc replacement, the spinal community has concentrated on why fusion fails. In the last century, it was commonly considered that failure was related to failure to fuse, and hence for some 30 years we concentrated on improving methods of fixation, combined with some attention being given to bone biology and hence the development of bone morphogenic protein. Now failure is ascribed to insufficient attention to achieving sagittal balance, not to mention the new ogre of adjacent segment degeneration, hardly ever mentioned in the last century. The paper by Roussouly [42] although dealing mostly with consideration of sagittal balance in ankylosing spondylitis

does introduce the reader to the concept that it is the overall balance of the spine that has to be assessed, and that our predilection in the past for regarding lumbar lordosis as being most important has to be revised. Readers of this article who do not operate on ankylosing spondylitis can cull from the paper the basic facts about overall sagittal balance, and the use of full length lateral X-rays to assess this, and apply them to fusion and disc replacement surgery.

The review article dealing with image-guided spine surgery [50] came to some disappointing conclusions. Of the 276 papers they reviewed, there were only two RCTs and one meta-analysis. There was a lack of evidence of clinical benefit for pedicle screw insertion in the most critical area, the thoracic spine. Although technical development has meant that a very high degree of accuracy can be achieved, the failure seems to be related to the interaction between surgeon and the navigation system. Considering the fact that image-guiding techniques became available in the mid-1990s, progress in their clinical application has been somewhat disappointing. The authors make an important point that as regards their use in pedicle screw insertion, we cannot afford a situation to arise where the surgeon is not a master of traditional methods, as on occasion navigation systems do fail, and the surgeon has to have the expertise to recognize this and use anatomical landmark-based pedicle screw insertion. However, they point out those guidance systems may have an important role in education of surgeons, in training spatial awareness skills, and it is in this area that the authors feel that more progress can be made.

A rather more dismal picture is painted by another review article dealing specifically with computer-assisted spinal pedicle screw placement. They reviewed 23 studies, including 5,992 screws, and concluded that “Navigation does not show statistically significant benefit in reducing neurological complications and there was insufficient data in the literature to infer a conclusion in terms of fusion rate, pain relief and health outcome scores”. However, they do say that this rather gloomy conclusion could be unduly pessimistic, and further randomized controlled studies which addressed validated patient-based outcome measures may provide better evidence of the value of the technique.

Spinal trauma

The paper concerning spinal injury classification by van Middendorp et al. [54] is not an easy read. However, it repays study. It is in part a response to the fact that classifications now have to embrace the fact that CT imaging rather than plain films is an essential part of the investigations used to assess a spinal injury. However, I was

surprised to note that they did not discuss the place of MRI scanning. I was also surprised that they felt that “given the current evidence and diagnostic imaging technology, descriptions of the mechanisms of injury and ligamentous injury should not be included in spinal injury classification”. It will be of interest to see if their concepts do in time produce a classification system relevant to surgeons treating fractures, and to journals publishing results of treatment.

The paper by Farrokhi et al. [14] deals with the concept that one should include the fracture level with pedicle screws in doing a short segment fixation of a thoracolumbar fracture. Although frequently done by surgeons on the Continent, in the United Kingdom, there is a prejudice against this, and I hope this paper will have a significant clinical impact. It is increasingly recognized that those fractures with a load sharing score more than 7 (Thoraco Lumbar Injury Classification Score-TLICS [51]) need an anterior approach, but more stable fractures are well treated by a short segment stabilization. The inclusion of the fracture level in the construct confers considerable benefit as this paper shows in preventing implant failure and kyphosis. Of particular interest was the particular benefit gained in Type C fractures (those with a rotational element) by including the fractured vertebra in preventing kyphosis developing. The implant failure seen in “non-included” cases of 21% compared with implant failure in “included” cases of just 5% is impressive.

The paper by Reinhold et al. [41] is reporting the results of a second internet-based multicentre study (eight participating units), of 733 patients with acute thoracolumbar injuries treated surgically between January 2002 and January 2003. It provides a snapshot of German and Austrian surgical practice in relation to these injuries. One object of the study was to assess if much had changed since the last similar review a decade ago, so it is an audit rather than a research paper. However, it is of interest that combined procedures (front and back) were used more often in this decade, in some 47% of patients. However, although producing better radiological success, this was not reflected in better clinical results. It was of interest that 2 years after the injury, some 21% were recorded as having achieved full back function, and this was more likely if a posterior approach only had been used. However, the information given does not allow the reader to assess whether the Compression Type A fractures treated by the combined approach were in fact worse fractures (A3), and so did less well.

Spine infection

We have an ageing population, diabetes is becoming commoner, back pain is common, and we are now

permitted to make a diagnosis of non-specific back pain, meaning back pain that we have not diagnosed the cause, and so the risk of missing spinal infection increases. The paper by Yoon [56] reassuringly lets us learn that although one should strive to find the infecting organism, which they only identify it in 75% of cases, patients can be treated successfully with what they describe as “antibiotics selected according to etiological setting”, which in their case were cefazolin or vancomycin, and the values of CRP and ESR were reliable guides of successful recovery.

The paper by Rajasekaran [40] dealing with the surgical treatment of severe post tubercular spine deformity using a single stage closing–opening wedge osteotomy was an important contribution to the surgery of tuberculosis. Although clearly difficult surgery, it will surely displace the transthoracic methods pioneered by Hodgson [18] in Hong Kong in the last century, certainly in patients with compromised pulmonary function.

Cervical spine

The paper dealing with C5 palsy after decompression of the cervical spine by Hashimoto et al. [17] raised the interesting possibility that this complication was in fact not due to root injury, but was a cord injury. In the past, when it occurred with a posterior decompression it was thought that the cord drifted backwards, and the C5 roots came under tension. But in this series, an anterior decompression had been done which would not be associated with backward migration of the cord. MRI studies identified changes in the cord, indicating pre-existing asymptomatic damage of the anterior horn cells. Such preoperative findings should alert the surgeon to this risk, and thus allow him to warn the patient of the possibility of this otherwise unpredictable complication, which occurred in some 8% of this series of 199 patients.

The value of carrying out a spinal decompression for chronic myelopathy, when the patient is in a wheelchair or bedridden, is often debated. However, Scardino et al. [44] show that such surgery is well worth while. Out of 55 patients operated upon, 9 were in wheelchairs, or bedridden, and six of these after decompression improved sufficiently to be able to walk again. Patients with longer lengths of disease had worse outcomes overall. High intensity signal change in the cord was not predictive of failure, but obvious cord atrophy was.

Low back pain

The review article by Koes et al. [20] sets out to present and compare the content of the various guidelines issued in

some 13 countries, and 2 international guidelines. Consistent features of acute low back pain were the early and gradual activation of patients, discouragement of prescribed bed rest and the recognition of psychosocial factors as risk factors for chronicity. For chronic back pain, consistent features of management included supervised exercises, cognitive behavioural therapy and multidisciplinary treatment. I noted that the Australian and New Zealand guidelines do not distinguish between non-specific low back pain and radicular syndromes, and in the UK it is also clear that this distinction is commonly not made. As a result many patients with a radicular problem are treated initially with physiotherapy, which may be continued for many months, and as the paper quoted above Silverplats et al. [45] showed a long period of disability before surgical treatment of a disc protrusion was associated with a poor result. It does seem strange that such a specific diagnosis as root compression should be lumped in with a diagnosis of non-specific low back pain.

The role of disturbed sagittal balance in patients with a disc herniation is covered in the paper by Endo et al. [13]. Patients with a disc herniation have disturbed sagittal balance, which improves after surgery. The authors suggest that some of the back pain patients experience is due to this. It would certainly suggest that physiotherapy after surgery should be directed to dealing with a continued disturbance of sagittal balance.

The paper by Poulain et al. [37] dealing with the value of a functional restoration program in getting patients back to work suffered from the fact that there was no control group. Although the mean time off work prior to intervention was 14 ± 19 months, patients who had been off work more than 1 month were accepted onto the program. They do state that if someone is off more than 6 months then they have a worse prognosis, suggesting that in this group functional restoration programs may not work, yet this is precisely the group that is most in need of an effective intervention [52].

The paper by van Hooff et al. [53] is an important paper, and is more reassuring concerning the value of a rehabilitation program. The program is an intensive one, which as well as physical rehabilitation has a cognitive behavioural program, and achieves impressive results, assessed at 1 year after treatment. The intensity and the length of the program are important. The authors establish that this program is a real alternative to fusion in many patients. It is to be hoped that this paper encourages the development of such programs, especially in the United Kingdom.

The significance of Schmorl's nodes has been a matter for debate for many years. And the paper by Dar et al. [12] provided some further insight into an examination of some 3,000 vertebrae in skeletons. The authors conclude that these lesions, predominantly on the lower surface of the

vertebra, more common in the lower thoracic and upper lumbar vertebrae, were probably associated with vertebral development in early life, and were due to the nucleus pulposus pressing the weakest part of the endplate, and related to torsional and loading of normal life, and did not represent an injury.

From the wealth of papers in this year's journal, it is inevitable that those I have selected leave out many excellent papers. My selection is a personal one of those that have interested me. Perhaps, however, in looking at the ones I have found interesting, the readers may accidentally read adjacent papers, which excite them.

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