The Michel Benoist and Robert Mulholland yearly *European Spine Journal* Review

A survey of the “surgical and research” articles in the *European Spine Journal*, 2012

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Introduction

It is a great pleasure to once again have the opportunity of drawing attention to readers of the *European Spine Journal*, papers and reviews that I have found most interesting during the year. Inevitably such a selection is a very personal one, and many very excellent contributions will not be mentioned. The *European Journal* is now truly international. Some 136 papers are from Europe, but 147 are from the rest of the world. Every European country is represented. Although China (35), USA (27) and Japan (34) dominate the non-European contributions, the remaining 51 papers are from 16 other non-European countries. EuroSpine—the Spine Society of Europe is very committed to education with the hope in time that a spinal speciality will be recognized as an independent specialty in Europe, and the Society will create a Diploma, which

aspiring spinal surgeons will aim to acquire. The *Journal* by being so international will play an important role in the educational process.

Review articles

During the year there have been 28 review articles. These are an important part of the educational role of the *Journal*. I believe them are all worth reading, as so many deals with subjects affecting our decisions about management of patients.

The review by Benoist et al. [8] deals with the evidence for the value of epidural injections for the treatment of low back pain with radiculopathy. It draws attention to the fact that the strength of evidence of efficacy of any treatment depends on a close correlation between clinical experience and the data of evidence-based medicine. Clinicians treating patients with low back pain and radiculopathy are convinced of their value as evidenced by the fact that for example in France 65 % of centres treating such patients regard such treatment as an important part of their care.

However, this positive view of epidurals was not supported by Cochrane type reviews in the mid-nineties. What Benoist et al. draw our attention to is that the great variety of ways that results are recorded in randomized trials and how this affects the conclusions. The paper is instructive not only because it does indicate that epidurals are of value, but the wider discussion of interpretation of evidence from prospective trials is of importance and in looking at any prospective trial we should carefully assess the methodology before fully accepting the conclusions. The review by Quraishi [39], which follows the above review, dealing with transforaminal injections of corticosteroid for lumbar radiculopathy gives two clear messages to me. They do work, but it is the anaesthetic or saline, and not the steroid.

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That is effective. Both papers relate the rare but serious complication of intra-arterial injection of steroid, occurring particularly when the foraminal route is used, especially in a previously operated patient, with scarring in the foramen, causing the serious complication of a spinal cord infarct, producing paraplegia. Clearly if the steroid is non-essential for a satisfactory result, then in the future we should only use anaesthetic and saline. As it is those steroids that are particulate that are incriminated, if we do feel we should use steroid, use the non-particulate ones.

The review by Mordecai et al. [35] dealing with the efficacy of exercise therapy for the treatment of idiopathic scoliosis identifies a common problem with literature reviews, a paucity of acceptable studies. In their review out of 155 papers dealing with the subject, only 12 were deemed to be relevant. Five of the ten prospective studies were by the same authors (one study had been published in two journals), who were also affiliated to a unit that heavily endorsed exercise therapy. The conclusion of the authors was that there was poor quality evidence supporting the use of exercise therapy in the treatment of AIS, and there was a need for well-designed controlled studies.

In the review article by Steiger et al. [51] the question is asked “Is a positive outcome after exercise therapy for chronic low back pain contingent upon a corresponding improvement in the targeted aspects of performance?” The answer was that it is not, that is the treatment effects of exercise therapy in chronic low back pain are not directly attributable to changes produced in the musculoskeletal system. They review the other effects that exercise may have, ranging from changes in the central nervous system, modification of motor control patterns, psychological effects, down to a positive therapist–patient relationship. I suspect that many of us have always doubted the direct relationship between improvement in muscle function and movement and clinical improvement, but the authors point out those targeting perceived physical deficiencies are expensive and time consuming, if in fact more direct interventions, for example strategies for retraining the cortical function might be appropriate. The importance of cognitive training alluded to below is surely part of this approach.

The two review papers by Aldabe et al. [2, 3] concerning pelvic pain in pregnancy were intriguing. We learn that it affects 20 % of pregnant woman, and they are three times more likely to get a post-partum depression. Yet in the UK they are not seen in orthopaedic departments, except the very few whose symptoms persist after delivery. The first paper finds that the disorder cannot be blamed on excessive amounts of relaxin hormone; the second paper suggests that there is a mechanical basis for the pain. Unfortunately the article does not make it clear whether exercise therapy during pregnancy would be of value. In the few patients I have seen post-partum, the pelvic pain is very much load related, and unilateral, and rather

different from the low back pain described in the paper following the above by Yoshihara [62], which deals with sacroiliac pain after fusion. Here the presumed sacroiliac pain is very much low back pain. If indeed the pregnancy-related pelvic pain is from the sacro-iliac joint, it is curious that the clinical features of so-called sacroiliac pain after fusion or indeed after lumbar disc replacement are so different. Siepe in 2008 [47] investigated 175 patients with back ache after disc replacement, by using local anaesthetic injections, and found that in 12 % the pain was from the sacroiliac joint. However, the pattern described was quite unlike the pelvic pain of pregnancy. Yoshihara does not quote the Siepe paper, but addresses in some detail the results of studies in which anaesthetic is injected into the SIJ, and notes that the suggested incidence of SIJ pain after lumbar fusion varies from 16 to 43 %! As one of our pan problems is how to diagnose sacroiliac pain, it is disappointing that as the pregnancy pain is so different, it is difficult to believe the same structure is the cause of the pain.

Review articles, which guide one very clearly in the management of difficult problems are always welcome, and the review by Quraishi [40] is one such, dealing with the management of metastatic tumours in the sacrum. The mainstay of treatment is palliative, and it is of interest that sacroplasty (injection of cement into lesions) is so satisfactory in those patients without neurological compromise. The authors give valuable guidance on which may be an appropriate surgical solution, and they outline the complications that may occur, especially important in any palliative procedure.

The review article by Park et al. [37] deals with the management of vertebral artery injury in anterior cervical spine operation, and the comment by Bartels [6] again gives valuable advice as to what to do if this catastrophe occurs. Their flow diagram is very clear and memorable. I rather disagree with the comment by Bartel that it is unlikely that a surgeon would recollect what this paper says in the drama of the event. However, his comment concerning the need to routinely look at preoperative scans and know if the artery is likely to be at risk is important, as is the advice concerning the use of a high speed drill, which was associated with vertebral artery injury in 59 % of the 39 cases described.

Supplements

There are five supplements, consisting of three national supplements, one supplement concentrating on one topic, and one containing all the papers at the annual meeting.

Italian supplement (Supplement 1)

This supplement contains a selection of papers, all from centres in Italy, as the Third Special Issue of the Società

Italiana di Chirurgia Vertebrale G.I.S. The Editorial by Lamartina [32] dealt with the fact that in Italy spine surgery was becoming a neurosurgical speciality, and he foresaw that in time orthopaedic surgeons would no longer be involved. It was of interest that he did not advance the cause of a specific speciality involving both disciplines. I noted that in this supplement of the 25 papers only two were from neurosurgical units, understandable as these were papers from an orthopaedic meeting, but does indicate that our neurosurgical colleagues in Italy may be poorly represented in our Journal.

British Isles (Supplement 2)

This was a judicious mixture of the abstracts of papers to be presented at Britspine (May 2–4, 2012, Newcastle) and a number of original papers. It is a first for the Spine Societies of the British Isles, and will ensure, as Sell and Boszczyk state in their excellent Editorial, that the range and breadth of research in spinal disorders undertaken in the UK will be now readily available throughout Europe, “bridging the channel” as they describe [10].

Papers presented at Spineweek in Amsterdam (Supplement 3)

This is a most valuable supplement, especially to those of us who did not attend the meeting.

The science of intervertebral disc replacement (Supplement 5)

This supplement in the main dealt with the biomechanics of disc replacement, evaluating the effect of design on segmental stiffness, and patterns of movement after disc replacement, compared with segmental function with a normal disc. The editorial by Wilke et al. [60] very fully outlines the background. The various papers demonstrated that there was much variation in the biomechanics of various replacements; an artificial disc could produce more stiffness, or more mobility of the segment. As similar clinical results appear to occur irrespective of what is achieved biomechanically, one wondered whether clinical success was related to factors other than the particular biomechanical parameters examined. I was disappointed that there was no examination of the load transmission of the various implants.

Swiss supplement (6)

The editors draw our attention to the great contribution surgeons and basic scientists working in Switzerland have made to our speciality [19] and the supplement illustrates

this with its selection of both basic science and clinical papers. I have elected to discuss some of these papers under the subject headings below.

Original papers

Thoracic disc herniations

In the April issue there were a number of articles dealing with thoracic disc herniations. Although only representing 5 % of all disc herniations, because they may involve cord function, and lead to irreversible serious cord injury, their diagnosis and management are of vital importance. In the past, approach by laminectomy produced such unsatisfactory results, that trans-thoracic approaches came to be used in the 1990s, and the development of thoracoscopic approaches followed from this. The paper by Coppes et al. [14] and the comment by Mehdian [34], the paper by Smith et al. [48] and the paper by Quint et al. [38] and Russo et al. [44] all discuss different approaches to their management.

Coppes et al. describe a transdural approach, in which the cord is rotated and lifted somewhat to gain access to the back of the disc, the division of the denticulate ligament being critical in achieving this degree of mobilisation without cord injury. He reports results on 13 consecutive patients operated on between 2004 and 2010, 6 improved, and 7 remained the same neurologically (Frankel scale). His paper produced a very strongly worded response from Mehdian [34], who regarded the technique as “perilous” and one that should not be recommended, as it violated the basic principles of spinal surgery. It would certainly appear to be a method that should only be used by surgeons regularly working within the dural space, but may be safe if the surgeon has that experience, but is perhaps not appropriate for “giant” protrusions. There are now available alternative approaches, which on the face of it are safer, as that described by Russo et al. [44]. Whereas in the above Coppes series of 13 only half were “giant disc protrusion”, that is more than 40 % canal encroachment, in the Russo series all were giant protrusions, with three having a transdural component. The surgical technique is a mini thoracotomy and the use of the microscope. The authors describe the approach very fully and outline the aspects of the many other approaches that have been used. There was no neurological deteriorating in any of their patients. The paper by Quint [39] presents a group of 167 consecutive cases over 10 years. However, only 37 % had myelopathic symptoms (in the series of Coppes and Russo, all had myelopathies). Pain and root involvement were much more common, unfortunately the degree of canal occlusion is not detailed, so that the case mix of this series included a large number of disc bulges, associated with pain, and in some

cases a radicular component, rather than a myelopathy. One suspects that thoracoscopic approach might well be suitable for this group. A much more minimally invasive technique is described by Ji Young Cho et al. [13]. He describes an approach using a tube, and microscope, and going through the muscle to the postero-lateral area, mid-pedicular point on the annulus. Tubular approaches used for lumbar discs have little advantage, [5] but in this area they may be more applicable. Of the five patients the authors report, four had a myelopathy, and the illustrative case, whose MRI is reproduced had a very significant degree of canal encroachment.

Scheuermann's disease

In the last few years the development of spinal implants to correct spinal deformity has made it possible and safe to correct the deformity in Scheuermann's disease. The indication is partly cosmetic, and also the view that in later life, patients with Scheuermann's will experience significant back pain. And correction will prevent or reduce that risk. The paper by Ristolainen et al. [43] examines that belief. They sent a postal questionnaire to 80 patients, who had been seen more than 37 years ago, with back deformity, and not treated, and 49 of them had classic Scheuermann's disease. They compared their report of disability and pain to a matched sample of the Finnish population. They found that they indeed did have more pain and disability than the controls, but it did not interfere with their work. Difficulty in mounting stairs without stopping was a feature of these patients, or carrying a load >5 kg. Sixteen percent of patients had pain in the thoracic spine, and 31 % in the lumbar spine, and 29 % in the whole spine, 25 % had no pain. However, their disability was not related to degree of deformity. This paper is valuable as it does provide the surgeon, considering correction of a Scheuermann's deformity, information to adequately counsel the patient.

Spinal fractures

The paper by Rahamimov [41] concerning unstable thoraco-lumbar burst fractures describes the use of percutaneous pedicle fixation of the vertebra above and below the fracture, and augmentation with cement around the screws, and vertebroplasty of the fractured vertebrae. They report on 40 patients, the fracture levels being from T6 to L4. Apart from one B fracture all were A, 1, 2, and 3 fractures (AO/Magerl classification). The technique allowed immediate ambulation with no external support, there was a loss of correction in the first 3 months of an average three degrees (range 0–13). They used PMMA cement, but state that in recent years, they have used tricalcium-phosphate cement, in younger patients, on the basis that it is more

biological. The severe kyphosis deformity that occasionally occurs with what are thought to be simple wedge fractures but are in reality burst fractures, makes it all the more important to diagnose the nature of a fracture in this region, and consider this method of treating.

Vertebroplasty and kyphoplasty

The paper by Bornemann [4, 9], compared the results of radiofrequency kyphoplasty in 33 patients, compared with conservative treatment in 38 patients. The organization of the study was unusual. All patients had conservative treatment for 6 weeks, and then were offered the choice of surgery, 26 chose to have surgery, and then at 12 weeks, a further tranche of 7 selected surgery. One would have thought that those electing to have continued conservative treatment felt they were progressing well, and did not need surgery, so it was surprising that this group on assessment was so much worse with regard to pain than the surgically treated group. The reason why so many patients elected not to have treatment is not really explained.

The paper by Zuozhang Yang et al. [61] reports the results of combining vertebroplasty and chemotherapy in treating vertebral fractures due to myeloma. There was an immediate value in terms of pain relief, but it appeared that there was some benefit in terms of full and nearly full emission at a year. However, the statistical significance of this is questionable, and clearly a longer follow-up is required to ascertain whether there is any long-term benefit of the combination.

Disc replacement

Intervertebral disc replacement has now become one of the options available in treating back pain due to disc failure. However, intensive rehabilitation has also been shown to be an option. Disc replacement has not proved to be the dramatic success we had hoped, so the review article by Hellum [24] "Predictors of outcome after surgery with disc prosthesis and rehabilitation in patients with low back pain" is welcome. It was a prospective study to evaluate whether certain baseline characteristics can predict outcome in patients treated with disc prosthesis or with rehabilitation. This paper was not a comparison of the two treatments, but a study of the particular factors that would influence outcome once one had selected a patient for either treatment. It found that patients with a short duration of low back pain, with Modic changes, 1 or 2, at the level operated upon, and a low degree of fear avoidance (low FABQ-W) were the best predictors of success in patients having a disc replacement. They noted that Esposito et al. [17] had found that the results of lumbar fusion were better in patients with Modic 1 changes at the operated level, and

they suggest that removal of the disc as a pain source may explain the good results. They suggest that long continued back pain and high FAB-Q associated with a poor result represent the effect of chronic back pain in producing negative attitudes to recovery, adaptation to the sick role, and central sensitization. It is certainly one explanation of the fact that disc replacement is more successful in the younger patients.

I found the paper by Zweig rather curious [63]. The authors state that radiculopathy is regarded as a contraindication to the use of an intervertebral disc replacement, and they present figures to show that examining a registry of 577 patients with SWISS spine, patients with severe leg pain and a radiculopathy had done as well as those without these features, indicating that these features should not be regarded as a contraindication to disc replacement. Surely the fact is that patients with these features are treated very successfully by a local decompression procedure, usually involving the removal of a disc herniation, rather than the major intervention of a disc replacement. They may do equally well with a disc replacement, but it was an inappropriate intervention in the first place.

In recent years, papers reporting the results of disc replacement usually start with a brief paragraph on the history of disc replacement starting with the Fernstrom ball, and its failure [20]. It was therefore with some incredulity that I read the paper by Siemionow [46] that it had been licensed by the FDA in 2005 for use, that licensing being withdrawn in 2007 after its predictable failure once again. The relevance of this paper is that any nuclear replacement that does not tension the annulus under load as well as axial loading, and thus transfer some of this load to the hard cortical periphery of the disc, (designed to take load), is doomed to fail.

Lumbar fusion

The advocates of disc replacement, and its importance in preventing what they see as the great disadvantage of fusion, namely adjacent segment degeneration would find the paper by Turunen [54] of interest. It is a retrospective review of 106 patients operated upon between 1992 and 1997 using an instrumented posterolateral fusion the authors identified four groups, degenerative spondylolisthesis with spinal stenosis, adult listhetic spondylolisthesis, failed back syndrome after one to five discectomies, and failed back syndrome after one to three laminectomies. The success rate differed in each group, and the authors are to be congratulated on the clarity of their tabulated results. As one might anticipate the adult spondylolisthesis with stenosis did best, with a 20-point improvement in ODI, and 90 % excellent or good. The adult listhetic spondylolisthesis did less well, with only an 11-point improvement in

ODI, and 78 % excellent or good. One suspects that this is because the presence of an isthmic defect often concentrates attention to a segment that may not indeed be the symptomatic level. The post-laminectomy patients did better than the post-discectomy patients. The reoperation rate for ASD was 9 %. I note their study finished in 1997, the year that Thomsen [53] published his Volvo award winning paper that showed that there was no benefit in adding instrumentation to a posterolateral fusion. Sadly this message has not been accepted by many spinal surgeons.

Lumbar segmental fusion is a successful operation for low back pain in some patients, the problem is to predict, which patients will be most likely to benefit. It is generally agreed that it is some form of discogenic failure that is central to the cause, so we had had hopes that MRI scanning before surgery would aid us in identifying what form of disc failure was most predictive of a successful fusion. The paper by Djurasovic [16] addresses this question. They looked five characteristics, disc desiccation, disc contour, and presence of a high intensity zone, Modic changes, and disc height. They found that only disc height was predictive statistically. If it was more than 7 mm, then improvement in ODI was 9.2, and if less than 5 mm, the ODI improvement was 23.4. Similarly SF36 PCS improved 9.5 points in the more narrowed discs, and only 0.7 in that disc >7 mm.

Low back pain

Any doctor involved in the assessment of patients with low back pain is very aware of the issue of secondary gain magnifying disability. The distinction between malingering, that is conscious magnification of symptoms for secondary gain, or sub-conscious magnification, the “cry for help” that Waddell described, [58] is not fully resolved by the Waddell tests. The paper by Kumar et al. [31] adds three other tests, which have the advantage of being more easily part of a standard clinical examination, and are more clearly positive or negative than the standard Waddell manoeuvres. This is a thoughtful paper, and deserves careful reading.

Over the past few years there have been a number of important papers demonstrating that chronic low back pain (should we call it non-specific low back pain that does not get better?) can be treated either by fusion, or an exercise program and cognitive intervention with broadly similar results [11, 18, 21]. These papers had a follow-up of under 5 years, but the paper by Froholdt [22] provides a long-term follow-up of 9 years. Three facts stand out: Firstly that what improvement occur is in the first year in both groups, and there is little change in the next 8 years, secondly the improvement is modest (ODI from a mean of 45 to 25), and finally that if we look at the patient overall

rating, “How would you rate your back today?” there are more, poor, unchanged, considerable complaints and severe disability patients in both groups than there are excellent with no or unimportant complaints. In the surgery group, including also those initially randomised to exercises and cognitive training, which eventually had surgery (8 patients), there were 12 excellent patients out of 63, and 20 poor, unchanged and severe disabilities. However, there were only 2 patients rated excellent in the exercises group, compared with 31 poor, unchanged and with severe disability. The authors entitle the paper “No difference in the 9-year outcome in CLBP patients randomized to lumbar fusion versus cognitive intervention and exercises”, but the paper does not truly support that title. It would appear to be the case that although on the various parameters measured, there is no statistical difference overall, these averages mask the fact that more of the excellent and good results are in the fusion group. The value of exercises and cognitive training is assessed in the paper by Van Hooff et al. [55]. This paper is a follow-up of their paper in 2010 [56]. The first paper described the immediate success of a program of 100 h of contact time, exercises and cognitive training, and the paper this year established that at 2 years the benefits continue. The cohort is selected in the sense that they were all referrals for a program, and a surgical solution was never deemed appropriate. Before joining the program, they had to establish that they were fully motivated for it to succeed. Clearly this cohort of patients is quite different to the patients that Froholdt et al. describes above.

The value of Modic 1 changes as a prognostic guide to recovery in patients with non-specific low back pain was addressed in the paper by Keller et al. [29]. In a cohort of 269 patients followed for 1 year, 50 % had Modic 1 or 2 changes. Their presence was not predictive of recovery, so the attractive concept that the Modic type 1 changes were inflammatory, and therefore more likely to settle is not supported. This paper and the one alluded to above by Djurasovic et al. [16] indicate that the value and significance of Modic changes are not fully understood particularly in chronic low back pain. However, the paper by Hancock et al. [23] does confirm that MRI findings were five times more common in selected patients with acute back pain than controls without back pain. The value of this paper is that the patient group was selected carefully to ensure that the likely source of the pain was discogenic, the pain was acute and it responded to centralization manoeuvres, generally accepted as being most typical of discogenic pain. This explains why the numbers in the study are small, 30 in each group. The authors make the telling point that in chronic low back pain there are likely to be many patients who have higher levels of centrally generated or enhanced pain, making it difficult to identify

markers of local pain producing pathology. Modic changes were also looked at by Sorlie et al. [50]. Assessing the influence of Modic changes present at the time of micro-discectomy on the patient’s recovery from back pain in the year following surgery. Their finding was of great interest, as although the presence of Modic type 1 changes did mean recovery from back pain was less, smoking had a greater effect in producing continued backache during the year following surgery.

“Non-specific low back pain” has become a “diagnosis” that is accepted, despite the fact that it is no more than a statement that the patient has back pain, we do not know the cause, but we think that it is not serious and it is likely to get better. However, the problem facing the clinician is if recovery is slow, then at what stage should more investigations be done? In those health systems where the state is responsible for the costs of investigations, there is pressure upon clinicians not to investigate too early, or often not at all. Unfortunately on occasion the diagnosis of serious disease-infection or tumour is delayed as a consequence. MRI imaging had the great advantage that it was non-invasive in terms of radiation, and is known to be sensitive in diagnosing infection and tumour. The review articles by Wassenaar et al. [59] and van Rijn [57] concern the value of MRI and CT in the diagnosis of lumbar spinal pathology in patients with low back pain or sciatica. Both papers highlight the deficiencies of these investigations, especially when they are separated from the associated clinical picture. It has long been recognized that spinal stenosis and root entrapment is a clinical diagnosis, and imaging is used to support the clinical diagnosis, and identify site and severity of the compression. In pointing out the inadequacies of these investigations in lumbar spinal stenosis with radiculopathy, the authors suggest that their role in detecting other serious pathology should be evaluated by further “high quality accurately reported studies”. It would be unfortunate if these reviews were as a result used to delay the proper investigation of persisting so-called non-specific back pain.

Lumbar disc protrusion

McKenzie treatment for acute disc protrusion is a well-established therapy. The paper by Albert et al. [1] asked the question whether the phenomenon of centralisation (and peripheralisation), which occurs in some patients as a response to repeated movement and positioning is associated with outcome or types of disc lesion. The value of this prospective study of 176 patients treated by the McKenzie technique was that they all had pre-treatment MRIs, so the type of disc was known. The study indicates that response is not related to type of disc protrusion (contained or non-contained or sequestered), but response is related to

outcome, that is those who do respond, are those patients who improve most. Those who did not respond had a less favourable outcome. Indeed the authors suggest that non-responders may have pain that is non-dismal in origin. This paper should be read in conjunction with the paper by Sheets et al. [45]. In a prospective randomized trial comparing McKenzie treatment with advice to remain active, take paracetamol, and reassurance that the outlook was favourable, the two groups were the same at 3 weeks, and that six features that are considered to indicate a response to McKenzie therapy were not predictive of a response. However, this study excluded patients with nerve root compromise, presumably the leg pain was referred rather than related to root compression. The predictive features they chose: pain worse on flexion, pain worse with movement, non-radicular leg pain and constant pain, no mention of restricted straight leg raising are not features of a disc protrusion, so that what their study appears to show that for acute low back pain and referred leg pain, the McKenzie treatment is no better than advice, activity and analgesia.

The paper by Jacobs et al. [27], which looked at the variety of surgical techniques used to treat a disc herniation, found that there appeared to be no clinical benefit of microscopic discectomy, apart from shorted scar, and a longer operation. There was no clear benefit of the tubular methods. However, they admit that their review conclusions are uncertain, due to limited amount and quality of evidence.

Spinal stenosis

One is always attracted to papers that validate a clinical examination, which is of value in diagnosis, and the paper by Chang-Hoon Jeon [28], which validates the ankle-brachial index as a useful test in patients with atypical claudication, that is in patients with degenerative changes in the spine and peripheral vascular disease. Of thirty-three legs with a low ABI (ABI <0.9), 29 had peripheral vascular disease as their cause of claudication. However, if the ankle-brachial was higher, then of 29 legs, 24 did not have peripheral vascular disease causing their claudication. That is in the differential diagnosis of peripheral vascular disease as a cause of claudication, ABI was 85.3 % sensitive and 85.7 % specific; I suspect that despite the value of this test in the clinic, CT evaluation of the spine would be done, but the need for angiography would be reduced in those patients with a low ABI.

The spinal surgical community is now starting to get the benefits of the development of Registries, and the paper by Sobottke [49] using data from the Spine Tango registry is a good example. It reviews the complications of operating on patients with spinal stenosis, and assessing their relation to

age in 1,764 patients in the registry operated upon between 2005 and 2010. General complications were more frequent in the elderly group (over 75 years), but not surgical complications. Older patients are less likely to be fused, than younger patients, but if fusion is done, then age does not affect outcome. The fact that age has so little effect on surgical complications may well reflect the selection process that any surgeon will use, and the techniques used. They quote the paper by Thome et al. [52]. The rate of complications after bilateral decompression (5 %) was less than a unilateral decompression (17 %) or laminectomy (22 %). It is clear that if the disability and imaging findings are such that decompression is appropriate, then age alone of patient should not deny them surgical treatment.

In 1991, Herkowitz [26] reported a small group of 50 patients, with degenerative spondylolisthesis, 25 of whom were decompressed fused, and 25 just decompressed. The fused and decompressed group did significantly better. The paper by Kleinstueck et al. [30] covers the same ground, somewhat more comprehensively. Confirming that degenerative spondylolisthesis is best both fused and decompressed, even though the main symptoms may be predominately leg pain and one might be tempted to decompress alone. It was unfortunate that the paper by Herkowitz was thought to imply that all spinal stenotics with leg pain should be both decompressed and fused, but it is important to emphasize that it is the spondylolisthesis patient that requires both.

Odontoid fractures

Last year Pal et al. [36] discussed the management of Type II odontoid, especially the unstable type II B, and concluded that the results of non-rigid (firm collar) were satisfactory in the elderly population. This year the subject is revisited again by Hénau et al. [25]. They advocate screw fixation, despite the fact that in their 9 patients out of the 11 operated upon (two died from unrelated causes), only 5 patients had an excellent outcome, the remaining living 4 patients had a good outcome (2), a fair outcome (1) and a poor outcome (1). However, five of the nine had only a fibrous union, only four a bony union. Unfortunately the authors do not relate the state of union with result. They quote the paper by Crockard [15] describing the development of myelopathy in 16 patients with odontoid fractures. However, these were all unrecognized or untreated fractures, in some the injury had not been recalled. Hénau very rightly describes the hazards of halo-immobilization, but do not discuss the alternative use of a firm collar. Their paper does not convince me that anterior screw fixation should be offered as the primary treatment of this fracture in octogenarians. I feel the view of Pal et al. last year is more persuasive.

Tuberculosis

Nearly 3 % of patients with spinal tuberculosis develop severe kyphotic deformity, which needs correction. A number of these patients develop late onset neurological deficit. Anterior decompression was thought on the past to be the approach of choice to deal with the cord compression, but is less effective in correcting deformity, and proved to be unpredictable in improving the neurological deficit. The paper by Basu et al. [7] dealing with neurological recovery after a posterior approach alone using a transpedicular decancellation osteotomy reports on 17 patients in all of whom the neurological deficit started more than 2 years after the lesion had healed, that is they were all chronic late onset cord compromise. The longer the deficit had been present, the less satisfactory the outcome. However, all but one patient did improve, either one or two grades on the ASIA scale. This improvement mostly occurred in the first 3 months. It is of interest that pre-operative MRI imaging is not of value in predicting the degree of recovery. The technique is well described, and these results, similar to those reported by Rajasekaran in 2010 [42] are such that trans-thoracic anterior decompression would seem no longer appropriate, as not only are the neurological results as good, but the degree of correction achieved is much better doing the above approach.

A large psoas abscess can be present, without much or any bony destruction, and needs to be drained, although the underlying disease can be very satisfactorily treated medically. The paper by Buyukbebeci [12] describes the technique of retroperitoneoscopic drainage of a tuberculous psoas abscess, reporting its use in 12 patients. Open drainage is a major procedure, and this technique, which the authors describe, clearly would seem a very valid alternative, used by surgeons competent with endoscopy.

Using a thoracoscopic technique, but combining it with a mini-open approach in treating spinal tuberculosis in the thoracic spine, with deformity, and bone destruction is described in a paper by Lu [33]. They describe the procedure and the results in a cohort of 50 patients, with a mean follow-up of 6.5 years. They show that the use of graft and metallic implants to maintain correction achieved is safe. Solid bony fusion was achieved in all cases by 18 weeks. Lung lacerations were the only serious complications, all treated satisfactorily with no long-term affect on pulmonary function. For surgeons with the requisite endoscopic skills, this is an appropriate alternative to an open thoracotomy, but the complications and results seem similar.

One is always conscious in doing this review of the necessarily personal nature of selection, and the need to leave out so many excellent papers. One only hopes that readers will go to the papers I have selected, and then be tempted to read adjacent papers in the same journal.

Conflict of interest None.

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