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REVIEW ARTICLE

The Michel Benoist and Robert Mulholland yearly European Spine Journal review: a survey of the "surgical and research" articles in the European Spine Journal, 2018

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Introduction

Once again, I have the privilege of doing this annual review of the surgical and research papers published in the European Spine Journal. The Journal continues to be in the forefront of educational activities in both what it publishes and the many other educational activities it promotes.

Low back pain

The January issue contained three papers that complemented each other. The paper by Stochkendahl et al. [1] concerning National Clinical Guidelines for the non-surgical treatment of recent low back pain or lumbar radiculopathy from Denmark, but very much reflecting the NICE UK Guidelines. What was surprizing was the lack of evidence for those guidelines, despite extensive reviews of the literature. They advised staying active, quoting two studies which showed a small statistically significant effect. They advised education, but restricted it to patients who were motivated and based on patient centred dialogue. They felt that the evidence did not support early imaging and rejected this on the basis there was radiation (not so with MRI scans) and likelihood that the clinician would often label patients with a diagnosis that alarmed them, surely a failure of communication. They also suggested that the use of pain medication, both for back pain and radiculography was unproven, which I find difficult to accept and must question the evidence on which this conclusion was based.

The paper by Zehra et al. [2] concerning our understanding of end plate changes was a very comprehensive review of the multitude of endplate changes, very beautifully illustrated, yet a marked difference in opinion amongst very experienced surgeons and radiologists as to their clinical significance. In the circumstances as clinicians themselves are uncertain about the significance of changes, explaining them to patients represents a difficulty. The editorial by Gunzburg [3], concerning "Structural vertebral endplate nomenclature and etiology: a study by the ISSLS Spinal Phenotype Focus Group", emphasized the significance of this paper and the need for further research. However, the paper by Karran et al. [4] dealt with the reassuring value of imaging to patients, if careful thought is given to how this is done. The authors developed a specific psycho-education programme and demonstrated its value to patients. They made a strong case of availability of imaging to primary care providers.

The paper in the March issue by Morris et al. [5] dealt with the value of osteopathic treatment of chronic back pain, acute back pain, neck pain and other varieties of axial pain. Using data derived from the Spine Tango registry, although it provided a wealth of statistical figures, obscured by the use of acronyms it relied on patient satisfaction in the main

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for assessing outcome. It did accept that if patients had an ODI of over 60, then results were poor. It made no reference to whether drug usage was reduced. The author's final comment that the data could not be used to draw conclusions about the value of osteopathy in chronic spinal pain was a realistic summary of the paper.

The May issue provided a number of papers dealing with low back pain. The paper by Fischgrund et al. [6] reporting a prospective randomized double-blind trial with a sham component of intraosseous nerve ablation in the treatment of chronic low back pain was of great interest to me personally, who have always believed a pain source in so-called mechanical low back pain was vertebral, related to abnormal loading patterns on the endplates. The study showed that indeed there was a 20% decrease in the ODI in the treated group, as compared with a 15% decrease in the sham group. Perhaps the most interesting feature in this paper is the analysis of the high placebo response. The authors argue persuasively that the presence of a significant placebo response does not indicate that the true therapeutic value of the procedure is less valid. The intervention is not a simple procedure; a trocar has to be introduced into the centre of the vertebra to target the basivertebral vessels and nerve. It would have been a useful addition to the paper to shows the anatomy of the neurovascular bundle. Their discussion of the evidence that chronic back pain is from the vertebra rather than discogenic was very well supported by a review of the literature. What was of interest is that the improvement held up in both the sham group and the treatment group. Both held up for the year of follow-up. They point out that their results are similar to those achieved by surgical treatment. The award-winning paper by Mannion et al. [7] draws our attention to the poor results of spinal surgery as compared with surgery for knee and hip arthritis. She entitles her paper "Time to remove our rose-tinted spectacles". Those of us who did both hip and spinal surgery have been well aware that spinal surgery was less successful than hip surgery and agree that we need further research to strive and improve results, but it is unfortunate that this disparity of results is used by funding authorities to deny spinal surgery to patients who will benefit.

Two papers by Yabe et al. [8] and Szita et al. [9] dealt with risk factors for spinal pain in children and adolescents. Both incriminated sitting for long periods playing computer games or watching TV. Interestingly, sitting at a computer, presumably in school, was not associated with back pain, presumably due to better-seated positions. They did not mention weight or other aspects of lifestyle such as sport, but the implication is that lazing about watching TV and playing computer games is bad for ones back, supporting the view that I suspect many of us hold already.

The paper by Ludwig et al. [10] reported on the incidence of back pain and leg pain in the elderly and its effect on their quality of life in three thousand and thirty-three Swiss people over 65 living in the community. The overall incidence was 29%. If this was associated with leg pain, there was a very significant effect on mobility. However, if there was no leg pain, then back pain alone had little impact on self-rated health and health utility measures.

Spondylolisthesis

The retrospective review by Kim et al. [11] of a comparison of TLIF (an interbody fusion procedure) with PLF—a posterolateral fusion alone in the treatment of degenerative spondylolisthesis demonstrated no difference, other than the surgeons doing the interbody procedure were paid more. Unfortunately, the paper does not say, whether in doing the PLF a decompression was carried out, one presumes it was. I have always been surprized that an interbody fusion relieves the L5 root pain as the compressing agent is the superior facet of L5 having migrated down the pedicle, and this anatomy is not altered by reduction. I presume that the fact that movement stops is the critical factor. As interbody fusion is now so commonly used for degenerative spondylolisthesis rather than a posterolateral fusion, this paper must raise questions about this trend.

Degenerative spondylolisthesis producing symptomatic spinal stenosis is commonly treated by spinal fusion in addition to spinal decompression. However, recent studies have shown that a similar clinical outcome is achieved by decompression alone. There may, however, be a subset of patients is such that decompression alone may tip the balance and allow further slip. The award-winning paper by Dombrowski et al. [12] reports on the use of dynamic imaging in degenerative spondylolisthesis, which identities a group with a degree of aberrant motion, which would not have been seen on current static imaging. Unfortunately, the complexity of the imaging procedure makes it unlikely that it would be used in clinical practice. However, it validates the decision to fuse in combination with decompression, especially in the younger patient.

The natural history of spondylolisthesis and spondylolisthesis was well described by Fredrickson et al. [13], but the paper by Lemoine et al. [14] adds further to our knowledge. They reviewed CT scans taken of young children for other reasons and identified the presence of spondylolysis, both unilateral and bilateral in these children. They confirmed that the incidence rose during the first 6 years of life, the incidence being 1% in children under age 3, 3.7% in children under 6 compared with 4.7% in the adult population. There was a suggestion from their radiographs that a high pelvic incidence played a role, but this was a weak association. It is likely that the abnormality is a stress fracture related to the upright posture and walking, but certainly, it is an acquired

abnormality. I was surprised that the authors made no mention of the Fredrickson paper.

The management of high-grade spondylolisthesis remains controversial. The paper by Ferrero et al. [15] reporting long-term results of reduction with sacral fixation and dome resection and correction of sagittal imbalance in 20 patients is impressive, if achieved at the cost of one patient permanently on crutches and with loss of bladder control, and overall 70% had neurological complications, which recovered apart from the patient detailed above. The main benefit over less extensive procedures (fusion in situ, anterior fusion with modest reduction of kyphosis) would appear to be the restoration of sagittal balance, a somewhat French obsession. The ability of the body to compensate for abnormalities in sagittal alignment is significant, as indicated in the paper by Bao et al. [16].

Disc replacement

The most important aspect of the paper by Furunes et al. [17] reporting predictors for long-term outcome is the importance of selection. The probability of employment at 8-year follow-up of a patient with more than 1 year of sick leave, an ODI of more than 50, and no higher education was 1%, whereas if the patient had less than 1 year of sick leave, an ODI of less than 50, no co-morbidities, and had received higher education, then 87% were at work. The presence and extent of Modic signs were also predictive of a good long-term result. One presumes that the presence of Modic signs indicates that the disc being replaced is at the symptomatic level.

The paper by Plais et al. [18] reporting on the long-term results of the Maverick prosthesis also highlights the significance of Modic changes predicting a better outcome. Mobility of the prosthesis was maintained at 10 years, but the incidence of adjacent segment change was 12% much as reported after fusion; hence, there was no evidence of a protective factor achieved against adjacent segment disease by a mobile implant. Four patients required revision surgery, and 30% were lost to follow-up. All the operations were done by one very experienced surgeon, so we may be confident that this is an ideal situation to ensure technical excellence. Half of the patients had a hybrid construction, a fusion at L5/S1 and a disc replacement at L4/5. The paper does not give sufficient information to allow one to conclude that the results are better than a two-level fusion, and if in fact they are the same, and there is no clear protection against adjacent segment degeneration, then the concern that the implant may represent a hostage to fortune in the long-term must remain. The value of the paper is somewhat diminished by the loss to follow-up of 30%.

I was very interested to learn that allograph transplantation of cervical disc has been successful, but that as feared failure is due to failure to establish a satisfactory blood supply in the transplanted disc. The paper by Huang et al. [19] describes their experience in grafting disc into goats. They found that although the blood supply was established, it differed very much from the normal. The paper is of interest as it gives a good resume of the problems that may be experienced in attempts to create a biological disc replacement.

Adult spinal deformity

The treatment of adult spinal deformity has become an important part of spinal surgery related to the ageing population and patient expectations. The provision of a back support and analgesia is not acceptable, largely as neither is very effective. The paper by Yagi et al. [20] looks at their cost-effectiveness of such surgery, comparing the experience in Japan with that in the USA. The comparison was very unflattering for the USA, their costs were double those of Japan, their infection rate higher, and their re-operation rate higher. Their cost/QUALY $1159.665 at 1 year, and $511,840 at 2 years, compared with Japan $459.135 at 1 year and $225,668 at 2 years. Of general concern is the fact in both countries, the cost/QUALY was much above that set by the WHO, which is three times per capita GDP, which should be $145.000 in the USA and $173.000 in Japan.

However, such surgery is effective. The paper by Cheng et al. [21] looked at the outcome of surgery for degenerative lumbar scoliosis, derived from the Swedish Spine Register. They looked at the records of 209 patients surgically treated for lumbar scoliosis, 45 had undergone fusion and decompression at one level, and 164 had more extensive surgery. Both groups had improved significantly, but those having major surgery in regard to pain and patient satisfaction were better. However, whereas in the minor group there were only three patients out of 45 who had complications, in the major group 21 out of 164 patients had complications. In the major group, the risk of further surgery was higher. Overall 57 patients out of the 209 had further surgery, principally from those who had more major surgery initially. This suggests that being conservative does not mean that further surgery is likely, or electing to do more major intervention does not reduce the likelihood of further surgery, but increases it, which is rather counter-intuitive. However, such a conclusion is a little suspect as the two groups were not the same in severity initially. The paper by Zanirato et al. [22] complements the above paper. They did an extensive literature review of complications of adult deformity surgery, reviewing 96 publications on 12,168 patients. The complication rate was 24–36% and occurred equally in MISS procedures

as in open ones. Combining MISS procedures with an open procedure produced the most complications.

Spinal stenosis

Since the classical paper by Herkowitz et al. [23], there has been a continued debate concerning whether decompression alone or decompression with fusion is the appropriate treatment for spinal stenosis due to degenerative spondylolisthesis. The systematic review of the literature by Dijkerman et al. [24] adds little to the debate, concluding that there is insufficient evidence to answer the question. Clearly, patients with this disorder have spinal stenosis and for that decompression is appropriate. Some of those patients have stenotic-related back pain, and some have back pain due to instability of the segment. Some may develop back pain subsequently due to this instability exacerbated by a generous decompression. A careful clinical history can identify stenotic-related back pain from mechanically related back pain. The value of this paper is that it makes clear that in the short-term decompression alone is appropriate and the decision to add a fusion must be made based on an understanding of the patient's symptoms, is the pain due to the stenosis, or is it due to mechanical factors? The likelihood of further slip due to the extent of decompression must be evaluated.

Restriction of walking distances is an important symptom in spinal stenosis and is a symptom that patients wish to be remedied. It is therefore important to assess this prior to operation to allow us to assess the success of the operation subsequently.

The paper by Jespersen et al. [25] examines the value of the Oswestry Disability Index in assessing walking capacity. The authors conclude that it is of value, but should not be used as a stand-alone assessment and that specific walking tests should be used. They advocate the development of GPS systems to be applied with the use of accelerometers, which could be incorporated in a walking plaster.

Access-related injury has been the driving force in the development of minimal intervention procedure, but has also influenced surgeons doing open procedures. The paper by Lee et al. [26] compared two techniques used in doing a decompression. Both minimally disturbed the muscles, one was a splitting of the spinous process, and then retracting both halves with their muscles attached, and one was a basal osteotomy, so that on one side, the muscles were detached from the spinous process, but not on the other. They found that in the first month, back pain was less in the spinous splitting group, and then symptoms were similar, but at a year, the basal osteotomy group had more back pain, possibly related in some patients to non-union of the osteotomy. This was a delightfully clear paper with excellent illustrations, and made a very clear point, that doing an open procedure it was still possible to minimize access-related trauma, and this was clinically valuable.

Infection

In the October issue, there were a number of clinically useful papers on infection. The paper by Higgins et al. [27] dealing with the value of preoperative screening for both methicillin sensitive (MSSA) and methicillin resistant (MRSA) and treating the positive carriers of nasal staphylococci with Bactroban nasal ointment and Octenisan body washes for 5 days prior to surgery. They report on 716 patients, screening only for MRSA from April 2015 to March 2016 who were screened only for MRSA, and had an infection rate of 2.6%, and a second cohort of 519 patients screened for both MRSA and MSSA, the latter with a colonization nasally of 26% preoperatively. Treating those positive for MSSA with the above regime produced an infection rate of only 1.02%. A further paper by Mallet et al. [28] showed that preoperative screening for nasal carriage of aureus, and then treating such carriers with nasal ointment for 5 days, reduced their staphylococcal infection rate in treating adolescent scoliosis 5.1–1.3%; the incidence of nasal carriers in their 331 patients was 23%. In our obsession with methicillin-resistant staphylococci, we should not forget the risk of infection with non-methicillin-resistant staphylococci.

The paper by Ohlin et al. [29], which reported the strong antimicrobial effect of titanium granules, treated with hydrogen peroxide, producing a Ti-peroxy. This was particularly effective against staphylococci, less so against proteus and pseudomonas, but very effective against anaerobic bacteria. They suggest that application of hydrogen peroxide to titanium implants prior to insertion would be of value.

The debate concerning the role of infection in causing back pain continues. The paper by Yuan et al. [30] produced further evidence of its possible importance. It is accepted that *P. acnes* is found in a proportion of discs excised surgically. However, the view is expressed that these are from contamination during the surgical procedure. In this paper, the authors showed that discs from which *P. acnes* had been grown had inflammatory changes and specific cytokines, which mirrored the degree of bacterial growth, suggesting that this was an inflammatory response to the bacteria, which of course would not be present if the reason for their presence was contamination at the time of surgery.

Cervical spine

The paper by Kelly et al. [31] looking at reoperations and complications after anterior cervical discectomy as compared with cervical disc arthroplasty is of value for two

reasons. Firstly, it draws on some 52,395 patients, and the records are from California's Office of Statewide Health Planning that the study is not supported in any way by commercial interests. I was surprised to note that of the over 50,000 patients, only 1469 had had a disc arthroplasty. The paper drew attention to the fact that since 2007, the use of arthroplasty as opposed to fusion by surgeons sitting their board exams has fallen. The paper demonstrates that arthroplasty confers no clear benefit over fusion as regards the development of adjacent segment disease. Essentially, the two procedures produce identical results in the long and short term. The authors do not comment on cost, but presumably, their fusion group are spared the cost of an implant.

The paper by Li et al. [32] reported on the use of a partly constrained cervical replacement, designed to allow a range of movement, but not as unconstrained as other cervical disc replacement. They reported on 70 patients, divided randomly into those who were treated by fusion and those who had the constrained disc inserted followed-up for more than 5 years. They were unable to demonstrate any protective effect on the development of adjacent segment disease, as compared with the fusion group.

One of the supposed advantages of a cervical disc arthroplasty is its potential value if two levels are to be treated and the view that a two-level fusion will carry a higher risk of ASD. The paper by Basques et al. [33] challenges this view. In 404 patients who underwent anterior cervical fusions, 130 had a one-level fusion, 192 had a two-level fusion, and 82 had a three- or four-level fusion. Multilevel fusions achieved excellent restoration of sagittal balance, and the one-level fusion gradually did. There was no difference within the groups in the development of ASD. Clearly, an important element was that in multilevel fusions, careful attention was paid to the correction of alignment.

The long-term non-recovery that is 2–4 years of a whiplash injury seen in an emergency department is in the region of 30%. The paper by Rydman et al. [34] looked at the effect of non-recovery if the patient was involved in an insurance claim. The authors show that the non-recovery rate at 2–4 years in a group of 114 patients culled from insurance records in a prospective study had a non-recovery rate of 50% if not compensated and a non-recovery rate of 70% if compensated. There were two possible reasons for this: firstly, the compensated group were worse and therefore that is why they were compensated, or the fact that they were compensated made them feel worse. In favour of the second explanation was the fact that at 6 months, there was no difference in their self-reported level of pain between the two groups. Clearly, the giving of compensation does not cure people, and one suspects that patients who assiduously pursue compensation do well on their disability making non-recovery more likely. They may subconsciously feel that to justify the fact they were compensated requires them still to be symptomatic. The paper does suggest that we should discourage our patients from seeking compensation. Doing so will make it less likely that they will recover.

When a patient has a significant cervical radiculopathy, with a proven disc protrusion the decision as to whether to operate is to some extent determined by the likely prognosis for recovery if treated conservatively. In their paper entitled "Clinical course and prognostic models for the conservative management of cervical radiculopathy; a prospective cohort study", Sleijser-Koehorst et al. [35] reported on the clinical course of a proven radiculopathy, with appropriate MRI findings. Some 55% had recovered or had minor symptoms at 6 months. At 12 months, 11% still had high-intensity arm pain, and 18% high-intensity neck pan. Length of history and severity of pain at the onset was predictive of a poor prognosis; curiously paraesthesia was a favourable feature. My own conclusion was that if a patient still had significant symptoms at 4-6 months, then surgical treatment should be considered, and this paper is of value in discussing this option with the patient.

Metastasis

In a patient with spinal metastasis treatment, options depend on the patient's prognosis. Patients likely to survive more than 3 months usually justify treatment other than just palliative. The point score devised by Tokuhashi is commonly used, where a high score indicated longer survival. The paper by Aoude et al. [36] retrospectively reviewed the records of 126 patients treated for spinal metastasis in which the Tokuhashi score was used, and they assessed the value of the various parameters. They found that the primary site was of much less value than the various other parameters, such as number of metastasis in the spine, metastasis in other major organs. It is generally considered that certain primary cancers, such as lung, have a poor prognosis; they are likely to metastasize early, but once metastasis has occurred, it is the degree of spread that determines survival, not the nature of the primary tumour. The Tokuhashi score worked well, but a low score due to consideration of the primary should not unduly influence the clinician in the decision as to treatment. However, Bollen et al. [37] in a systemic review of prognostic factors predicting survival in patients with spinal metastasis come to an entirely different conclusion. They reviewed some 22 papers, and I noted that in 11 of them there was no follow-up, so it is not clear how the papers quoted arrived at survival times. Clearly, a patient with a lung cancer has a poorer survival than a patient with a prostatic cancer; the issue is whether once either cancer has metastasized, it is the degree of metastasis that determined survival. I found it strange that Bollen et al. did not in their

discussion mention the use of the Tokuhashi score or indeed any other scoring system.

Osteoporotic fractures, kyphoplasty and vertebroplasty

In their introduction to their paper concerning predictive factors of a poor result in conservatively treated osteoporotic fractures, Muratore et al. [38] state that conservative care leads to good results and that they heal with excellent functional recovery, although the papers he quotes to support this view accept that there are no satisfactory studies owing to the lack of standardized care and in adequate follow-up. Clearly, however, some osteoporotic fractures have a very poor long-term result, and his paper identifies features and the position of the fracture that are likely to be associated with a poor result which on MRI scanning have a bow-shaped anteror projection, or have a linear black signal, those with middle and anterior column damage, and those that are at the lumbo-dorsal junction. This is a valuable paper, which truly aides the surgeon or physician as to the role of kyphoplasty or vertebroplasty.

There is a continued debate about the relative merits of kyphoplasty or vertebroplasty. The paper by Butscheidt et al. [39] reported the incidence of cement pulmonary embolism in a cohort of patients who had had vertebroplasty. His local department of Legal Medicine routinely does a full body CT scan of patients who are having a post-mortem. As a consequence, the authors were able to accurately identify the true incidence of pulmonary spread of cement. The cause of death in all cases was unrelated to the pulmonary cement; it was in effect an incidental finding. However, the incidence of leakage was impressive; local leakage was 69%, intervertebral 31%, intraspinal 14%, retrograde venous 17%, and lung was 52%. The authors grade the extent of lung spread into four grades (0–3), but 10% were grade 3, a branching deposit, which may well have been clinically relevant. Although the authors emphasize that this level of spread was not associated with patient mortality, it would seem from these figures that the degree of spread is unacceptable, and kyphoplasty would be the preferred procedure.

In carrying out a systemic review and meta-analysis, comparing single balloon versus double balloon bipedicular kyphoplasty, Jing et al. [40] demonstrate the effectiveness of both techniques in restoring kyphotic angle and pain relief. They demonstrated that using one balloon into one pedicle, and then taking it out and inserting it into the second pedicle, was as effective as using two balloons simultaneously. Clearly, using one balloon was cheaper.

A further development in the treatment of osteoporotic fractures is the use of a stent, described by Schützenberger et al. [41] as used in treating coronary arteries. In carrying out a balloon kyphoplasty, the balloon is removed, and cement inserted into the space so created. However, using a stent, this is inserted into the balloon, and after the balloon has been expanded, the stent is also expanded, and then the balloon is removed leaving the stent in place and that is filled with a cement. In their paper, Schützenberger et al. report a series comparing balloon kyphoplasty with the stent technique. They used a calcium phosphate cement, rather than PMMA. This cement has much less compressive strength than PMMA, but its compressive strength is closer to boner, and it is resorbable. The radiological result was better in the stented group, but this was not reflected in the clinical outcome, which was good in both groups. Essentially, using caP cement there was more loss of correction, but this was less in the stented group. They used a bipedicular approach, using two balloons. One suspects that this type of intervention is costly two balloon and two stents.

Imaging

The value of upright spinal MRI is addressed in the paper by Botchu et al. [42]. They have the advantage that the spine can be examined in positions other than supine, but at the price of a more expensive machine, a field strength only up to 0.6 Tesla, and a poor signal to noise ratio. It can be clearly demonstrated that the standing erect spine in extension has smaller canal diameters, but we know that. Much is written in the paper about instability, but the importance of this in the genesis of back pain is increasingly questioned Mulholland [43]. I was surprized that the author did not show a clear disc protrusion, not present on a supine MRI but demonstrated on an upright one. I suspect that the main value of such scanners will be that they can be used for a patient who is claustrophobic. A unit who had one would also have to have a standard scanner in view of the low Tesla rating and signal to noise ratio. There should be more of these machines so that access to them was facilitated for certain situations.

The paper from Singapore by Lau et al. [44] deals with the use of MRI and CT in the assessment of patient conscious or semiconscious “obtunded” with suspected or possible cervical injuries. Because in the clinical setting of an emergency department, and a possibly multiply injured patient, where a CT scan may be required to assess other injuries, a CT scan is an effective and rapid way of assessing the possibility of a cervical spine injury. It was the unit practice to follow this up within 48 h with an MRI scan. Of 63 patients reviewed retrospectively, the subsequent MRI did reveal injuries not detected on CT scanning. Their conclusion was that a follow-up MRI is not always essential, but it should be mandatory if there was a suspected thoracic spine injury, neurological defect or a unilateral facet dislocation.

It should be considered if the patient was female, there was direct blunt force, a fall from a standing height, bilateral facet dislocation or intracranial haemorrhage. Its use as the primary imaging was impracticable in the setting of an accident department.

One important aspect of MRI is the lack of radiation, and this is particularly important in children. The paper by Dhouib et al. [45] looked at the diagnostic accuracy of MR imaging as compared with CT imaging doing an extensive literature search. Reviewing some 900 papers, eventually the authors relied on four papers, which met their exacting criteria. They point out that one paper showed that out of 100 adolescent athletes with back pain, 47 had a spondylolysis, illustrating the need for imaging in this group and the need to avoid radiation if possible. Their meta-analysis showed that there was a moderate to high sensitivity and a high specificity for MRI for the diagnosis of lumbar spondylolysis in children and young adults. They make the important point that as the defect is at an angle to both sagittal and coronal planes, in the older patient the presence of facet arthropathy makes identification difficult. They also emphasize that the use of 3D slices with oblique reconstruction in the axis of the pars was desirable.

We are now in the fortunate position as a result of MR imaging to be able to recognize disc degeneration before bony change reveals it in standard X-rays. Indeed, primary examination of the spine now should be by MR imaging and not X-rays, if the patient's condition permits it, such as is the case in a patient attending casualty with back pain or sciatica, in which if it is felt they merited imaging. However, what MR images are best to quantify the degree of disc degeneration? The paper by Hu et al. [46] looks at this issue. They looked at the images of 66 subjects, ages ranging from 22 to 84. They postulated that as people got older there was age-related disc degeneration that is there would be expected to be more degeneration in an 80-year-old than a 60-year-old. They then looked at the images to assess which images best showed this change and established that it was the mid-sagittal. However, they made two other important points: firstly, anterior bulging as a most significant change rather than posterior bulging in diagnosing the degree of disc degeneration.

In treating spinal fractures, it is important to know if the posterior ligamentous structures are intact. The paper by Khurana et al. [47] presents a report on 105 patients with lumbar or thoracic fractures, whose imaging studies were retrospectively reviewed, both MRI and CT, to establish whether the CT alone could predict posterior ligamentous disruption. It was clear in those severe injuries, with anterior displacement one could infer that the posterior complex was disrupted, but in much less severe injuries, where the only feature on the CT might be increased in the gap between the spinous processes; this sign had considerable observer error and was not reliable. Clearly, in severe fractures, the surgeon is going to stabilize in any event, but is the less severe fractures, where the decision to stabilize may depend on knowing the state of the posterior complex, MRI is required.

Supplements

The supplements are an important addition to the monthly journals. Two of the supplements this year merit careful study. The first in February focussed on spinal osteotomy and introduced us to the educational concept of "Meet the experts" (Gunzburg et al. [48]). This educational initiative consists of a printed special issue of the Journal, and then a group of experts and a number of delegates meet together at a selected venue and for 2 days discuss the papers with an interactive question and answer technique. The supplement deals with the complex topic of spinal osteotomy and is almost a textbook on the subject. Any surgeon involved in spinal osteotomies should study it carefully. Koller et al. [49] give sound practical advice of technique and planning of surgery in ankylosing spondylitis.

In the last 20 years, adult spinal deformity correction has become an important part of spinal surgery, but as Redaelli et al. [50] point out, we only manage to achieve proper correction in 32% of cases, and 68% remain over or under corrected. They provide a very concise description of the problems in distinguishing focal problems that produce compensatory mechanisms and the complex relationship with pelvic inclination. I particularly enjoyed their clear exposition of initially progressive pelvic rotation to solve a kyphotic problem and then, when it had reached its limit, the role of the knee and ankle. I found it a very readable paper on the issue of sagittal balance and its influence on surgical planning.

The supplement in September, dealing with "A Global Spine Care Initiative", was an interesting background, summarized by Gunzburg et al. [51]. World Spine Care was a charity founded by Joan and Scott Haldeman, and it convened a Global Spine Care Initiative aiming to improve the care of underserved people with spinal disorders. The supplement provides information concerning spine disability in what is sometimes called the third world that is countries that are poor. What I found surprizing was the fact that woman was more often the major sufferers, that there was little difference between rural poor and urban poor, and that factors such as poor education, depression and other psychological factors and alcohol were all important. In such countries, it is the case that hard physical work is a common experience, but this is not commented upon as been a factor in back pain. Is this further evidence that the mechanical factors such as repeated stress causing back pain disability

has been overrated and supports the view that hard work and lots of movement is good for the healthy back?

There are over 350 papers published in the Journal each year and Grand Rounds and Letters. Clearly, my selection of around 50 gives no idea truly of the enormous educational achievement of the Journal. I hope readers will look at some of the papers I draw their attention to and perhaps read others that they come across in doing so. Space does not allow me to discuss the many Grand Rounds, which I believe are a very important educational source, as so often they deal with diagnosis rather than treatment, and skill in diagnosis comes with experience rather than reading papers.

Compliance with ethical standards

Conflict of interest The author has no conflict of interest.

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