A survey of the “surgical and research” articles in the European Spine Journal, 2006

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Introduction

With the greatly increased size of the Journal it is of course very difficult to draw attention to all the papers of great merit. My hope is that by alluding briefly to some, with a brief account of what I feel is important about them, readers will be encouraged to read them. Some papers I am critical of and in these cases it is perhaps even more important that the reader examines them and makes their own judgement.

Review articles

The review article, of which fifteen were published this year, is an important part of the Journal's role in continued education and ensuring the practicing

orthopaedic surgeon is reasonably informed about recent advances in treatment, and assessment of established treatments. I found them all stimulating reading but space permits me only to discuss a few.

Efficacy and safety of balloon kyphoplasty [9]

It is clear that cement augmentation of vertebrae (vertebroplasty and kyphoplasty) represents an advance in the treatment of osteoporotic fractures of the spine, and myeloma. It is clear that the leakage rate is much less with kyphoplasty, but apart from that, the benefits compared with vertebroplasty having regard to the extra expense of kyphoplasty are still uncertain. For any surgeon considering the use of these treatments this review is extremely comprehensive and the conclusions very helpful. Although leakage in vertebroplasty [46] may be as high as 81%, it is usually innocuous. However, it can produce serious complications, so I suspect that a patient may prefer kyphoplasty, whereas a provider/purchaser would regard vertebroplasty is an attractive alternative. A valuable assessment of the paper, drawing attention to the pitfalls of a systematic review, when one is so dependent on the reviewers assessment of the primary papers is provided by Verlaan [52].

My colleague Michel Benoist has dealt very fully with the excellent systemic review paper dealing with percutaneous vertebroplasty in the management of osteoporotic fractures [41]. The review does not deal with balloon kyphoplasty or other lordosing procedures involving the use of cement. Whereas the clinical benefits of correcting deformity are uncertain, the theoretical basis for attempting to achieve it are made very

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clear in the paper by paper by Rohlmann et al. [44]. Using a finite element model they demonstrated that correction of sagittal alignment is the most important factor in preventing excessive stresses above, presumed to be a cause of further fractures. In vertebroplasty the erector spinal loads increased by 200% due to persisting deformity, in the kyphoplasty the load increase was only 55%. However, this assumed almost complete correction of the deformity, which is usually not achieved by balloon kyphoplasty. In their model stiffness of the segment was not associated with excessive erector spinal loads. In this regard the paper by Orler et al. [38] describing what they term “Lordoplasty” is of considerable interest. Using cement augmentation of the vertebrae above and below the fracture, and inserting pedicle pins into these, in the prone position they manually reduced the fracture by a lordosing manoeuvre, and then filled the fractured vertebrae with cement. They achieved considerably more reduction than balloon kyphoplasty would have, although not complete reduction. They report on 26 out of 36 patients, the procedure mainly being done at the lumbo-dorsal junction, presumably none in the thoracic spine and relate that two subsequently required open surgery for what they call “segmental instability”. As the paper by Rohlmann alluded to above points out, the body has other compensatory mechanisms to deal with kyphosis at the lumbo-dorsal level, or below, that is the hips and upper body, so that the extra reduction gained by the procedure of lordoplasty at this spinal level will be of value even if complete reduction is not achieved. Because of the hospital stay, and the fact that it is usually done under a general anaesthetic, it will not be a cheaper option than balloon kyphoplasty. However, in this region of the spine striving for as much correction as one can may be worth while, as the body’s compensatory mechanisms can augment the effect. Further assessment of this technique is promised by the authors.

Endoscopic surgery

Endoscopic surgery on the thoracolumbar junction of the spine [8] although only representing the experience in one unit is of considerable interest to anyone contemplating adding this skill to his surgical practice. The technique is used for fracture fixation, decompression, deformity correction, infection and removal of loose implants. The principal advantage of such techniques is the reduction of approach related complications from around 14% for open procedures to about 5% in their experience. However, the incidence of vascular complications was 0.3% compared with reported incidence

in open procedures being 0.08%. As far as non-access related complications-apart from the vascular ones, they are same as open procedures. The impression was that extending the thorascopic approach to the thoracolumbar area was a major step, where the benefit was insufficient to justify not only the risks, but the considerably greater expertise required.

The review paper on the thorascopic treatment of hard thoracic discs by Gille et al. [18] illustrates the importance before such surgery in categorizing a thoracic herniation as “hard”, the diagnostic features being the presence of calcification, association with Scheuermann’s disease, and being commoner in woman. The incidence of 39% dural tears, and the need to subsequently do an open operation to repair the dura in over half of those with a tear led to their valuable observation that incomplete removal of the herniation, leaving a bony chip on the dura, thus avoiding a tear produced an equally satisfactory clinical result.

Management of hangman’s fractures (X.-F. Li) [31]

Despite the ghoulish name for these fractures, nowadays they are caused by commoner and more banal events, such as diving, road traffic accidents or falling. The great merit of this paper is that it makes it clear that classification is important and determines management. There are no X-rays showing the different types of fracture, the line drawings illustrated the classification rather than explained. Clearly the authors presuppose that the reader is confident in diagnosing the various types. I found that it repaid to read the paper in conjunction with the original paper by Levine and Edwards [30], which has both illustrations and X-rays, and clearly explains the basis of the classification. The particular difficulties in distinguishing a type 11 (flexion/compression) from a type 11 (a) (flexion-distraction injury) are not spelt out in the review paper, or that if conservative treatment is decided upon the method of reduction and maintenance of reduction differs. Although Levene in 1985 reserved operation for type 11 fractures, he emphasised the very unstable nature of the type 11 (a) fractures, and the advice of the review article that operation for these should be considered seems appropriate.

Electrical stimulation therapies for spinal fusions: current concepts

The original papers on this subject by Bassett [6, 7] caused much excitement amongst those of us treating

fracture non-union, not justified by subsequent experience with its use. However, in fracture treatment there is again increasing interest, (some 107 papers on Medline dealing with the effect of electrical stimulation and bone healing) and a recent prospective randomized study concerning its use in treating tibial fractures showed a significant treatment effect [47]. It was against this background that this paper must be assessed. I note the lead author is the research director of a company that manufactures the implants. I was surprised that the clinical studies quoted by the author were all in the eighties and nineties, apart from a couple the first couple of years of this century, and two more recent “animal model” papers by the lead author in 2004. As numerous studies have shown no relationship between clinical success and a solid fusion, I was surprised at the reported cost effectiveness of their use reported in 1996 [23] by Kahanovitz which, however, had seemingly little effect on its clinical use at that time. The authors suggest that DC stimulation up regulates osteo inductive factors in a rabbit model, quoting a paper of their own which is in press. There is a good account of the various forms of electrical devices currently available. This review did not remove my scepticism.

Spondylolisthesis

Fusion for low-grade adult spondylolisthesis [21]

Lytic spondylolisthesis may present in the late teens and early twenties as a painful back, not related to disc degeneration, but related to the lysis -insofar as lysis repair in this group is a satisfactory treatment (but see below) It may present in the early thirties and later as essentially a disorder related to disc degeneration (the radiosterogramatic studies of Axelsson et al. [5] have shown no abnormal movement over and above that seen in degenerative disease unassociated with spondylolisthesis), or it may present in the fifties with root entrapment. What was disappointing in this review was that it encouraged the concept that “low grade adult spondylolisthesis” was a diagnosis of a specific clinical disorder meriting specific treatment. Many of the series reviewed contained patients from all three types of presentation. Hence their conclusions are somewhat unsatisfactory. I was particularly concerned that whilst they accepted that instrumentation was unproven, and was a cause of complications, they seemed to accept its use on the basis of the surgeons “empirical experience”.

Because of their failure to distinguish between the various types of presentation they could state that

“decompression has not been proven to be necessary and may be detrimental”. Tell that to a 55-year-old man with severe L5 root pain with a lytic spondylosis and no back pain. Their recommendations for research are again flawed by this lumping together an anatomical abnormality as a single clinical entity. Anyone reading this review might feel justified in carrying out a front and back procedure with reduction on a 25-year-old man being unaware of the paper by Hagg et al. [19] concerning the risk to sexual function of anterior fusion revealed in the Swedish Fusion study!

Classification of Spondylolisthesis

A proposal for a surgical classification of pediatric lumbosacral spondylolisthesis based on current literature [32]:

This is a useful classification as it takes account of affect of dysplasia in moderate and severe grades of spondylolisthesis, and the effect of sagittal balance and a retroverted pelvis in patients with a spondyloptosis. It emphasises the significance of the kyphotic angle, and the importance of correcting this, rather than the degree of displacement, and the effect recognition of this has on planned surgical treatment. It emphasises the central importance of sagittal balance, and nowhere suggests that reduction is mandatory. They correctly point out that there is only little evidence in the literature supporting the theoretical advantages of reduction. What appears clear is that reduction is not necessary to achieve sagittal balance indeed reduction is sometimes followed by the development of a listhesis above, suggesting that it does not produce normal sagittal balance The paper concerning reduction presented at the ESS meeting in Istanbul shows this well, with 6 of the 39 developing a kyphosis above the reduction (Steib) [49], three of which required further surgical correction. Perhaps this classification with its inclusion of the spino-pelvic dimension into the classification will allow us to identify those patients who after reduction develop a kyphosis and indeed a spondylolisthesis above.

The paper concerning tight hamstring syndrome [25], which reviewed retrospectively 102 children who had presented with “tight hamstring syndrome” and found that some 73% had a serious underlying disorder. To my surprise in only 27% was the cause a disc protrusion or a high-grade spondyloptosis. Some 37% were due to tumour, and 15% due to infection. Their view that the syndrome is due to irritation of the lumbar roots and meningeal branches is important. This may explain why in spondyloptosis once fusion is

achieved, with no reduction, this irritation ceases, as the postural abnormality due to tight hamstrings resolves.

The paper by Schlenzka [45] concerning the long-term results of fusion compared with lysis repair in low-grade spondylolisthesis and spondylolysis is of some importance, and the answer is unexpected. The paper demonstrates that if faced with the choice of repair of the lysis or fusion of the segment, fusion is the best option. Lysis repair has more complications; it does not prevent the underlying disc becoming degenerate, and curiously is associated with greater stiffness of the segment above. I hope this paper does not have the effect of encouraging surgeons to do anterior fusions, reductions or instrumented posterolateral fusions, not appreciating that the operation that repair was being compared with was an uninstrumented posterolateral fusion in situ, and the results of this were excellent.

Supplements

This year we had four supplements, which are important as they bring together a body of expertise in a field from various sources and give an excellent overview of important areas.

Back pain

European Guidelines for the management of low back Pain [14]:

Many countries have guidelines for low back pain but this supplement is the publication of a 5 year project, presenting evidence from 48 experts from 14 European countries initiated by the European Commission [14]. The editorial comment indicates some of its weaknesses [2] and this was further amplified in a review article [3]. The principal criticisms were that therapeutic recommendations should have been more rigorous, and lack ambiguity. I did not feel they were particularly ambiguous in their dismissal of nearly all invasive treatments for back pain, except for two I was unaware of, percutaneous electrical nerve stimulation and neuro-reflotherapy. Acupuncture, nerve blocks, facet blocks, epidural steroids, trigger point injections, IDET, etc. were all dismissed as treatments they could not recommend. Their acceptance of fusion for low back pain if one waited for 2 years before doing it may make some clinical sense, although the scientific basis for delay is not established. A recent review of what happens to patients on waiting lists, showed that whilst some 18% did improve, 35% got worse, and 44% were

unchanged [33]. One's conclusion was that if one did use non recommended treatments, then it was imperative they be cheap and safe, and if non effective would at least be harmless. Some non-recommended treatments carried significant risks, such as spinal cord stimulation, or IDET, or even disc injections of steroid. One would hope that the success of multidisciplinary therapy, including cognitive behavioural therapy would help in the development of centres providing such treatment, which are few.

I found that the guidelines for acute non-specific low back pain clear and easily assimilated. I was disappointed that they did not address the role of the MRI as strong weapon in reassuring a patient that despite the severity of the pain, the complaint was “not serious”. I would hope that their recommendation of multidisciplinary programs in the work place would not fall on deaf ears. I found the supplement an excellent overview of where we are now, and I was interested to note that they recommended an update of guidelines in some 3 years, perhaps at that time taking note of the criticisms levelled at this set.

Two reviews, “What predicts outcome in non operative treatments of chronic low back pain?” [53] and “Biosocial risk factors after lumbar disc surgery” [15] are usefully considered together especially when read in conjunction with the guidelines dealt with above. The first paper demonstrates that bio-psycho-social factors that greatly influence the success of conservative treatment of back pain, and the lack of a close relationship between pain and disability. It is clear that disability in chronic low back pain is maintained primarily by factors other than objective medical data. When we look at the second paper, it is also clear that the 10–20% failures of treatment in those patients who have had what we would all regard as a satisfactory surgical solution commonly are in the main unsatisfactory for the same set of bio- psych-social reasons. Careful appraisal of patients before such surgery would allow this to be identified. Some patients have two problems, root entrapment and bio-social risk factors and both have to be treated. Time devoted to this endeavour may be more valuable than developing materials to prevent fibrosis [20].

As we have seen the European guidelines emphasise the importance of behavioural therapy in treating chronic low back pain, the paper by Soegaard [48] illustrates this well when applied to such patients after they had their back fused. After operation they randomly allocated their patients to three groups, a group shown a video and given advice, a group who were enrolled on an exercise program, and a group who were given the video, and then attended a Back

Café—literally a café where they met other fusion patients, and had a physiotherapist skilled in rehabilitation who answered queries and anxieties of the group. They met in the café just three times for 1.5 h. The paper assessed the success of the three groups by monitoring their use of their primary care provider over a 2 year period. In a previous publication [13] this group had demonstrated that the back café group performed functionally better than the other groups, indeed the training group had more pain than the other two groups. This paper demonstrated that besides being more effective than the other two groups, it was also cheaper.

A further important supplement concerned the lumbar intervertebral disc dealing with basic science of the disc and the clinical aspects [34].

For anyone involved in the basic science of the disc this supplement brings many strands of basic science together, genetics [11], degeneration imaging [4] tissue repair [22], senescence [43], regeneration [36] and much other work on the basic science of the disc, and its elucidation.

New technology

I read with interest, but with some concern the paper concerning injectable biomaterials by Boyd and Carter [10]. I note that one of the authors would appear to be working for the firm that makes the implant described. The development of the device, an artificial nucleus, to be used after microdisectomy, to replace the removed nucleus, and prevent the dangers of disc degeneration. The great majority of patients who have a satisfactory operation for a disc herniation do not develop disc degeneration of clinical significance. Moreover a recent paper [50] shows that merely removing the compressing agent does not lead to a higher recurrence rate, and that clearing the disc out even in the limited way customary in microdisectomy has a deleterious effect on disc leading to greater Modic changes than are seen with a sequestrectomy alone. Clearly if it is intended that a replacement nucleus will be inserted, a considerable clearance of the disc will be required for an unproven benefit. I was concerned that clinical studies for the use of the artificial nucleus are planned.

The paper concerning IDET [17] made fascinating reading, especially highlighting the problems of a prospective randomized study, and the importance of blinding the investigator. The claimed effectiveness of IDET established by the Pauza [40] paper, using average scores of improvement, hid the fact that 50% did not benefit at all. It does appear to support the view that the investigators must be blind to the therapy gi-

ven in any prospective randomized trial if possible. The paper is an excellent example of impressive early results from the inventors of the device, gradually deteriorating as the device is used by others, and finally revealed as ineffective by a careful randomized trial. It also highlights the hazards of mean scores, in hiding failure in many patients, as even in the Pauza study, only 50% appeared to benefit, and only 40% overall achieved greater than 50% relief.

The paper by Freeman concerning the present status of disc replacement is required reading reporting not only European experience, but also the results of the FDA studies, the latter benefit from being reported by someone not involved in the studies [16]. The paper gives very complete information of considerable value to the surgeon and easily conveyed to patients when obtaining consent. One suspects that the underlying concept of disc replacement is right for some patients and at present at one level alone, but improvements in the implants will improve success. Both should be aware of the overall modest success rate (57%) and the uncertainties of the long-term.

General journal articles

The paper by Trouillier [51] concerns facet loading after disc replacement. In a prospective study of patients who had a Charite disc replacement, they assessed the bone density changes in the facet joints, at the level of the arthroplasty, and the level above. They postulated that if this density did not increase, then the facet was not experiencing greater load, and if it reduced, then the arthroplasty was unloading the facet joint. In 10 of the 13 patients, bone density reduced at the level of the arthroplasty, in 6 of 12 patients above the arthroplasty, and in 3 of 5 below. If one accepts their hypothesis that reduced bone density indicates reduced load, then perhaps this is further evidence that facet failure is not a cause of arthroplasty failure. This accords with the observation of Steib [49] that the degree of facet arthritis in long-term follow up of treated spondylolisthesis was unrelated to back pain. It also rather weakens the case for separate facet arthroplasty.

Cervical spine surgery

The case report by Lagares [29], which demonstrates that very marked non-rheumatoid pannus behind the odontoid, producing a significant myelopathy resolved fully after posterior occipito-cervical fusion alone, with resolution of the myelopathy. Many surgeons would

have considered that transoral resection of the pannus would have been required as this was a non rheumatoid patient, as even with transoral resection of pannus a posterior stabilization is usually also done. The paper by Moskovich [37] indicates that if the compressing agent is a fractured odontoid, then transoral route is appropriate, but it would suggest from this report that if pannus alone, then even in the non-rheumatoid posterior stabilization alone is adequate.

The introduction of lateral mass screws for fixation of the cervical spine amongst other benefits allowed us to avoid the kyphosis that not infrequently followed laminectomy for myelopathy in the osteoarthritic spine. The paper by Kast [24] concerning complications associated with the use of pedicle screws in the cervical spine shows that after an appropriate learning curve, they are as safe as lateral mass screws. They suggest that as a stronger construct than lateral mass screws, they can be used without ventral fixation. Of particular interest was the use in a number of patients of a percutaneous system of insertion using a trokar system, being more reliable than either in an open procedure or with an image guidance system.

The paper dealing with the use of selective diagnostic root block as a diagnostic tool in cervical radiculopathy in the multilevel degenerate spine [1] illustrated the great value of this technique. The clinical problem is that if there are two adjacent degenerate disc, and a radiculopathy, one cannot be confident which level is responsible? Unlike the situation in the lumbar spine there is only a 50% correlation between the classical sensory dermatome, and the actual root involved. The authors give a full account of the reasons for problems in correlating distribution of pain with level. In their group of 30 patients 18 obtained relief from a block, 12 at the level of maximum degeneration, and 6 at the level of less degeneration, and surgery in the latter at the least degenerate level produced a good result in 5. On occasion both levels could be shown to be productive of root pain. A few of their patients had long-term relief from the blocks, and surgery was not required. This paper makes a very persuasive case that pre operative selective blocks should be used before operating for a radiculopathy in a multilevel degenerated neck.

Spinal fractures

Why do we brace stable fractures? In the early weeks such bracing does relieve pain, but probably after a few weeks the discomfort of the brace and the inhibiting effect it may have on rehabilitation, both psychologically and physically as compared with the relief of pain

it produces must question its use for other than the first weeks for pain relief. The paper by Post [42] is of interest in this regard. At 5 years there was no functional difference between 15 unbraced patients, and 18 braced patients, the brace having been worn for 9 months, the first 6 months day and night, and the last 3 months at daytime only. The groups were not matched, and bracing was given to those whose fractures although all A and deemed stable were thought to be more severe. Another rather intriguing result was that some 37% of the patients overall had a persisting degree of restriction of physical capacity, although only 10% had stopped work due to their injury. This physical impairment did not affect their quality of life, and the authors could not find any convincing explanation for it. They describe the long-term results as reasonably good.

The paper by Moller [35] dealing with the long-term results of vertebral fractures in late adolescence was very reassuring. One was relieved to learn that thoracic and lumbar fractures in late adolescence have a predominantly favourable outcome, despite the lack of modelling capacity in the spine at that age. However, as the authors point out the number of unstable fractures was low, the majority being stable one column compression fractures. However, some were associated with a degree of kyphosis, which increased, but seemingly had no clinical consequences. They do not describe degenerative changes as occurring, and draw attention to the fact that long-term follow up of such fractures in adults is not nearly as favourable [12].

Disc surgery

The paper describing the combined intra-extracanal approach to lumbosacral disc herniations with bi-radicular involvement by Paolini [39] gives a beautifully illustrated description of this approach and its particular value if there has been previous surgery, and the disc protrusion is both paramedian, foraminal, and extraforaminal, and thus compressing both the exiting and the traversing root. Removal of these unusual protrusions by either approach alone is very unsatisfactory, and this paper describes the combined approach very clearly and concisely, with beautiful illustrations. I personally believe that certainly for the lateral approach a microscope should be used, as otherwise the bleeding sometimes seen in this approach is difficult to control, and excessive use of the diathermy may lead to rather unpleasant post-operative neuritis, due I presume to vascular injury to the ganglion.

Classification of disc degeneration

It was interesting but hard going to read the four papers concerning grading systems for disc degeneration in the cervical and lumbar spine [26, 27, 54]. It was beautifully illustrated and clearly presented. However, of the three features used in the grading, disc space narrowing, sclerosis, and osteophytes, the two latter observations were influenced by radiographic technique, and to my mind more importantly they have little clinical relevance. Especially is this true of osteophytes, I suspect that the effort involved in up to a 24 points score for size and position of osteophytes is not time well spent. As in the end there are but four grades, it would be of interest to see if a series of increasingly experienced surgeons or radiologists assigned grades very differently X-rays which they “eye balled” only, compared with this very precise assessment.

The paper by Kriek [28] dealt with classification of spinal fractures on the AO system, which is of importance in the clinical management. The important message in this paper was the importance of additional clinical information when classifying fractures and the need to have some experience. The latter was achievable in 3 months by residents.

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