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A survey of the “surgical and research” articles in the European Spine Journal, 2004

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Abstract The Deputy Editor has explained in his editorial the reason for this review. I found it a challenging but very rewarding project. The overall quality of papers was so good that it was very difficult to select the necessarily few which space would allow me to review. Inevitably I am influenced by my particular interests, and hence very many excellent papers will not be discussed; failure to discuss a paper is not a reflection on its quality or importance. Criticisms of papers again reflect my own prejudices.

Keywords Spinal stenosis surgery · Thrombosis · Vertebroplasty · Outcomes · Pregnancy

Thrombo-embolic prophylaxis

It is well established that anticoagulant therapy in orthopaedic surgery reduces the incidence of DVT. It is not clearly established that it reduces the death rate from pulmonary embolism, unless full anticoagulant therapy is used; that is, warfarin. Most spine surgeons are reluctant to use anticoagulants because of the risk of haematoma in the confined space of the spinal canal producing neurological complications. The paper “Review of venous thromboembolism in spinal surgery”, essentially a literature study by Brambilla et al. [7], and the paper following by Gerlach et al. [10], confirmed to this reader that chemical prophylaxis was not justified in patients undergoing posterior surgery alone. One particular statistic was of interest, in a retrospective study of 16,656 patients having lumbar disc surgery, some 10,351 had no prophylaxis, and 6,305 had prophylaxis. In the group who had prophylaxis, the DVT rate was half that of the other group, but the PE rate was double! Perhaps the explanation of this anomaly is that chemical pro-

phylaxis makes DVT less likely, but if a DVT occurs it is likely to be less adherent or inflammatory than usual and hence can break off and produce an embolism. In clinical experience, fatal emboli occur in symptomless DVTs, rather than in symptomatic ones.

Clearly those patients who have a spinal cord injury, and are having anterior surgery, have such a high risk of embolism that anticoagulant therapy is appropriate. The statistics suggest that anterior surgery does carry a risk of pulmonary embolism, especially when the major vessels are manipulated, but in these patients the risks of intra-spinal bleeding is not usually a problem. Full warfarin prophylaxis is appropriate.

Failed back syndrome

This is one of the most unfortunate concepts in spinal surgery, as it conveys the idea that if a back operation does not cure a person permanently then the operation has failed, and in this litigious age this may well lead to

litigation. The paper by Schaller et al. [21] presents a retrospective review of 2,353 patients who had a microdisectomy, and in this group 12 required a subsequent operation at a mean of 2 years after the primary procedure. Patients prior to surgery were carefully evaluated for features of “spinal instability”, using methods of examination that, in the main, have not been validated as indicating abnormal translational movement, but certainly may indicate that the patients were not uncomplicated disc herniations, but had features of a more significant disturbance of spinal function. Unfortunately it is not clear how many were refused a microdisectomy because of “instability”. Certainly the fact that only 12 out of the 89% of the 2,353 that were followed up required subsequent surgery is an impressive achievement, representing only a 0.5% failure rate. It may seem that screening using the various tests described flexion/extension films, traction films, compression films and careful appraisal of the pattern of spine movement was important in producing such results. However, perhaps what the paper is telling us is that patients who have established chronic back pain, who then present with a disc protrusion, do less well than patients who have an acute disc herniation. It would have been of value to know how many were in the whole group, those who had just a microdisectomy and those that had a primary fusion. The authors state that the decision to carry out a fusion was based on “clear radiological evidence of instability” using the criteria described above. One presumes that some patients who had a microdisectomy alone, had some features of “instability”. It is strange that in the 12 patients who failed, none had any abnormal features of “instability” before operation. Is it likely that discectomy alone cures minor degrees of instability? An equally reasonable interpretation of the facts would be that such signs are signs of a painful back due to other factors than a disc protrusion alone, perhaps the degree of disc degeneration, and that the 12 patients who failed unfortunately continued to degenerate and became symptomatic, and then did show these signs.

In reviewing the 12 patients that did fail, it seems that the surgical approach was of importance. Ninety-two percent of the 12 failures had had a hemilaminectomy, compared with only 9% in the whole group. Equally in the non-failures, more had had a monosegmental approach and a monosegmental discectomy. It is presumed by the authors that a bisegmental discectomy removed more of the disc, and this was associated with a somewhat greater chance of a patient being in the failure group. Patients who had a bisegmental approach were more likely not to be straightforward acute disc protrusions, however, and this may be the reason they were more likely to fail.

The authors draw conclusions concerning disc space narrowing and its significance. Unfortunately there is no

record of the incidence of disc space narrowing in the asymptomatic patients, so it cannot be claimed that disc space narrowing is truly evidence of instability, unless we believe in asymptomatic instability. We have no record of why the 12 were chosen for surgery, one presumes that there were other patients who were not a perfect result, but did not merit surgery. The classic paper by Hanley et al. [12], reviewing 100 patients who had had a discectomy, showed that disc narrowing bore no relationship to clinical failure. One suspects that 12 patients are insufficient to draw such a conclusion. One is therefore concerned that the authors suggest that narrowing of the intervertebral space by more than 30% is a clear radiological sign of segmental instability requiring further surgical procedures. It is not unlikely that one of the reasons that the 12 came to surgery was that they had disc space narrowing and were selected for surgery because of this.

When reading this paper it is of value to also read the paper by Axelsson et al. [2] concerning “Intervertebral mobility in the progressive degenerative process. A radiostereometric analysis”. This study used a radiostereometric method to assess abnormal movement in 18 patients with disc degeneration. Each spinal segment was categorized according to grade of disc degeneration: normal without dehydration, normal with dehydration, disc height decreased by less than 50%, disc height decreased by at least 50%. They concluded that segmental mobility status cannot be deduced from radiographic degenerative stage since the inter-individual differences in mobility are pronounced for the same disc status. Although vertical instability decreased as the disc narrowed (not surprisingly) and in the final stage of disc degeneration sagittal instability was reduced, as suggested by Kirkaldy-Willis; however, there was no stage of instability. Their conclusion puts paid to the often quoted Kirkaldy-Willis concept of disc degeneration having a stage of instability, which then stabilizes as degeneration proceeds. It was an armchair concept, convenient to reassure patients that they would get better, but encouraged the belief that there was a disorder called instability, remedied by fusion.

If we combine the information from the above paper with the clinical paper by Schaller, it is clear that far from disc narrowing of more than 50% being an indication for surgery, it might be said that it was a contraindication. If one's belief is that instability was the cause of pain, instability (being excessive translational movement) is reduced as the disc narrows.

Sacroiliac joint fusion

The paper by Giannikas et al. [11], dealing with a simple method of sacroiliac fusion using bone blocks from a posterior transarticular approach, attracted me

for a number of reasons. The operative technique described is simple, using Cloward-type blocks of bone taken from the iliac crest, and punched into the joint through its projection on the inner side of the iliac wing. Any surgeon doing posterior spinal fusions will be well aware of the outline of the joint, as care has to be taken when taking a bone graft not to transgress it. Five cases are described, with satisfactory results in four, the one failure being a patient with persistent back pain. Diagnosis of sacroiliac pain is difficult. The authors used anaesthetic-steroid blocks. It would have been of interest to know how useful clinical examination was; they refer to it “suggesting” sacroiliac joint pathology. It was of interest that MRI was not of value, nor plain films. In the early part of the last century, the sacroiliac joint was commonly regarded as an important source of low-back pain, but the “dynasty of the Disc” displaced it, and I suspect that in the last 30 years, orthopaedic surgeons have underplayed its significance, in part because fusing it was difficult before the advent of effective intra-operative imaging. The advantage of this method of fusion is the low complication rate, as no metal work is used, and hence the penalty for misdiagnosis, and hence unsuccessful surgery, is less.

Haemostasis in spine surgery

The availability of a blood transfusion service has made spinal surgeons somewhat cavalier about blood loss, because in the main it is unusual that it presents a serious clinical problem, and if it does the transfusion service rescues us. The Editorial Board, by having the courage to devote a whole supplement to this somewhat neglected field in spinal surgery, brought the whole subject to our attention. The supplement is in the main a series of reviews, dealing with positioning on the table, various chemotherapeutic agents of possible value in reducing blood loss, the use of pre-operative haemopoitin in spine surgery, haemoglobin substitute research, the use of local agents and other relevant areas.

The original paper dealing with “Predeposit autologous donation” by García-Erce et al. [9], although clearly burdened with an excess of data for a quick skim through, is an invaluable source of information to any group of surgeons who wish to set up such a program. The need for careful organization and certainty of the time of operation is emphasized, something that may not be possible in units where emergency surgery and shortage of ITU beds may interfere with operative schedules. The public are increasingly and rightly concerned about the hazards of donated blood, and pre-donation is popular with them, the professions should respond to this, and this paper provides much basic information.

Spinal stenosis surgery

The three papers dealing with spinal stenosis were valuable additions to the literature on this subject. The first by Spratt et al. [22], “A predictive model for outcome after conservative decompression surgery for lumbar spinal stenosis”, was a difficult read, but very rewarding. We are all aware that the outcome of surgery for spinal stenosis is unpredictable. We all have unsubstantiated views as to why, and this paper, besides showing overall that minimal decompressive surgery is as good as more extensive decompressions, alerts us to the likely importance of aortic calcification as a predictive factor for a poor result. It certainly suggests that both venous congestion, produced by the stenosis, and arteriolar blood flow (affected by aortic disease) are factors in the clinical picture of spinal stenosis, and the fact that decompression does not deal with the arteriolar component may be an explanation of the somewhat disappointing results of spinal stenotic surgery. The importance of non-organic signs and excessive degree of pain as predictors of failures in men, but not women, and the fact that canal diameter seems to be a predictor in women and not men were intriguing. Clearly the numbers are small, and one would hope that this type of study will continue, and give us further insight.

The study by Zucherman et al. [24] on the X-STOP device, a very minimal treatment for spinal stenosis, although a multi-centre study, with a significant drop-out rate in the untreated group, certainly provided a basis for a prospective, independently reviewed study, preferably in a single centre. The simplicity of the procedure is attractive.

The third stenosis paper, by Arinzon et al. [1], dealing with treatment of diabetics with stenosis, whilst confirming what many of us felt—that is that results are less good, and unpredictable—showed it was worthwhile. This is the sort of paper that provides useful information to surgeons, for counselling patients whilst obtaining informed consent for operation.

Cervical spine degeneration in soccer players

The paper concerning cervical spines in soccer players by Kartal et al. [15], was a good example of the importance of reading the whole paper and not relying on the abstract or the title! The abstract correctly says that there was a tendency towards early degenerative change in the cervical spine in soccer players, but it is only when one reads the paper, one realises how questionable this is. The degree of trauma that the neck experiences, up to some 2,000 N each time the ball is headed, is impressive, yet the actual degree of change comparing controls with veteran players of more than 10 years is minimal; indeed, bulging of discs and osteophytes are commoner in controls. Even cord compression was commoner in the

control population, but was at a different level. The authors based their conclusions on the group of younger players, where there was a slight difference between controls and football players. After reading this paper, I would be very happy to reassure that playing football was safe for the neck.

Vertebroplasty and kyphoplasty

The review paper by Heini et al. [13] is an excellent review of the techniques of kyphoplasty, a comprehensive literature review and a brief report of personal experience. Dr. Heini has considerable experience with vertebroplasty and this allows him to make a very considered judgement concerning the use of kyphoplasty in osteoporotic fractures. His conclusion, that the expense of the device, the modest correction obtained, and the alternative technique of correcting kyphosis by postural reduction, or the insertion of pedicle levers in vertebrae above and below, make the use of balloon kyphoplasty in this disorder of questionable value.

The paper by Berlemann et al. [4] reports in greater detail the results in 24 patients, with a follow-up of 1 year. The relief of pain was remarkable, in all but one patient, and in the year following, further improvement occurred, although two deteriorated slightly. However, correction of kyphosis was not related to success and the authors make the point that the value of height restoration and correction of deformity has yet to be established. What was of interest was that in only half was pain relief immediate; in the others, pain improvement continued, especially during the next 2 months. This rather excludes the destruction of sensory nerves as being the cause of the relief of pain.

One presumes that the pain of fractured vertebrae is due to micro-fractures continuing and that the cement stops these by providing a buttress to the thin cancellous bone. Alternatively, the thin lamellae, placed now not in the line of weight-bearing, may be repeatedly bent abnormally, and this is painful. Could we stop low-back pain by similarly strengthening with cement the cancellous bone beneath an abnormally loading degenerate disc?

Anyone contemplating embarking on either kyphoplasty or vertebroplasty should read these papers, and will be stimulated to review other papers quoted, concerning vertebroplasty, which is now clearly an established technique, of great value to many patients.

Condition-specific outcome measures for low-back pain

The two papers concerning specific outcome measures for back pain by Müller et al. [18, 19] are, I believe, required

reading for anyone planning a prospective study concerning treatment of back pain and assessing results. For the non-statistician, the papers were somewhat hard going, but anyone embarking on a prospective study dealing with assessing patients, and the results of treatment, would find the statistics easy. It was of interest that of the 81 methods of assessing outcome in the literature, only nine had been fully validated, and all had advantages and faults. What was clear was that selecting the best outcome measure for a particular study was important, and which was best for any particular study depended on patient population and range of result? I particularly liked the clear exposition of the three domains, developed by the WHO, of impairment due to bodily loss or body dysfunction, impairment specific to the individual that is disability in daily living, domestic tasks and communication, and finally handicap due to interaction with society, that is work, social integration or sport. The nine currently used condition-specific outcome measures were all designed before the WHO introduced the International Classification of Functioning, Disability and Health, and one feels that the time is ripe for an outcome instrument incorporating in its design the WHO classification, to be developed.

Pregnancy and postpartum back pain

The review article on pregnancy-related pelvic girdle pain, by Wu et al. [23] is the first of two reviews, and deals with terminology, clinical presentation and prevalence. A second one is promised, dealing with pathology, diagnosis and treatment. Curiously it is not a disorder that we see in the UK in our back clinics, and, indeed, while physiotherapists treating pregnant and postpartum woman are aware of it, obstetricians I have spoken to do not view it as a significant problem. The authors suggest that doctors do not report it as they do not diagnose it, and most women, if it is mild, regard it as one other affliction associated with pregnancy. Although the overall incidence may be in the region of 45%, pain for which medical advice is sought is as low as 5%, which may explain why few find their way to a spinal clinic. Whilst prior back pain is a risk factor for postpartum pain, it is not a risk factor for pregnancy pain, a useful piece of information to be given to our back pain patients contemplating becoming pregnant. However, there is clearly a small group where postpartum back pain is of considerable clinical significance, and it was a couple of such patients I had to treat which made me aware of what a problem it can be, so I await the second paper with interest.

Post-traumatic kyphosis

The surgical management of painful post-traumatic thoracolumbar kyphosis is controversial. Is the pain

related to an associated degenerate disc, or is it related to deformity? The paper by Been et al. [3] was of great value, as it deals with a reasonably “pure” group, with no associated spinal cord injury; indeed spinal cord decompression was not present in any of the patients. The study deals with the results in 25 patients of corrective surgery, in terms of degree of correction achieved, pain relief, and what surgical approach was to be preferred: combined posterior and anterior, or anterior alone. The follow-up is impressive. The degree of pre-operative deformity was modest: in the group operated upon anteriorly alone, the median kyphosis was 23°; in those with combined procedures, the median kyphosis was less at 21°. Only seven patients had a kyphosis over 30°. The finding of the authors that getting a solid fusion was more important than the degree of correction in achieving a good clinical result as regards pain may be related to the fact that the pain was discogenic in the main, rather than related to the modest deformity. Clearly, in this group with modest deformity, anterior surgery alone sufficed, but the clinical results were modest: only one patient returned to their pre-injury back pain score. The authors concluded that there was no benefit in the combined approach. But it must be noted that the deformities treated were all modest, and the same opinion may not be appropriate for greater deformity. A very useful paper for surgeons contemplating operating for a modest painful kyphotic deformity in discussing likely outcome for the patient.

Back muscles

Physiotherapy is the most used therapy in the management of back pain. Whether it alters the natural history of the pathological basis of back pain is unproven, but it alters the disability that the pain produces. It is, therefore, always valuable to see papers dealing with the muscles—they tend to be few.

The clinically orientated study of back muscle by Helmhout et al. [14] dealt with a comparison between a low intensity and high intensity lumbar extension program in treating low-back pain. The intensity refers to the increased loads used in the high intensity (HIT) group, not the frequency of treatment, which was the same in both groups. Although there was a treatment effect insofar as the HIT group ended up with stronger back muscles, there was no clinical difference, and the authors conclude by agreeing with Marras [17] that physical restoration of moderate low-back pain using specific types of exercise seems not to be justified, and with Mannion et al. [16] that training induced effects like modification in pain perception and the analgesic effects of exercise are responsible for the improvement in spinal function.

These studies taken together certainly support the view that the patient who has the pain is more central to the problem than their muscles.

Spinal tumours and degenerative disease

One does not become an expert diagnostician by conducting prospective randomized trials; it is based on experience—one's own or others. I, therefore, liked this straightforward paper by Börm et al. [5], concerned with cases where the diagnosis of tumour had been missed. It is best to learn from other doctors' errors than one's own. The paper alerts us to the fact that tumours and degenerative disease in the spine may co-exist, and using the mantra that “common things occur commonly”, alas we often diagnose symptoms as being due to degenerative disease, and do not consider tumour. This paper illustrates how this is often the case, and it is lack of thinking and assessing symptoms carefully that allows us to fall into this trap. A careful history is essential in all patients with back pain, but note the significance of what the authors call “recumbency pain”—I prefer the term night pain—and also failure to correlate precisely the signs with the level of degenerative disease. MRI examination of the spine above the degenerate level was often the reason that a malignancy was diagnosed.

Anatomy

The paper on “Conjoined lumbosacral nerve roots” by Böttcher et al. [6] was somewhat of a disappointment to me. The outline of the types of abnormalities found was valuable, but the suggestion that such abnormalities were a cause of back pain was not substantiated. Indeed, of the three cases described, Case I, who had severe sciatica and back pain, got better with gymnastics, backstroke swimming and TNS, so the relation of the abnormality to the symptomatology was unproven. I was disappointed that the authors did not emphasise sufficiently that a major importance of conjoined roots is the risk of root injury during surgery, either due to the fact that the divided “adhesion” is in fact an anomalous root, or the fact that in removing a protrusion the degree of root tension created in mobilizing the root is greater, as the conjoined root is much less mobile. However, their comments concerning interpretation of MRI as the gold standard in identifying conjoined roots were pertinent. Clearly, whenever a disc is to be operated upon the surgeon must always consider the possibility of a conjoined root, not as a cause of symptoms, but as a hazard of surgery. It is implied that more major decompression may be required to achieve a satisfactory clinical result; I believe this is the case, but such a view can only be anecdotal.

Approaches to the lumbar spine

There were two papers dealing with a comparison of conventional versus minimally invasive retroperitoneal approaches to the anterior lumbar spine. The first by Saraph et al. [20] compares the approach developed by Mayer, essentially a muscle-splitting approach carefully targeted over the appropriate space, with the usual approach described by Fraser, where the external oblique is split, and the internal oblique is cut across its fibres. The second paper by Farooq et al. [8] describes a minimal approach, by a paramedian incision, entering through the posterior rectus sheath, the rectus being retracted laterally. Farooq used a balloon to aid the separation of the peritoneum from the anterior muscle, and also used a special retractor. Both papers demon-

strate that the minimal approach was quicker, and was associated with less blood loss, and there seemed no advantages associated with the conventional approach. The Saraph paper did not have the use of the specific retractor described by Mayer. The Farooq group had a specific retractor, and had the cost of the balloon. The majority of fusions were single level, but in the Saraph paper no comment is made as to how extensile the approach is. Mayer describes two incisions for adjacent levels, but in the Saraph paper it would appear that two levels were done through the one incision, slightly larger for the two levels. It certainly seems to be the case that the destructive approaches we used in the past are now not appropriate. One is somewhat doubtful about the need for a balloon costing 173 euros in the paramedian approach, but special retractors do have benefits.

References

1. Arinzon Z, Adunsky A, Fidelman Z, Gepstein R (2004) Outcomes of decompression surgery for lumbar spinal stenosis in elderly diabetic patients. *Eur Spine J* 13(1):32–37
2. Axelsson P, Karlsson BS (2004) Inter-vertebral mobility in the progressive degenerative process. A radiostereometric analysis. *Eur Spine J* 13(6):567–572
3. Been HD, Poolman RW, Ubags LH (2004) Clinical outcome and radiographic results after surgical treatment of post-traumatic thoracolumbar kyphosis following simple type A fractures. *Eur Spine J* 13(2):101–107
4. Berlemann U, Franz T, Orlor R, Heini PF (2004) Kyphoplasty for treatment of osteoporotic vertebral fractures: a prospective non-randomized study. *Eur Spine J* 13(6):496–501
5. Börm W, Gleixner M, Klasen J (2004) Spinal tumors in coexisting degenerative spine disease—a differential diagnostic problem. *Eur Spine J* 13(7):633–638
6. Böttcher J, Petrovitch A, Sörös P, Malich A, Hussein S, Kaiser WA (2004) Conjoined lumbosacral nerve roots: current aspects of diagnosis. *Eur Spine J* 13(2):147–151
7. Brambilla S, Ruosi C, La Maida GA, Caserta S (2004) Prevention of venous thromboembolism in spinal surgery. *Eur Spine J* 13(1):1–8
8. Farooq N, Grevitt MP (2004) “Does size matter?” a comparison of balloon-assisted less-invasive vs conventional retroperitoneal approach for anterior lumbar interbody fusion. *Eur Spine J* 13(7):639–644
9. Garcia-Erce JA, Muñoz M, Bisbe E, Sáez M, Solano VM, Beltrán S, Ruiz A, Cuenca J, Vicente-Thomas J (2004) Predeposit autologous donation in spinal surgery: a multicenter study. *Eur Spine J* 13 (Suppl 1):S34–S39
10. Gerlach R, Raabe A, Beck J, Woszczyk A, Seifert V (2004) Postoperative nadroparin administration for prophylaxis of thromboembolic events is not associated with an increased risk of hemorrhage after spinal surgery. *Eur Spine J* 13(1):9–13
11. Giannikas KA, Khan AM, Karski MT, Maxwell HA (2004) Sacroiliac joint fusion for chronic pain: a simple technique avoiding the use of metalwork. *Eur Spine J* 13(3):253–256
12. Hanley EN Jr, Shapiro DE (1989) The development of low-back pain after excision of a lumbar disc. *J Bone Joint Surg Am* 71(5):719–721
13. Heini PF, Orlor R (2004) Kyphoplasty for treatment of osteoporotic vertebral fractures. *Eur Spine J* 13(3):184–192
14. Helmhout PH, Harts CC, Staal JB, Candel MJM, de Bie RA (2004) Comparison of a high-intensity and a low-intensity lumbar extensor training program as minimal intervention treatment in low back pain: a randomized trial. *Eur Spine J* 13(6):537–547
15. Kartal A, Yildiran I, Senköylü A, Korkusuz F (2004) Soccer causes degenerative changes in the cervical spine. *Eur Spine J* 13(1):76–82
16. Mannion AF, Junge A, Taimela S, Muntener M, Lorenzo K, Dvorak J (2001) Active therapy for chronic low back pain: part 3. Factors influencing self-rated disability and its change following therapy. *Spine* 26(8):920–929
17. Marras WS (2000) Occupational low back disorder causation and control. *Ergonomics* 43(7):880–902
18. Müller U, Duetz MS, Röder C, Greenough CG (2004) Condition-specific outcome measures for low back pain: Part I: validation. *Eur Spine J* 13(4):301–313
19. Müller U, Röder C, Dubs L, Duetz MS, Greenough CG (2004) Condition-specific outcome measures for low back pain: part II: scale construction. *Eur Spine J* 13(4):314–324

-
20. Saraph V, Lerch C, Walochnik N, Bach CM, Krismer M, Wimmer C (2004) Comparison of conventional versus minimally invasive extraperitoneal approach for anterior lumbar interbody fusion. *Eur Spine J* 13(5):425–431
 21. Schaller B (2004) Failed back surgery syndrome: the role of symptomatic segmental single-level instability after lumbar microdiscectomy. *Eur Spine J* 13(3):193–198
 22. Spratt KF, Keller TS, Szpalski M, Vandeputte K, Gunzburg R (2004) A predictive model for outcome after conservative decompression surgery for lumbar spinal stenosis. *Eur Spine J* 13(1):14–21
 23. Wu WH, Meijer OG, Uegaki K, Mens JMA, van Dieën JH, Wuisman PIJM, Östgaard HC (2004) Pregnancy-related pelvic girdle pain (PPP), I: terminology, clinical presentation, and prevalence. *Eur Spine J* 13(7):575–589
 24. Zucherman JF, Hsu KY, Hartjen CA, Mehalic TF, Implicito DA, Martin MJ, Johnson DR II., Skidmore GA, Vessa PP, Dwyer JW, Puccio S, Cauthen JC, Ozuna RM (2004) A prospective randomized multi-center study for the treatment of lumbar spinal stenosis with the X STOP interspinous implant: 1-year results. *Eur Spine J* 13(1):22–31