

Journal of Surgical Orthopaedic Advances

LETTERS TO THE EDITOR

RE: Adolescent Idiopathic Scoliosis: Modern Management Guidelines, Shindle, M.K., Khanna, A.J., Bhatnagar, R., Sponseller, P.D., JSOA 15(1):43–52, 2006

Dear Editor:

There are some issues with this manuscript that I think worth pointing out:

- 1. The stable zone of Harrington is mentioned on page 43 and seen in Figure 2 with the comment "it often indicates the recommended distal extent of a posterior spine fusion." In the figure, any vertebra from T10 to S1 could be the distal extent of the fusion, so I do not see why the authors feel this measurement of any value.
- 2. On page 46 under Natural History, the paper by Weinstein et al. is quoted as saying "untreated AIS patients were able to lead productive lives with minimal physical impairment." This is a gross oversimplification of a complex paper since only 50% of the cohort were located and studied, many patients had had to have surgery, and many had died or were in cor pulmonale due to their large thoracic curves. As a spine surgeon working in Minnesota, I had the opportunity to treat many of these sadly neglected Iowa patients so I know personally the deficits of this publication.
- 3. On page 49 under Procedures, paragraph 3, thoracoscopic anterior instrumentation is stated to be indicated for "single thoracic curves measuring $40^{\circ}-70^{\circ}$." Since these are the same indications for a posterior surgery, I can't understand what the advantages are. Yes, it avoids an open thoracotomy, but adds the complications of a 6-hour surgery, high pseudarthrosis rate, high X-ray exposure rate, the need for an orthosis postoperatively, and the problems associated with opening the chest (thoracoscopically) such as pneumothorzx, atelectasis, etc. A posterior procedure will accomplish an equal correction, a shorter operating time, far less irradiation, no risk of pneumothorax, no need for a brace, and fewer complications.
- 4. Figure 8 on page 48 shows a posterior operation which is too long. There was no need to go higher than T5.
- 5. On page 50, paragraph 1, line 16 states that Cotrel/ Dubousset instrumentation consists of "a derotation maneuver whereby the spine is rotated about the construct." This is not true. The rod is rotated in order to translate the apex of the curve toward the midline

and out of lordosis into kyphosis. Multiple studies have shown that the vertebrae are not derotated, only translated.

- 6. Also on page 50, paragraph 2, line 12, it states that the use of pedicle screws "allow[s] for a greater area of arthrodesis." What do the authors mean by this? The two quoted articles by Suk do not address this question.
- 7. On page 50, paragraph 3, line 3, it quotes a reference regarding the neurological injury rate. The reference quoted is not applicable, being an overview, not a scientific article dealing with neurological injuries.
- 8. Finally, on page 50, paragraph 5, line 3, the term "pseudo-King II curves" is used. There is **no such thing** (I was one of the original authors of the King et al. paper). The decompensation problem mentioned was due to a too-long fusion area, the distal fixation point being at or close to the apex of the secondary lumbar curve.

I apologize for the length of this letter, but I didn't want to skip important items.

Robert B. Winter, MD University of Minnesota

In Reply

Dear Editor:

Dr. Winter's letter raises several issues which merit further discussion than was provided in the article. We have responded to his points as he enumerated them.

- 1. The stable vertebra is only one of the several parameters in determining the distal extent of fusion; others include rotation of the end vertebra, anticipated correction, and the character of any distal curve.
- 2. The article by Weinstein et al. is indeed a landmark work. Any interested parties are recommended to read it in its entirety in the 2003 *Journal of the American Medical Association*, as referenced. This article paints as comprehensive a picture as possible of the 50year health status of people with unoperated adolescent idiopathic scoliosis, including orthopaedic status of these patients. The incomplete follow-up of the cohort is a weakness already acknowledged by the authors. However, a multi-year exhaustive attempt was made to locate all patients, even those who sought treatment elsewhere, and no overt selection bias seems evident.

Readers can also see the editorial by Sponseller in the same issue of *JAMA*, entitled, "Sizing up Scoliosis," which provides comment on the article. Dr. Winter's experience in treating people from Iowa who chose to visit him is only a small part of the picture.

- 3. Thoracoscopic instrumentation of scoliosis is an evolving procedure. Dr. Winter acknowledges the weakness of the technique, and his personal opinion. It remains under development. Improving results seem to be forthcoming, as reported by Lonner in the May 2006 issue of the *Journal of Bone and Joint Surgery (American)*. Clearly, more time and perspective are needed to evaluate the role of this technique.
- 4. Dr. Winter provides another perspective on how he would have treated this curve.
- 5. It is indeed true that the vertebrae are not rotated in a derotation maneuver — only translated in the other two planes. More recent techniques such as direct

vertebral rotation do, however, seem to provide this method of correction.

- 6. The use of pedicle screws allows for potentially more in-line and compact placement of vertebral anchors. However, the main value of pedicle screws is their improved fixation.
- 7. The true neurologic injury rate in idiopathic scoliosis surgery seems to vary from study to study. Therefore, we have provided an overview since individual studies capture only certain populations.
- 8. Thank you for clarification of your use of the King– Moe classification from one of the original authors.

Paul Sponseller, MD A. Jay Khanna, MD Michael Shindle, MD