

LETTER TO THE EDITORS

SERIAL CASE REPORTING YOGA FOR IDIOPATHIC AND DEGENERATIVE SCOLIOSIS

Dear Editor:

We would like to compliment the authors for this interesting attempt to study the effects of a specific yoga pose on scoliosis.¹ We strongly support research providing good evidence in relation to conservative scoliosis treatment.² Nevertheless, we have serious concerns about this paper's results and conclusions. It has been considered worldwide by various media as proof of yoga's effectiveness but has too many flaws to engender confidence.

Inclusion criteria are not consistent with the literature: (1) Curves that are not scoliosis were included: by definition, 6 degrees Cobb is not scoliosis,^{3,4} and we don't know if Adam test, a crucial sign to define scoliosis, has been performed.⁵ This diagnostic flaw completely impairs the content of the paper. The curve pattern confirms this impression, being half right thoracolumbar, which is very infrequent in real scoliosis but not in postural imbalance. (2) Adolescent and adult scoliosis are both included. The former has a high risk of sudden progression, while the latter does not. These totally different populations are never treated together in scientific papers.³ (3) Idiopathic and degenerative types of scoliosis are mixed: again, these relate to very different populations. Degenerative scoliosis is usually lumbar, not thoracolumbar,⁶ and is much less severe than idiopathic scoliosis.

Other relevant limitations include the following: (1) The follow-up radiograph schedule is unclear, ranging from 3 to 22 months, with very short- and medium-term results mixed together. (2) Due to the progressive trend of adolescent scoliosis, end growth evaluation is needed to evaluate the effectiveness of treatment. (3) The authors define secondary curves as more severe than primary ones. This is surprising and contrary to the literature; it is not clear how they were defined. (4) Noncompliant patients had 0.45% positive results performing the pose 4 times or fewer per week, whereas compliant patients experienced 40.9% positive results with 5 to 7 repetitions. This is a striking difference. How was the threshold for compliance defined and was it done *a priori* or post-hoc? (5) The claim of better quality of life (QoL) with yoga versus bracing is not supported by existing data. A recent randomized controlled trial⁷ clearly demonstrated that bracing doesn't reduce QoL compared to observation.

Based on these relevant methodological flaws, the authors should be more cautious: yoga cannot be considered an alternative to any effective treatment³, such as physiotherapeutic scoliosis-specific exercises (PSSE), bracing, and surgery. The correct conclusion of this paper should have been that the use of yoga warrants further study and that it is not possible to draw any conclusions about its effectiveness. Moreover, it should be

studied in contrast to PSSE, the standard of care for scoliosis treatment not requiring bracing.^{3,8} Today, yoga is considered a generalized physiotherapy not specific for scoliosis⁹ as it lacks self-correction, the most important and specific part of scoliosis rehabilitation.¹⁰ Conversely, some yoga poses (like the one studied here) could significantly increase the generic stabilizing effect of exercises,⁸ another key point of PSSE.¹⁰

Minnella Salvatore, MD, Zaina Fabio, MD, Romano Michele, PT, and Negrini Alessandra, PT, ISICO (Italian Scientific Spine Institute), Milan, Italy; Negrini Stefano MD, Prof, Department of Clinical and Experimental Sciences, University of Brescia, Italy, and IRCSS Don Gnocchi Foundation, Milan

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Author Response

Dear Editor:

We are pleased to have the opportunity to respond to the thoughtful letter by Dr Salvatore et al. To our knowledge, this is the first study of yoga as a treatment for scoliosis. We were motivated to publish this by Dr Fishman's clinical successes in his private practice. As a result, we were subject to the substantial limitations of a case series from a private medical practice. We acknowledged those limitations in our report and called for more rigorous future studies. We listed some of the design features of more rigorous trials that would be necessary in order to determine whether a yoga intervention should be a more routine part of standard care for persons with scoliosis. We think this background is important in responding to the comments made by Dr

Citation

Global Adv Health Med.
2015;4(1):79-80. DOI:
10.7453/gahmj.2014.084

Salvatore and colleagues. We thank the authors for their comments related to design features of a more definitive clinical trial.

We included one scoliotic curve of 6 degrees; all others were 10 degrees or higher. Exclusion of that one individual would not have changed our findings (average of 46.4 degrees for adult and 23 degrees for adolescent idiopathic scoliosis [AIS]). Scoliosis is usually defined as a curve of 10 degrees or more, and in that sense, the 6 degree curve should have been excluded from our series. However, a great many conditions warrant treatment before they progress, such as osteopenia or an undefined mass, even though some people with these conditions would not progress untreated. That said, a more rigorous clinical trial would benefit from following the 10-degree standard.

We did, in fact, perform the Adams test on the patients in our series. We note that it is not a perfect test, having a sensitivity of .94 but specificity of only .60.¹ Sixty percent of our patients had left-sided scoliotic curve. We used x-rays to determine the angle of the curve in all cases. The radiologists were not aware that these individuals had practiced yoga or would be reported in a case series. We believe that the use of X-rays is a reasonable method of assessing progress, although we agree that including end growth analysis would be important in a clinical trial.

We asked patients to practice daily, so defining noncompliant patients as those who practiced on fewer than half of the days each week, ie, 3 or fewer, seems reasonable and likely conservative. Nonetheless, because this was done after the data were collected, we strongly believe these findings need to be repeated in a larger sample with more rigorous measures of compliance. We recommended this in our article.

While we chose to include both AIS and adult scoliosis in the same report, we did present some of the results separately (Table 3). We suspect that some of the adult scoliosis cases may in fact have been untreated AIS but acknowledge that it would not always be possible to know the origin of the adult scoliosis. While we found that both groups responded favorably to the yoga, we would plan to focus on each subtype separately in future rigorous clinical trials.

We agree that having a vastly different timeframe for follow-up is indeed a limitation, which we would correct in a clinical trial. But we think our observation that a single pose appears to have markedly beneficial effect on two types of scoliosis, measured at different time intervals, is intriguing and worthy of future, substantially more rigorous study.

The letter authors asserted that we “define secondary curves as more severe than primary ones,” possibly because the mean of the primary curves was slightly smaller than the mean of the secondary curves. However, this is simply an artifact of 18 patients not having a secondary curve, and secondary curves tended to be present in patients with more severe primary curves. Thus, the primary curve was defined as the

more severe curve, as is standard practice.

While Dr Salvatore and colleagues assert that higher quality of life with yoga vs bracing “is not supported by the literature” and cite a recent trial, we found several European studies that reported a profound loss of self-esteem and change in body image in adolescents contemporaneously with their using braces for scoliosis.²⁻⁴ Our article never stated that we measured or had shown better quality of life with yoga vs bracing in our study.

In summary, we believe that our results are provocative and worthy of further study but do not provide definitive evidence of yoga’s effectiveness for scoliosis. We believe that we devoted sufficient space in the Limitations and Future Research sections of the article to make this clear.

Loren M. Fishman, MD, Columbia College of Physicians and Surgeons, New York; Erik J. Groessl, PhD, University of California San Diego, VA San Diego Healthcare System; Karen J. Sherman, PhD, MPH, Group Health Research Institute, Seattle, Washington

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ERRATUM

The quote, “Self-diagnosis and self-treatment may result in self-malpractice,” that appeared in the Editorial in our November 2014 issue (*Global Adv Health Med*. 2014;3(6):5-6) should have been attributed to Gregory Plotnikoff, MD, MTS, FACP, rather than Ruth Lindquist et al. *Global Advances in Health and Medicine* regrets the error.