Reviewer's report

Title: Treatment of adolescent idiopathic scoliosis with segmental pedicle screws and combined local autograft and allograft bone for spinal fusion

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Reviewer: Jan Hartvigsen

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Thank you for giving me the opportunity for look at this submission which is a report on a series of cases of operative treatment of adolescent idiopathic scoliosis. The data are derived from a retrospective file review from a department of orthopedic surgery in Shanghai, China.

Not being an orthopedic surgeon myself, I will restrict my review to the scientific issues in the paper. I cannot comment on whether the clinical findings are of relevance but they appear to be.

First, it should be clarified in the title that this is a retrospective case-series. Therefore by definition the inclusion criteria are somewhat liberal which is a scientific weakness but on the other hand a reflection of the clinical reality. Furthermore the number of patients is relatively small. Therefore – if the paper is published – it is to be considered as preliminary evidence without much scientific weight. This should be mentioned specifically in the discussion.

All operations were performed by the same surgeon which is a weakness and should be discussed specifically since it limits the generalizability of the results.

The most important weakness of the paper from my point of view is that there is no account of the evaluation of the outcome measures. How was it performed (for instance the measurement of back pain)? Was it systematic? Are these measurements reliable and valid in relation to pseudoarthrosis? Who did it (was it the surgeon who had performed the surgery or a blinded person)? How were differences between baseline and follow-up calculated? Were there really no missing data (I find that hard to believe in hospital files)? All of these issues need to be clarified in the text.

I find it very difficult to believe that “no patients had complaints of back pain”. That would make them healthier than the general population in spite of extensive back surgery!

Table 1 and 2 can be collapsed into one which in turn should be expanded to account systematically for the outcome measures.
If the authors are able to deal with all the above issues, in my opinion the conclusion should be rephrased into something like: This retrospective case-series indicates that in situ autograft bone combined with allograft bone may be a promising method to be used in AIS treated with pedicle screw placement. Further prospective studies using systematic inclusion criteria, systematic follow-up and control groups, preferably in a randomized design, are needed to determine just how useful this procedure is.