Reviewer's report

Title: Preoperative assessment and evaluation of instrumentation strategies for the treatment of adolescent idiopathic scoliosis: computer simulation and optimization

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Reviewer: Clayton Adam

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The authors present a computer simulation study in which the ‘ideal’ surgical correction profile for a single scoliosis patient was identified by 11 different experienced surgeons. The extent to which different surgical strategies achieved the desired correction objectives were then explored by running multiple computer simulations of the correction procedure. The study concludes that different surgeon-specified objectives can result in different optimal surgical strategies. Overall, this is a well performed study which demonstrates the growing ability of computer simulations to assist in surgical decision making.

Major compulsory revisions

1. Clarification of terms in objective function – The objective function is defined as a weighted sum of squares of post-op to pre-op ratios in which each square term tends to zero as that correction objective is ‘optimised’. However in the case of the two sagittal plane angles (thoracic kyphosis and lumbar lordosis), these terms are referenced to arbitrary ‘normal’ values of 35 and 40 degrees respectively. I am concerned that in the case of AIS patients who may already be normokyphotic (i.e with preop kyphosis in the normal range), the denominator will tend to zero and the term will therefore become infinite. Please address this.

   Also, the F/Fo ‘mobility factor’ ratio (num unfused vertebrae/ max num unfused vert in all strategies) appears problematic because as the number of unfused vertebrae approaches the maximum for all strategies, this term approaches 1 from a value less than 1, therefore this term should not be minimized but rather needs to be maximized. Again, please address.

2. Comment on model validation – Although the authors have a strong track record in the development of computer simulations of scoliosis surgery, I still believe it is necessary to include specific comment on model validation in the current study, and particularly on the potential limits of model validation when running large numbers of simulations (702 in this case) across a wide ‘solution space’. Can we have equal confidence in the model predictions across all instrumentation strategies? Or are there some instrumentation combinations where the model’s ability to correct predict post-operative spinal shape has been less thoroughly tested against actual patient data? In other words, what is the confidence in the model validity across the solution space investigated in this study? How well did the model predictions for this patient match the actual
surgical outcomes for the strategy used with this patient? At the very least a paragraph on model validation and a statement detailing the author's level of confidence in the predictions across the parameter space explored in this study should be included in the Discussion.

3. Additional methodological details – While it is appreciated that the paper refers to previous model development work by the authors for some aspects of the model development, additional methodological details for the single patient simulated in this study are still required. In particular, how was the patient-specific anatomy generated given that the radiographs in Figure 1 seem to be of quite low contrast (there is almost no visible endplate definition in the sagittal radiograph)? Also, the authors seem to imply that patient-specific spinal stiffness values were determined for this particular patient – is this the case? Thirdly, what was the goodness of fit between the ‘simplified model representing the 12 geometric measurements as a function of the six instrumentation variables’ and the actual outputs of the S3 model? What were the maximum errors between S3 output and simplified model output? Were the errors significant relative to the quantities being optimized? What loading was applied to the models? Please give a statement in the Methods on how the models were ‘loaded’ when adding the instrumentation.

4. Discussion of model limitations – The choice of boundary conditions applied to the spine model has already been discussed by the authors as a limitation. Please also make the point that the analysis performed in this study only considers geometric aspects of the deformity correction, and there is no consideration of predicted biomechanical quantities in the study. For instance, some of the instrumentation configurations simulated might be more prone to implant loosening or inducing junctional kyphosis at the proximal uninstrumented than others under post-operative loads, but these biomechanical factors have not been considered in the identification of ‘optimal’ surgical outcomes in the present study.

Minor essential revisions

5. Inconsistencies between text and Tables – Results paragraph 2 states that the upper instrumented level varied from T2 to T6, whereas Table 2 shows the range as T2 to T5. Similarly the range of lowest instrumented vertebrae is stated as L1 to L5 in the text and L1 to L4 in Table 2. The PT region Cobb angle range is stated as 26 and 42 degrees in the text, where in Table 3 it is given as 26 to 40 degrees, and similarly for the TL/L Cobb angle range which is given as 13 to 28 degrees in the text and 13 to 27 degrees in the Table. Please correct these inconsistencies.

Discretionary revisions

6. Figure 2 seems redundant as it is very difficult to see the differences between instrumentation strategies at the scale of the images given. It may be better to include 2 (larger) images which show the difference in surgical strategy and post-operative spinal shape between two of the surgeons with clear differences
between them.

7. Abstract, Methods subheading, sentence 2 is not clear ‘For each surgeon, 702 surgical configurations were simulated and eleven were sorted out which were the most favourable…’ This sounds like 11 configurations were selected for each surgeon, rather than that each of the 11 surgeons selected his particular preferred surgical correction outcome. Please rephrase more accurately.

8. Background last sentence, ‘was examined’.


10. Methods first paragraph, please give the thoracic kyphosis and lumbar lordosis angles for the patient.

11. Methods 4th page, line 9 ‘but its parametric formulation will allow the use of more detailed data…’.

12. Discussion last paragraph, lines 7/8, ‘further studies are required to elucidate their role’.

13. Conclusions line 1 ‘our study’.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests