Author’s response to reviews

Title: Modular stem in total hip arthroplasty for patients with trochanter valgus deformity: surgical technique and case series

Authors:

Xiangpeng Kong (18810999609@163.com)
Wei Chai (chaiweiguanjie@sina.com)
Minzhi Yang (yangminzhi1995@163.com)
Alvin Ong (alvin.ong@rothmaninstitute.com)
Jiying Chen (361782992@qq.com)
Yan Wang (wanyanguanjie@sina.com)
Yonggang Zhou (zhouyg301@sina.com)

Version: 1 Date: 17 Dec 2019

Author’s response to reviews:

Dear Editors and Reviewers,

Thank you for taking the time to review our manuscript. We appreciate your valuable feedback and insights, which are the important guiding significance. We will try our best to make the article meet the requirements of BMC Musculoskeletal Disorders sincerely. Revised portion are red with underline in the manuscript. The responses (red) to the editor and reviewers are as flowing:

Editor Comments:

The Reviewers are positive, although this Editor has serious concerns that must be addressed.

Thank you very much for the comments. We have read the comments carefully and made corresponding correction, which we hope to meet with approval.
Table 1 makes no sense, please correct it.

Thanks for your suggestion. We have simplified the table and removed some useless or duplicate data. Please see table 1 on page 14.

Do the authors have consent for publication with the clinical images used in this manuscript? This is necessary when showing clinical images with details of patients' medical information. Please remove medical records info otherwise.

Thanks for your reminder. We have got the consent for publication before reviewing the medical information and clinical images. We also added it in the declaration section. Please see lines 268-269 on page 10.

If this study was retrospective, then how did the authors prospectively have ethical approval for a technique not yet reported in the literature. Kindly explain this, i.e. what was the evidence base for this new procedure without literature?

Thanks for your question. We got the ethical approval just for the retrospectively reviewing the patients’ medical information and publishing. Although there was no literature, the corresponding authors have delivered several lectures about this technique and been recognized by the experienced experts. Thus, we were given the ethical approval to evaluate the technique by the ethics committee.

Please explain why a case-control study was not possible. it is still to be determine whether the lack of a control group in a submission advocating a new technique should be rejected automatically.
Thank you for catching this important point. Lack of the control group is one major limitation of this study and will inevitably affect its persuasiveness. The significant limitation was emphasized in lines 244-246 on page 9. Because the patients enrolled in this study were rare (about 5000 cases of primary hip replacement were performed between 2006 and 2014 in our institute, only 12 patients met the inclusion and exclusion criterion), it is not suitable for prospective RCT. Indeed, in the early period of this retrospective study, we tried to set the control group, but no comparable patients with such severe trochanter valgus deformity were found. We just aimed to introduce one potential alternative for the surgeons when facing the similar cases. Hope you can understand and accept our explanation. Please do not hesitate to contact us if having any questions.

Reviewer reports:

Dejin Yang, M.D. (Reviewer 1): The authors described the surgical technique and results of using a modular stem system for THA patients with valgus deformities at the inter-trochanteric or sub-trochanteric area. It is an interesting and well-organized paper. Overall, I suggest the editorial office to publish this paper after a proper revision.

Thank you for your support and suggestions.

My comments are listed.

1. The technique described by authors is commonly used for this specific group of patients. Nowadays, an angular osteotomy on the proximal femur is not suggested in hip preservation surgeries for the consideration of added difficulties for potential future THAs. But this specific deformity is still occasionally seen in this region in conversion THAs for DDH with previous surgeries, which accounts for a significant challenge. When introducing the technique as a successful solution for this kind of patients, it is necessary to emphasize the technical difficulties to remind surgeons who are still performing angular osteotomies on proximal femurs even in a salvage surgery for hip preservation.

Thank you very much for your valuable suggestions and reminding the readers of the underlying causes of this deformity. Indeed, the treatment of malformation is only a stopgap measure, prevention is the key to preventing such catastrophic deformities. This suggestion (how to proceed from the source to prevent iatrogenic proximal femoral deformity) sublimates the idea of this article. We added this part into the discussion section. Please see lines 238-243 on page 9.
Exactly as mentioned in the results, the intra-operative fracture rate (2/12) is not as low as the rate for a normal patients group. It is not appropriate to conclude it as a simple surgery in the conclusion of the paper, even though we can see the authors did a great job. And this difficulty should be further discussed in the discussion part.

Thank you for the objective comments. In this study, 2 patients had intra-operative fracture, which was higher than the rate of normal THA. In this point of view, the “simple” was not appropriate for the conclusion. We have deleted the “simple” from the conclusion and added the difficulty in further discussion. Please see lines 75 on page 3, lines 255 on page 9, lines 225-229 on page 8.

2. S-ROM is a modular stem system with a porous sleeve (with a triangle spout) or a cone (without a spout). The sleeve can be locked with the stem with any version, as described in its surgical technique note (“The S-ROM sleeve can be placed in 360 degrees of version”). The word "reverse" is actually not accurate here in the title. I suggest the authors to highlight the specific deformity (trochanter valgus deformity) and the modularity of the stem in the title, such as "Modular stem in total hip arthroplasty for patients with trochanter valgus deformity: surgical technique and case series".

Thanks for your suggestion. You are right, the title was really not suitable for the main content of this article. We have changed the title as you suggested. Please see lines 2-4 on page 1.

3. S-ROM system provides an option with a sleeve as well as a cone for the proximal fixation and further bone integration. Another solution for those cases described by the authors is using a cone. With a cone, it has the advantages including easier bone preparation, less bone loss in the trochanteric area and less stress focusing at the tip of the sleeve; but it has potential disadvantages including less anti-rotation stability especially for the cases needing subtrochanteric osteotomy. This should be mentioned.

Thanks for your suggestion. We have added the related part about “cone” in the discussion section. Please see lines 216-219 on page 8.
4. In case of using a sleeve with nearly 180 deg version, it's not possible to do an instructed calcar reaming. Besides the method described by the authors, it is also reasonable to use a BUR for bone preparation for the spout placed towards the great trochanter, which I believe could provide more accuracy and cause less bone loss.

Thank you very much for the empirical advice. We revised the discussion section and will apply the bur for bone preparation in future cases as you suggested. Please see lines 225-229 on page 8.

5. Page 5, lines 32-34: "Up to now, no other study has described this special sleeve-implanting method and analyzed its clinical outcome systematically." This sentence should be moved to the instruction or deleted.

(BTW, the line numbers are so confusing, and I tried my best to make it accurate.)

Thanks for your comments. We have deleted the sentence “Up to now, no other study has described this special sleeve-implanting method and analyzed its clinical outcome systematically”. Please see patients and methods section on page 5.

6. Page 6, line 2: "Because there was no instrument to managing sleeve spout in the greater trochanter side, the surgeon handled the spout manually." Is it "sloop"? The prominent part of the sleeve is often called "spout" and a "reamer" is often used to prepare the calcar. Make is clear here. And as I mention above, a bur may help in this step.

Thanks for your suggestion. We had changed the “sloop” to “spout” and revised in the manuscript. We will apply the bur for bone preparation, not manual work, in future cases as you suggested. Please see line 124 on page 5, lines 145-146 on page 6, line 376 on page 15.

7. Page 6, line 23: "The patients were followed up at 4 and 12 months after surgery." Why was this follow up protocol? It is specific for this group of patients or not?
Thanks for your comments. In our institute, we set the follow-up protocol (the patients were followed up at 4 and 12 months after surgery) for the DDH people, which was different from the protocol for osteonecrosis of the femoral head (the patients were followed up at 3 and 12 months after surgery). Due to the congenital dysplasia of skeletal muscle and previous surgical history, the recovery of postoperative function in these patients may be slower than that of non-congenital patients. So we set the specific follow-up protocol.

8. Page 6, line 39-50: the last paragraph before the statistical analysis statement. The details of measurement should be moved to the figure legends for a friendly reading flow and better comprehension of the measurement with the help from illustrations.

Thanks for your suggestion. We have moved the details of measurement to the figure legend. Please see lines 383-388 on page 16.

9. In the last sentence of the method, the classification or grading of ICC is not necessarily declared. And the second last paragraph in the results should be moved here at the end of the methods, as a concise statement of the ICC measurement and the ICC value.

Thanks for your suggestion. We have changed the structure of the manuscript according to your guidance. Please see lines 172-177 on page 7.

10. Page 7, line 36-41: the statement about G/L. It is not a RESULTS part except the first sentence. It is a part of discussion. What is more, I don't think it is appropriate to take it as "reverse sleeve", it is just a sleeve with nearly 180 deg version or more accurately a sleeve towards the great trochanter rather than sitting on the calcar.

Thanks for your suggestion. We have deleted the sentence. Please see lines 195-196 on page 7. Most “reverse sleeve” was changed to other expressions in the manuscripts and only a few “reverse” was kept. Please see the title on page 1. lines 65-66 on page 3. line 107 on page 4. line 115 on page 5. lines 126-127 on page 5. line 131 on page 5. line 145 on page 6. lines 209-210 on page 8. line 233 on page 9.
11. Page 8, line 17: "The long-term clinical outcome was significantly inferior to common THA". This is probably not due to subtrochanteric osteotomy but the severity of the disease or deformity or technique difficulties. Subtrochanteric osteotomy is still necessary in certain cases with a need of careful technique. Even for some valgus deformities, to reduce the position of GT to facilitate the offset of the muscle, or to make the insertion of a stem possible in case of an severe angular deformity stopping any stem to pass, a subtrochanteric osteotomy is still indicated. This should be clarified and added to the discussion.

Thanks for your suggestion. It is true that the sentence "The long-term clinical outcome was significantly inferior to common THA" is somewhat unfair and does not take the severity of disease into consideration. We have revised the sentence. Please see lines 204-207 on page 8. Indeed, this technique can’t solve all valgus deformities, and some patients with TVD still need osteotomy to facilitate the offset and straighten the medullary cavity. We have deleted the related sentence and added this insightful view to the discussion section. Please see lines 220-223 on page 8.

12. Page 8: "Secondly, additional fixation and special prosthesis were needed to achieve satisfying initial stability." This is also not appropriate. S-ROM is still a good solution for cases with subtrochanteric osteotomy

Thanks for your suggestion. Sorry for our inappropriate expression. We have revised the sentence. Please see lines 204-207 on page 8.

13. Overall, I suggest authors to delete the 2nd and 3rd paragraph of the discussion, or make it concise, as they are not directly related to the results or just common sense.

Thanks for your suggestion. We have deleted the most sentence in the 2nd and 3rd paragraph of the discussion. Please see lines 204-207 on page 8.

14. Page 8, line 42: "arbitrarily"? How about "freely"? In the same sentence, "usually" or "seldom"?
Thanks for your suggestion. Sorry for our error. We have changed to "freely" and "seldom". Please see line 209 on page 8.

15. Once again, in the discussion of the technique, a cone provided as an alternative should be mentioned. And in the conclusion, obviously it is not a simple technique. And G/L seems not important to mention in conclusion, as the sleeve rotating freely is a well-known design. But in contrast, a suggestion of technique caution including burs or other certain tools should be standing by for bone preparation, or a cone should be considered especially when an osteotomy is not planned (anti-rotation stability would be weaken by the osteotomy).

Thanks for your comments. We have mentioned the cone as an important part of discussion. Please see lines 216-219 on page 8. We also deleted the “simple” and “G/L” from the conclusion. Please see lines 255-256 on page 9. We have added the bur as the useful tool for bone preparation. Please see lines 228-229 on page 8.


Thanks very much for your careful reading. Sorry for our negligence. We have asked an native speaker to help us with the grammar problems. Please see line 60 on page 3.

Jianlin Xiao (Reviewer 2): Generally speaking, manuscript content is a topic of interest to researchers in related fields, with moderate significance. The manuscript design is reasonable, the method is acceptable, and the cases provided are also clear. Hence, I suggest that this paper is accepted without further modification.

Thank you for your support and kindness. It's a great honor to receive your recognition.
Youn-Soo Park (Reviewer 3): Severe trochanteric valgus deformity is a challenging situation for THA. In this study, Kong et al. retrospectively reviewed 12 cases of THA with S-ROM modular stem using reverse sleeve technique for degenerative hips with trochanteric valgus deformity. The authors reported that the implant survivorship with an end point of revision for any reason was 100% at 6 years. There was no aseptic stem loosening, and Harris hip score improved from 34.3 preoperatively to 84.1 postoperatively.

Although the reverse sleeve technique has been discussed in several conferences and review articles, no clinical study had specifically focused on this technique. The main limitation of this paper appears to be the small number of cases. Nevertheless, this article is worth publishing, as the trochanteric valgus deformity of G/L ratio greater than 1.50 is extremely rare in THA. Overall, it is a well performed study. However, there are some minor issues and concerns that need to be addressed before publication.

We are very glad that our research has won your interest and support. We will try our best to address your concerns and inappropriate statements.

1. Femoral stems with proximal modular sleeve design are currently not popular in ordinary primary THAs. Therefore, simply expressing "reverse sleeve in THA" in the title can be somewhat vague and confusing. It is also worth mentioning that these are severe cases of trochanteric valgus deformity, as there is no need for this technique in mild cases. I recommend modifying the title to "The reverse sleeve technique in total hip arthroplasty with cementless modular stem for patients with severe trochanter valgus deformity: surgical technique and case series" or similar expressions.

Thanks for your kind suggestion. We have changed the title as your recommendation. Please see the title on page 1.

2. Page 5, lines 7-9: It is important to describe the total number of cases with the same technique during the study period, not just mentioning, "the patients who hadn't have regular follow-up and complete information were excluded." Please address the minimum follow-up duration as well as the number of cases that could not be evaluated in the current study.

Thanks for your comment. We have added the total number and the excluded number of cases during the study period. Please see lines 115-117 on page 5.
3. Although the authors found that the actual indication of the reverse sleeve technique was G/L ratio greater than 1.50 retrospectively, please state clearly what was the "preoperative indication" of applying this technique. For example, "The reverse sleeve technique was considered in patients with severe trochanteric valgus deformity, where the modular stem could not be inserted by ordinary technique without performing varus osteotomy simultaneously."

Thanks for your friendly reminder. As you mentioned, these patients were retrospectively analyzed and their measurement data were summarized to find the actual indication. However, the indication was unknown before this study. In clinical practise, we usually used film (before 2014) or template software (after 2014) for preoperative plan. According to the plan of the patients with severe trochanteric valgus deformity, we will consider to implant the sleeve reversely when the modular stem could not be inserted by the ordinary technique without corrective osteotomy simultaneously. We have revised the related section in the manuscript. Please see lines 129-132 on page 5.

4. S-ROM modular stem has the polished central stem and the porous- or HA-coated sleeve. Osseointegration in the proximal sleeve is critical for long-term stability of the implant. It would be informative to describe the mode of fixation of the proximal sleeve as, "bone ingrown, fibrous stable, or unstable" as suggested by Engh et al.

For example, "Sixty-two (95.3%) hips demonstrated stable bone ingrowth. Two stems were found to have stable fibrous ingrowth radiographically, and no stems had unstable status. Spot welding around the inferior border of the metaphyseal sleeve was observed in fifty-four hips (84.4%)."


Thanks for your suggestion. The mode of fixation of the proximal sleeve should be described. We have added this part in the result section. Please see lines 160-166 on page 6. lines 192-194 on page 7.
4. One of the limitations of the current study is the heterogeneity of the study population including various primary etiologies. It would be appropriate to state this concern in the limitation section.

Thanks your objective comments. We have added the heterogeneity into the limitation part. Please see lines 251-252 on page 9.

5. Page 4, line 5: Consider rewriting "high crosslinked polyethylene". "Highly cross-linked polyethylene" is a more generally accepted expression.

Thanks your suggestion. We have changed to the "Highly cross-linked polyethylene". Please see line 85 on page 4.

6. There are no descriptions on the parentheses in Tables 1 and 2, despite expressions in the statistical analysis section of the main text. Please give exact information on the bottom of the tables.

Thanks your suggestion. We have gave exact information on the bottom of the table 1 and 2. Please see tables on page 14.

8. More detailed descriptions are required for Figure 1. Although the surgical technique was described in the main text, please restate it briefly in the figure legends as well.

Thanks your suggestion. We have added the brief descriptions to the Figure 1. Please see figure 1 on page 15.