Author's response to reviews

Title: Can Hyperbaric Oxygen be Used to Prevent Deep Infections in Neuro-muscular Scoliosis Surgery?

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Point By Point Responses To The Reviewers:

Reviewer 1:

1- Patients were not “chosen” for hyperbaric oxygen. As they were accepted as patients with high risk for infection, they routinely received hyperbaric oxygen after “April 2009” and this was clearly stated in the manuscript.

2- The sentence “It is possible that those undergoing HBOT had closer surveillance and more detailed nursing care than those who did not.” was added to the discussion section.

Reviewer 2:

General Comments:

1- We agree with the reviewers point. We do not present a statistical superiority as our patient sample and the number of infections do not let us to apply valid statistical methods for analysis. Everybody knows that even with this number of patients it is theoretically possible to show some kind of statistical validity but would it be ethical? Westill believe that this study, as it is the first of its kind, can be an important basis for other authors who may reach to a larger number of patients.

2- Yes, duration of 6 weeks is too long for prophylaxis but when we compared the costs of reoperation to the cost of hyperbaric oxygen treatment, hyperbaric treatment is still quite cost effective. (hyperbaric oxygen treatment is very cheap in our country as 1 session costs about 30 US dollars)

Specific Comments and Recommendations For Revision:

1- Abstract: Extensive changes were made in all parts of the abstract to increase readability and understandability. (we can not do further changes because of the word limit)
2- Background: We do not feel that the purpose of this study is difficult to understand but we agree that it is interesting.

3- Methods: You have written below that the manuscript does not need to be seen by a statistician but you say that there are no statistical methods. Are you saying that it would be much better to manipulate the results and reach statistical significance? Clinical experience is something that should be respected especially in areas where no other experience have been previously reported.

Our inclusion criteria is very well described. Who says that the HBO group consists of nonserious cases mainly?

Since when are you asking for informed consent in a control group in a retrospective study where patients had undergone a standard treatment?

It is impossible and meaningless to do a multivariate analysis in a patient sample this small. This far exceeds the patient size and purpose of this study. This is not a book chapter, systematic review or a metaanalysis.

4- Results: We feel that this section is clear enough. By inspecting the tables addressed in the manuscript it is easier to understand the context.

“There were no significant differences between the HBO group and the control group in terms of the number of fused levels, the amount of intraoperative bleeding, and the duration of operation.”

It is clear that looking at the same data we do not “feel” the same about the conclusion of our study as our reviewer but speaking about “feelings” we know that we do not have to. This study is not that weak.

5- Discussion: We believe that the discussion section is well organized and enough. We did not do any changes to this section.

6- Quality of English: Extensively edited.