Dear Editors:

Thank you for considering for publication our manuscript titled **"Parental Acceptability of the Watchful Waiting Approach in Pediatric Acute Otitis Media"**. We revised it according to reviewers' corrections and suggestions , as follows:

Reviewer 2979437

The central research question is to describe parental knowledge and opinions about the watchful waiting approach in acute otitis media (AOM). The settings are 3 primary care centers and one pediatric emergency room in southern Israel. The question is clear and relevant if watchful waiting is to be introduced (as I understand it already is) so as to know how to develop parental education in this context. The study is descriptive.

The study is based on questionnaires given to parents of children with AOM. The source population with parents seen at primary care centers as well as the pediatric emergency room, seems appropriate. The authors do not comment on whether this part of Israel might be representative for the entire country.

Answer: we added in the discussion section (page 13) the following paragraph clarifying this aspect:

We recruited parents from 3 primary care clinics in southern Israel, and from the pediatric ER of the only medical center in the area. The pediatric population iof Southern Israel is extremely heterogeneous in terms of its ethnic composition and socioeconomic status and therefore the findings presented in this study cannot be extrapolated to other geographic areas of the country and of course not to the whole Israel population.

A regular sample size calculation could not be done, since the study does not have a testable hypothesis, however, the study size is fairly large for a descriptive study. It is a problem that it is not clear how many parents declined to fill in the questionnaire. If a certain type of parent were more likely to not participate, it introduces a selection bias. Is it known how many eligible parents actually decined?

Answer: we added in the Results section (first paragraph, page 7) a sentence specifying that none of the parents approached in the 4 centers refused to complete the questionnaire.

The questionnaire was not a validated one, and is not shown in its entireity as a figure or additional material.

The main question being the willingness to accept the watchful waiting approach, was evaluated with multivariate analysis, which seems appropriate. However, there was a lot of other statistical testing gping on, as well, please see my comments below.

The data presentation is not very clear, and a lot of not-so-interesting results, where the method of statistical analysis is not entirely clear, are shown, please see my comments below.

Answer: the so called "not-so-interesting results" and not relevant results were deleted in many sections of the Results section.

The language needs a lot of editing.

Answer: more language editing completed.

Specific comments:

Page 3, abstract, end of Results paragraph: "No correlation was found between the education level and the willingness to accept the watchful waiting approach." At the same time, the authors describe an association between education and knowledge and between knowledge and acceptance. Is the lack of correlation first described just due to sample size?

Answer: The main findings of the study were:

1. A significant association between parental education and knowledge about bacterial resistance to antibiotics.
2. Previous experience with AOM was significantly associated with reluctance to accept the WW approach (what we defined as inverse correlation).
3. More parents with knowledge on bacterial resistance were willing to accept the WW approach compared with parents without such knowledge.
4. No correlation was found between the education level and willingness to accept the WW approach.

We cannot speculate anything more definitive about the conclusion no. 4 and we think that the population sample was appropriate. We would add that we analyzed 2 different aspects of education: general education (as reported in terms of parental years of school, high school, college or university completed) and education in terms of knowledge on antibiotic resistance induced by broad antibiotic use, and these are 2 completely different parameters.

Page 7, last sentence: "…with the exclusion of confounding factors". What is meant by this? Surely the confounders must have been kept in the multivariate model so as to adjust for them?

Answer: the reviewer is right. Of course the confounders were kept in the logistic regression analysis and we corrected the respective sentence

Page 8, first paragraph: "…with a statistically significant higher proportion of mothers than fathers in Center C". Is this really interesting? Wouldn't it be better to include the gender of the parent in the multivariate model to see if answers differ between mothers and fathers? It is not clear at all which factors were actually included in the model, see below!

Answer:

We deleted many of the non-relevant results, as suggested by the reviewer.

Page 9, second paragraph. Here is a whole lot of statistical testing going on! Some of these tests are bound to be "significant" just by chance. The difference between the centers is not very interesting to the reader, and it's not that important to answer the main research question. Better then, as suggested above, to make a big table of the multivariate regression, where it is clearly visible what background factors were included and what the risk ratios, confidence intervals and p-values were.

The results section is difficult to read, and you soon "get lost" in all the testing. Most of the important figures could be presented in one or two tables, as already suggested, and the more robust and interesting associations be commented on in the text.

Answer:

The specific differences between the centers were deleted for the majority of results presented.

A new table (Table 4 in the corrected manuscript) deals with the logistic regression analysis and details the parameters included in the model.

Tables 2-3: As already said several times, another type of table would make the results clearer. Here, it is difficult to see how the significance testing was done. What was compared to what? Risk ratios? Confidence intervals? Actual p-values?

Answer: we of course compared the variables mentioned in the tables with stars and other asterisks and the P values were obtained using the chi-square test as mentioned in the Methods section.

Table 4 to me seems superfluous. It is not interesting to know the exact figures for each center. It would be better to just list the most well known adverse effects in the text in the results section.

Answer: Table 4 was deleted and the results presented in the text of the Results section.

The discussion would be easier to read if the main results of the present study were presented together with the corresponding results from previous studies, making it easier to compare as you read. Now, there is a large section about previous studies, followed by the main results of the present at the end.

Answer: the way of building the discussion section is a question of personal taste, knowledge and experience. We think that the way chosen by our group is well accepted and acceptable and would like to stay with it as it is.

It would also be desirable to interpret the results and their implication for the future more. If it shall be possible to introduce watchful waiting as a proper alternative in Israel – what needs to be done in terms of parental education etc? Do any figures exist on how often the watchful waiting approach is actually used in southern Israel?

Answer: This is a very difficult issue to be addressed, particularly when the issue is AOM, a disease extremely common among infants and young children and whose diagnostic is extremely problematic, its prevention controversial, and treatment debatable. The recommendations for WW approach exist since the 1980-90' in Holland and, more recently, in a more exact and controlled format, in the American Academy of Pediatrics, American Academy of Family Medicine and American Society of ENT guidelines (during 2004 and re-enhanced in 2013), recommendations adopted all over the world and, as mentioned in the Introduction section, in Israel as well. These recommendations deal with many difficult decisions in the management of AOM in children, first of all related with diagnosis which is problematic, and furthermore related to real clinical decision problems (what is severe AOM?/ how severe irritability can be assessed? /how to deal with the recommendation that bilateral AOM cases have to be always treated with antibiotics, by knowing that more than 50% of AOM cases are bilateral and therefore such a condition weakening considerably the possibility to adopt the WW approach etc.). We would also like to mentioned the relatively recent 2 major papers published in NEJM (2011) by Hoberman (USA) and Tahtinen (Finland) showing a clear and significant advantage of antibiotic therapy (using amoxicillin/clavulanate) over placebo in real and true AOM cases diagnosed appropriately by really stringent criteria by ENT physicians.

Therefore, no data is or could be available all over the world, and in Israel as well, at the present time, on the real rates of acceptance and implementation of the WW approach because all the management aspects of the disease are still controversial and the diagnostic skills, management preferences and approaches of the clinical practitioners are strictly personal and difficult to evaluate globally as a group.

Reviewer no. 3382921

This article is about a study of the parental acceptability of the WW approach in treating acute otitis media.

The results of the study are not surprising, but they are interesting and may incourace the practitioners to use the WW approach in the treatment of acute otitis media. The structure of the manuscript is incomplete and there are some minor things that should be clarified:

Materials

The article is missing totally the Materials- section, and this should be added to the manuscript, part of the text in the methods section belongs to the materials –section.

Answer: Additional data is presented in the Material and Methods section, describing in more details the study population.

Methods

I would move the sentence ”The statistical review of the study was performed by a biomedical statistician.” to the end of the Methods –section.

Answer: moved.

Discussion

In the discussion –section on the page 12, on the third chapter the sentence that begins with ”We found that parental knowledge about therapy…” is too long and difficult to understand. This should be modified.

Answer: sentence modified.

On the page 13, on the first chapter, the sentence ” Previous experience with AOM was found to be significantly associated…” sould be removed to before the sentense ” The willingness to accept the WW in AOM….”.

Answer: sentence moved.

On the page 13, the second chapter could start where the sentence ” Willingness to accept….” starts.

Answer: done.

Also the sentence ” Our findings are somewhat different that the published data on this subject” could be removed totally from the text.

Answer: removed.

Conlusions

A short conclusions sections could be added at the end of the manuscript.

Answer: a conclusions paragraph was added at the end of the manuscript.

Finnaly, we would like to thnk to the reviewers for their important, critical and constructive suggestions and corrections, which improved considerably, we think, the quality of our manuscript.

Sincerely,

Prof. Eugene Leibovitz, MD

NB: All of the revisions made in the revised manuscript are mentioned and highlighted in the response letter and also highlighted (in red colour) in the updated version of the manuscript.