Author's response to reviews

Title: Disability perceived by primary care patients with posterior canal benign paroxysmal positional vertigo

Authors:

Ricard Carrillo Muñoz (rcarrillo@ambitcp.catsalut.net)
Jose Luis Ballve Moreno (ballvejl@gmail.com)
Ivan Villar Balboa (ivillar@ambitcp.catsalut.net)
Yolanda Rando Matos (yolanrando@gmail.com)
Oriol Cunillera Puertolas (ocunillera@ambitcp.catsalut.net)
Jesus Almeda Ortega (jalmeda@ambitcp.catsalut.net)
Estrella Rodero Perez (24698erp@comb.cat)
Xavier Monteverde Curto (xmonteverdecurto@yahoo.es)
Carles Rubio Ripollès (carlesrbrplls@gmail.com)
Noemi Moreno Farres (morenonoemi@hotmail.es)
Austria Matos Mendez (amatos@ambitcp.catsalut.net)
Jean Carlos Gomez Nova (dr.jeancarlosgn@gmail.com)
Marta Bardina Santos (martabardina@hotmail.com)
Johan Josué Villarreal Miñano (johanjvm@hotmail.com)
Diana Lizzeth Pacheco Erazo (dpacheco@ambitcp.catsalut.net)
Anabella María Hernández Sánchez (anabellahs@gmail.com)

Version: 1 Date: 10 Dec 2018

Author's response to reviews:

Dr Akila Sridhar, Barcelona, December 10th, 2018

Editor
Dear Dr Akila Sridhar,

We submit a revised version of the manuscript "Disability perceived by primary care patients with posterior canal benign paroxysmal positional vertigo" (FAMP-D-18-00272). This revised version incorporates the suggestions made by the reviewers. Answers to the reviewers’ comments are marked in blue and all changes made in the revised version of the manuscript are marked in yellow.

We are grateful to the reviewers for their comments, which have undoubtedly contributed to this improved version of the manuscript.

All authors have read the revised version of the manuscript, agree that the work is ready for submission and accept responsibility for its contents.

Thank you for considering this revised version. We look forward to hearing from you.

Sincerely, on behalf of the co-authors,

Yours sincerely,

José Luis Ballvé Moreno

e-mail: ballvejl@gmail.com

CAP Florida Nord, Parc dels Ocellets, s/n, 08905 L’Hospitalet del Llobregat
Tel: 0034 93 4471080

http://orcid.org/0000-0002-4911-4477

Technical Comments:

1. Ensure email addresses of authors in manuscript is the same in the system
ANSWER: We have ensured email addresses of authors in manuscript is the same in the system.

2. One of the requirements for patient studies is the completion of a STROBE statement. Please find the instructions here – http://www.equator-network.org/reporting-guidelines/strobe/ =; and attach the completed statement as a supplementary file with the revised submission. This file will be removed upon acceptance and prior to publication of the manuscript.

ANSWER: We have attached the completed STROBE statement STROBE as a supplementary file.

3. Figure titles (max 15 words) and legends (max 300 words) should be provided in the main manuscript, not in the graphic file.

ANSWER: Thank you for your comment. We have provided the figure titles and legends in the main manuscript.

4. Please move all the figure legends out of the results section and place them after the References in a section called "Figures, tables additional files".

ANSWER: Thank you for your comment. We have moved all the figure legends out of the results section and place them after the References in a section called "Figures, tables additional files".

Reviewer reports:

Bryan Ward (Reviewer 1): The authors sought to assess perceived disability among patients diagnosed with BPPV in a primary care clinic. They performed a cross-sectional analysis at 2 centers of patients with posterior semicircular canal BPPV, assessing DHI in the short form. The patients were recruited as a sub-study of a randomized controlled trial for the Epley maneuver. The DHI screening score indicating some functional impact of the presence of BPPV and these were higher in women and those with vertigo, but without nystagmus. The paper was well-written, and the study appears to be well-designed, with all the relevant data presented clearly. It can be difficult to distinguish some patients with positional nystagmus/vertigo from vestibular migraine patients and the authors appear to have done a careful job of excluding patients that met these criteria. I have only a few minor edits, but otherwise think the study is worthy of publication.
ANSWER: Thank you very much for the comment.

Line 133 - Use Otolaryngologist rather than 'Ear, Nose and Throat specialist'

ANSWER: Thank you for your comment. We have modified the sentence as suggested by the reviewer. Line 134.

Line 106 - Anxiety and dizziness are often linked, and the diagnosis gets complicated with the overlap of Meniere's disease and vestibular migraine. I do not think there is strong causal data suggesting anxiety can trigger Meniere's disease. The paper cited discusses attacks of Meniere's disease in patients who already have the diagnosis.

ANSWER: We thank the reviewer for his reflection, in which we coincide. We have clarified the sentence as follows:

“Being exposed to emotional stress increases the odds of getting an episode of Menière's disease during the following hours in diagnosed patients (12), and anxiety it is also a risk factor for poor prognosis in primary care patients presenting with dizziness (13).”

Line 142 - otolaryngologist rather than 'ENT specialist'

ANSWER: Thank you for your comment. We have modified the sentence as suggested by the reviewer

Line 169 - negative DH test

ANSWER: Thank you for your comment. We have corrected the sentence as suggested by the reviewer. Line:171.

The authors mention this is part of a larger study and the details are uploaded in supplementary information. If all the patients were being treated with betahistine, details on this treatment should be mentioned in the methods. If betahistine was started after the survey was administered, it is reasonable to exclude this.
ANSWER: We are very grateful to the reviewer for the comment. Following the reviewer’s recommendation, we have added the following sentence in the methods section (fourth paragraph, lines 147-149):

Line 147: “Patients were consecutively recruited by the participating GPs and referred for baseline evaluation within a maximum of 10 days by one of six GPs in the clinical trial team. All patients were being treated with betahistine 8 mg 8-hourly at the initial visit, along with the instruction of P.R.N. use (up to 3 times a day) until improvement of symptoms. The recruitment period was between November 2012 and January 2015

Reviewer 2 (Reviewer 2): PEER REVIEWER ASSESSMENTS:

GENERAL COMMENTS: Overall, this is a good study that has some interesting and important implications. The measures used are appropriate, and the methods are sound and well designed to avoid confounding etc.

ANSWER: Thank you very much for the comment

However, I have some issues with the analysis. Firstly, I am not sure why Wilcoxon has been used for between-groups comparisons, since this is a within-groups variance test.

ANSWER: We apologize for not being enough clear in the methods section. Wilcoxon test - also known as Wilcoxon Rank Sum test - is a non-parametric test used to compare a numerical variable between two independent samples. It is based on the ranks of the ordered values thus allowing comparison of variables with or without gaussian distribution, permitting us to use the same test across the whole paper even when besides the lack of continuity, the subgroups presented a non-symmetric distribution of the Dizziness Handicap Inventor Score. In order to avoid any confusion, we have modified the sentence in methods for “The distribution of total scores by subgroup was compared using the Wilcoxon Rank Sum test” (Line: 167).

Secondly, there is no justification given for the use of a non-parametric, as opposed to a parametric, test of difference.

ANSWER: We agree with the reviewer that these aspects were not described in the text. As stated before, parametric tests rely on specific distributions of the variables or some of its derived statistics. As an example, the usual t-test relies on the gaussian distribution of the variable and its mean. It is a strong assumption which assumes at least continuity and symmetry; not only the second is not guaranteed in some subgroups, but, being the DHI score the sum of ten 0-2-4 scores, the first is clearly arguable (not a single decimal or even score). Being a limited variable (0-40) would be another argument against the assumption of normality. For reasons as the stated
the research group decided to perform non-parametric tests for the DHI Score when possible. However, for limitations in the length of the manuscript and given the audience of the journal, we prioritized to describe other aspects. However, if the reviewer considers these are major aspects that must be addressed in the paper, we can add this information.

Finally, there is no mention of adjustment for familywise error. In this study, there are many unadjusted comparisons being made, so I’d expect at least one or two of them to crop up through random error alone. Given that the conclusions are strongly predicated on the results, I think these issues need to be addressed.

ANSWER: We thank the reviewer for pointing out that aspect. In the present paper, we found p-values below 0.05 in the following contrasts: DHI-S by Sex (0.001), Item 7 by Sex (0.001), Item 10 by Nystagmus (0.012), Item 2 by Sex (0.015), Item 8 by Sex (0.024), Item 3 by Sex (0.029), Item 6 by Nystagmus (0.029), DHI-S by Nystagmus (0.033), Item 9 by Anxiety/Depression (0.035), Item 2 by Nystagmus (0.038), and Item 7 by Anxiety/Depression (0.042). Considering 45 contrasts and keeping the significance level at 0.05, no matter which familywise correction procedure is performed: only DHI-S by Sex (0.001) and Item 7 by Sex (0.001) maintain their p-value statistically meaningful when correcting for FWER. We strongly agree that this is a major issue when testing for particular associations or effects; however, the exploratory nature of the paper is not focused on determining the association of a specific characteristic with a given aspect of vertigo-related disability rather than giving a general descriptive profile. The reviewer is absolutely right on that all this was not correctly addressed in the paper, this is why we have added a specific paragraph in the discussion:

Line 307:

“Results must be interpreted in light of the descriptive nature of the paper. Significant tests were performed independently with no adjustment for multiple hypotheses testing. In fact, given the large amount of contrasts, significance corrections would have led to only two tests finding statistically significant differences: DHI Total Score and the 7th Item by sex. Studies designed to specifically test each of the “independently significant” contrast would be required to confirm statistical significance.”