Author's response to reviews

Title: Validation of a Spanish language version of the Pain Self Perception Scale in patients with fibromyalgia

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Author's response to reviews:

Melissa Norton, MD
Editor-in-Chief
BMC Musculoskeletal Disorders

Dear Madam,

We include the answers to the reviewers’ questions.

Sincerely

The authors

Reviewer 1

- Major Compulsory Revisions

1. Are there sensitivity and specificity data for the Standardized Polyvalent Psychiatric Interview? How will the reader know that this interview yields valid classification of psychiatric disorder?

As we have described in the manuscript (reference 25), the validation of SPPI was published in Psychological Medicine in 1993. In this paper, sensibility and specificity of this psychiatric interview were described. Indeed, the sensitivity and specificity ranged from 80-95% for all of the modules of SPPI.

2. What method of rotation method was used in the process of coming to an interpretation for the results of factor analysis. Was it an orthogonal or oblique method? In some ways the results presented almost look like an unrotated solution. It seems that there is not a single variable that has its highest loading on
factor 2. That seems unusual. The basis for rejecting the two factor solution does not seem clear. If the factors were rotated in such a way as to allow them to correlate, were they correlated? Was an interpretable second factor extracted at any point? Did indeed only one variable have a salient loading on factor 2?

We used a Varimax rotation for the results of the factor analysis. This was the most common of the orthogonal rotations available. There was not a single factor that had its highest loading on factor 2, which was not as unusual as it seemed. Indeed, when one factor explains such a small percentage of variance (7.3%), it is common to not have items with high loading on this factor.

There were a couple of factors that influenced our decision to reject the two-factor solution. First, after using the Kaiser-Guttman method to extract the number of factors, rejecting the two-factor solution seemed to be best based on the results of the factor loadings and the fact that the increase in the percentage of total variance explained by this second factor was only 7.3%. Secondly, the scree plot suggested that one factor was the best solution. Finally, the authors in the original validation (Tang et al., 2007) found similar results in factor loading and considered a one-factor model to be the best solution.

- Minor Essential Revisions
3. The authors say that 16 patients were excluded because CFS symptoms predominated. They later say “none of the participants were ruled out because of exclusion criteria. This seems inconsistent.

We have added a sentence at the beginning of the paragraph describing the characteristics of the sample: “Sixteen patients were excluded from the study due to predominant CFS symptoms.”

4. In what is either the last or second to last paragraph “This data...” should be “These data...”. The word data is plural.

We have modified this as suggested.

5. Reference 14 included errors, “Copying” should say “Coping.”
Thank you for pointing this out. We have made this correction.

- Discretionary Revisions
6. In the description of the HADS the authors use the term “organic disorders.” I think this term is not ideal. Maybe a different term like “physical health problems”, “medical conditions”, or something like “people being assessed in hospital or other healthcare settings.”

We have substituted “medical conditions” for “organic disorders”.

7. The authors may want to rewrite the section of the PVAS. The phrase “a thorough and understandable” seems extraneous. “Ancourage points at each
end are characterized by verbal expressions such as...” could easily be rewritten to say simply “The anchor points are labelled...”

We have eliminated “thorough and understandable”, and the sentence now reads as follows: “The PVAS was designed to allow a subjective assessment of pain.” In addition, we have rewritten the sentence following the reviewer’s suggestions, and the new phrase reads as follows: “The anchor points at each end were labelled as “No pain” ....

8. The “face validity” data presented seem to come from a separate sample not fully described in the study. Is this then the result of a separate study? It is not clear whether this addition is helpful or whether this ought to be presented as a result of the study.

We have not explained this clearly. The study was carried out in a subsample (N=150 patients) of the validation study sample. Because it was carried out at a later time, patients were interviewed at the Association of Fibromyalgia. We have included this data because the patients were from the same overall sample and these data help validate the questionnaire.

We have modified the sentence: “For face validity assessment, a subsample (N=150) of the validation study sample was randomly selected.”

9. Anyone looking closely at the PSPS will see catastrophic thought content and as much of more depressive thought content. What happens if both catastrophizing and depression are controlled, does the PSPS continue to significantly predict anxiety and fibromyalgia impact on functioning?

Consistent with the original authors that developed the concept of mental defeat (Tang et al., 2007), we found that mental defeat was a concept related with depression and catastrophising, but the two did not completely overlap. In fact, we carried out an analysis between mental defeat and catastrophising: “To confirm that pain catastrophising and mental defeat were different constructs, a partial correlation analysis was performed between PSPS-Spanish and the other scales while controlling for pain catastrophising....”. We have not carried out the suggested analysis, but we are involved in a new study of patients with FM that will analyse the degree of overlap between different pain-related psychological constructs, such as catastrophising, acceptance, mental defeat, psychological flexibility, perceived injustice and mindfulness.

10. I am not sure that table 1 is needed. It might be best to include a note saying that this information is available from the authors.

We have eliminated Table 1 (modifying the number of the other tables) and included it as Annex I.

Reviewer 2
1) At the end of the second paragraph of the background section the authors state “Tang et al (6) theoretically distinguished mental defeat from catastrophizing. However, there are no studies that confirm this hypothesis”. However, the study the authors go on to cite, “Tang et al (9)”, measured catastrophizing as well as mental defeat and do in fact report results that suggest that mental defeat is a different aspect of the chronic pain experience from catastrophizing. This should be acknowledged in this paragraph. We have not explained this well enough. We wanted to say that they demonstrated that catastrophising and mental defeat were different, but no other studies have replicated these data. So, we have modified the sentence in this way: “Tang et al. (6) theoretically distinguished mental defeat from catastrophising because the former is a type of catastrophising that is focused on the effects of pain as an assault on the person’s life and sense of identity rather than the experience and meaning of pain per se. However, there are no other research studies that have replicated these findings.”

2) With that point in mind, the final paragraph of the background section which discusses the aims of the study needs to be elaborated upon. My impression from reading the paper is that the main aim of the study is to validate the Spanish version of the PSPS but I feel this isn’t properly introduced in the Background section. For instance, it seems that catastrophising is introduced first yet the authors state that distinguishing catastrophizing from mental defeat is the secondary rather than primary aim of the study. As the reviewer suggested, we have included a sentence about the need to translate PSPS to Spanish in the Background section before introducing the question of the difference between catastrophising and mental defeat. We also explained that the PSPS is important to use in fibromyalgia, which is one of the most prevalent chronic pain disorders. “This questionnaire has not been validated in languages other than English, and it has not been used in one of the most important chronic pain disorders, fibromyalgia (FM).”

3) In the last sentence of the conclusion the authors write “MD and pain catastrophizing are different psychological constructs in patients with FM and, supposedly, in chronic pain patients in general”; supposedly should be removed from the sentence and instead a sentence relating the findings back to those of Tang et al (2010) should be added. We have modified this sentence as follows: “… and in chronic pain patients in general, as have been previously demonstrated (6).”
Minor Essential Revisions:
4) I noticed that in the study sample the ratio of women to men is approximately 9:1; is this reflective of the ratio seen in the FM population? I’m aware that FM is more prevalent in women but I hadn’t realised it was such a great difference. A statement to that effect in the sample characteristics section might be useful.
   We have added the following sentence in the paragraph (results section) describing the sample: “The greater number of women than men in our sample reflected the prevalence of FM in women compared with men.”

5) In the first line of the third paragraph of the Discussion section should “SPSP-Spanish” be corrected to PSPS-Spanish?
   Thank you for pointing this out. We have made this correction.