Author's response to reviews

Title: High prevalence of muscles with myofascial trigger points in patients with shoulder pain.

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Author's response to reviews: see over
March 23, 2011

Dear editor-in-chief

BMC Musculoskeletal disorders

We would like to submit the revised version of the manuscript “High prevalence of myofascial trigger points in patients with shoulder pain” for publication in BMC Musculoskeletal disorders. We are pleased that all reviewers considered our paper suitable for publication. We would like to thank the reviewers again for their thoughtful comments, which we have incorporated into the second revised version of the paper. All changes in the manuscript have been highlighted in red. Our responses to the comments have been included below in this cover letter. As proposed in the email of the editor, we have asked a native speaker (American English) for proofreading the manuscript and we have made the proposed corrections.

Looking forward to your final decision,

Yours sincerely,

Carel Bron, PT, MPT (PhD student)

Co-Authors: Jan Dommerholt PT, DPT; Boudewijn Stegenga DDS, PhD; Michel Wensing PhD Habil; Rob Oostendorp PT, MPT, PhD.

Title: High prevalence of myofascial trigger points in patients with shoulder pain.

We have corrected the few minor errors mentioned in the reviews of Dr. Ge and Fleckenstein and we are glad to see that they and Dr. Fernández-de-las-Peñas consider the manuscript to be acceptable for publication. Dr. Fleckenstein suggested adding a table listing all outcome measures, but as we already included table 2 and 3 with the study outcomes, we feel that another table may not be indicated.

Version: 3 Date: 1 February 2011
Reviewer: Peter P T Dorsher

Reviewer's report
Specific Issues/Suggestions indexed to manuscript sections

Title: Would perhaps the article title “High prevalence of myofascial trigger points in patients with shoulder pain” be more accurately worded “High prevalence of shoulder girdle muscles having myofascial trigger points in patients with shoulder pain”? The authors did not measure all the trigger points in all those muscles, just the number of the 17 muscles studied that have trigger points in each patient. We have changed the title into ‘High prevalence of shoulder muscles with myofascial trigger points in patients with shoulder pain’
Abstract: Results subsection. I think the wording of the last sentence in the results section, though perhaps technically accurate English wording, could be misleading to the casual reader who might not read the article in depth. I think the authors should either term the correlation of number of muscles with active trigger points to DASH score as a “low-moderate” correlation, or preferably explicitly mention that the correlation found suggests the number of muscles in the shoulder girdle with active trigger points only explained about 10% of the patients’ shoulder pain symptoms as measured by the DASH score. This would provide the reader a more accurate and balanced interpretation of the data from the outset. For the interpretation of the correlation we used the classification of Feinstein (see section data-analysis). We agree with dr. Dorsher that 0.3 is the lowest value that is classified as moderate. We have added the word ‘only’ in the text to express our concerns about the low correlation. It is now saying ‘was only moderately correlated with the DASH score’.

The first and second paragraphs of the Introduction that follows imply, correctly or not, that myofascial trigger points may be an important pathophysiologic cause of chronic non-specific shoulder pain, but this study’s data (which admittedly did not look at total number of trigger points in each muscle or their sensitivity) does not show presence of active trigger points to be any more predictive than depression score in terms of the patients’ DASH scores. We believe that there is enough evidence to state that myofascial trigger points might be a possible explanation of shoulder pain. Therefore we wanted to know if MTrPs were prevalent in patients with shoulder pain as primary aim of this study.

Further, the authors’ implication in their response that the number of trigger points in these muscles or their severity/sensitivity might better predict shoulder pain DASH scores does not seem to be corroborated by their data as Table 4 suggests finding active trigger points in the muscles studied only accounted for at most 11% of the VAS-P scores’ variance. Hopefully their other data in BMC Medicine will elucidate this. In this study, indeed, we were able to achieve clinical relevant results after 12 weeks of treatment and the number of muscles with MTrPs correlated positively with the DASH score at 12 weeks (r = 0.49; regression coefficient 2.15; p=0.000; ANOVA F = 9.6; p=0.000), which indicates that about 24% of the DASH score was predicted by the number of the muscles with MTrP. See http://www.biomedcentral.com/1741-7015/9/8

Introduction:
Paragraph 3. Whether a twitch response (jump sign) (according to the textbooks, LTR and jump sign are considered to be different characteristics) is diagnostic of myofascial pain is questionable, as prior study by Sciotti et al had shown even experts had difficulty agreeing on the presence of this phenomenon. You are right. But, although it is considered to be of diagnostic value when it is present, we have not used the twitch response as a diagnostic criterion. In a previous study we already reported the low interobserver agreement of the LTR. http://www.ncbi.nlm.nih.gov/pmc/articles/PMC2565638/

Did not see Figures 1a-d, only saw a Figure 1 in the revised paper and think the
authors meant Figures 1-3 instead. Thanks, you are right and we have corrected this.

Materials and Methods
Section 2.4 Table 1. Thank you for providing this table. I appreciate the authors’ comments on the levator scapula and serratus anterior as not directly causing shoulder pain, however if due to active or latent mTrPs those muscles are not activating properly to rotate the scapula, then does it not logically follow that abnormal substitution of other scapular stabilizers and rotator cuff musculature might cause development of trigger points (active or latent) in those muscles which will be difficult to treat long-term if the scapulothoracic abnormalities are not dealt with? The vast majority of the levator scapula’s referred pain (and the serratus anterior’s most concentrated pain) as described in the Trigger Point Manual is in the distribution of the “shoulder region” diagram provided by the authors, so I am still unsure why especially the levator scapula was not examined but I appreciate the authors’ perspective. Maybe it is a good idea to include this muscle in future studies, but in this study we did not examine it.

2.4 The link to the DASH score produces an error message. We were unable to locate the error message.

3 Results: Flowchart is now Figure 4 as labeled on the manuscript. You are right and it has been corrected.

3.1 might be more accurately worded “Prevalence of muscles with trigger points in subjects” since total number of trigger points was not ascertained, just the number of muscles with trigger points. We added ‘of muscles with’ which is indeed more accurate.
3.2 might be more accurately worded “Prevalence of myofascial trigger points by muscle” as the other wording suggests the authors are counting the number of trigger points in a given muscle. We have changed this.
3.4 There is an error here as active trigger point correlation with DASH was 0.33 and not 0.44 as in the first sentence. You are right. It is corrected in the text.

The authors present only the most favorable data to their conclusion—the presence of MTrP “poorly” correlated (0.29) with DASH and the presence of active trigger point was only minimally better though termed “moderate”. Which of course was not our intention to do so.
The BDI scores were better predictors than the trigger point presence of DASH scores. We are not sure if one may say it is a better predictor. As the BDI scores were extremely low, we consider this as not relevant for this study.

The negative correlation of latent trigger points to DASH scores is not reported. Although it was negative, the correlation was very small (-0.12). To our opinion this means no correlation instead of a relevant negative correlation.

Again, I believe it would be more balanced data presentation if the fact that only 9% of the DASH score would be explained by presence of active trigger points in muscles studied to clarify the clinical significance of this “moderate” correlation found.
The presence of muscles with active trigger points only explained 11% or less of the VAS-P scores variance, so I am interested to see if other data from the study in other publications shows multiple trigger points in muscles studied plus sensitivity helps explain a significant portion of the other 89% of the variance. We agree with you, but we have not studied sensitivity and multiple MTrPs. In future studies this would be very interesting to study.

“Figure 5” should be “Figure 8” in the last sentence. You are right.

4.3 It would be optimal if the first sentence specifically added at the end of the first sentence “including shoulder pain and disability”. That the active trigger point presence only explained 11% (r-square) of the subjects’ current pain and only 8% of their average pain over the past week would tend to counter the authors’ assertion that if the sensitivity of the active trigger points per Hidalgo had been studied the correlation of active trigger points and DASH scores might have been higher.

The authors also don’t discuss at all the unexpected result that presence of latent trigger points had a negative correlation to DASH scores. If these latent trigger points presumably cause ROM restriction and subsequent dysfunction, how do the authors explain this negative correlation? All patients had normal to ‘only’ slightly limited range of motion. It is conceivable that this had no influence on the DASH score as a slight PROM restriction has no relevant influence on daily functioning or pain and therefore it will not influence the DASH score. We consider this slight PROM restriction more as a clinical sign and not as a patient reported complaint.

Would more trigger points found in those 17 muscles that had negative correlation or weak positive correlation increased the prediction of DASH score? That is an interesting question. Based on the current study, we cannot render any conclusion. It would be an interesting subject for future studies.

The correlations of active trigger points and latent trigger points to duration of symptoms were extremely weak (0.12 and 0.04, respectively). Possibly, but the weak correlations make that a very uncertain claim. It is not clear to which claim the reviewer is referring. We do not believe we made any specific claim in this context.

The authors don’t even mention the BDI scores which correlated to the DASH scores better (though still weakly) than the presence of active or latent trigger points. The BDI is not developed as an outcome measure. It is developed and used to discriminate between several stages of depression and in our study to exclude patients with a major depression. It is unclear whether the BDI predicts the DASH or the DASH predicts the BDI.

My concern about this is that the discussion in this section does not report results that are not favorable to a conclusion that trigger points are an important clinical cause of chronic shoulder pain, and the discussion seems to be trying to justify why the correlation of active trigger points to DASH scores is weak (9% of variance of DASH score) instead (presumably from lack of mTrP intensity measures and number of mTrP per muscle in the present study). Our primary aim was to study the prevalence of MTrPs in patient with shoulder pain. We agree with
you that the number of muscles with (one or more) MTrPs does not correlate well with the DASH. In section 4.3, we have discussed the possible reasons for this finding. We have mentioned that we have not measured the Pressure Pain Threshold or the number of MTrPs in each muscle. Furthermore, we have discussed the limitations of the DASH outcome score, as this measurement tool does not discriminate between the affected and non-affected arm. At the other hand, it is still obvious that in every patient with shoulder pain we were able to elicit the familiar shoulder pain by firmly pressing on one or more ‘active’ MTRPs (which is pain producing by definition).

To this end, I think the last sentence should have said “Though the presence of active trigger points in muscles studied only accounted for 9% of the variance of the shoulder pain and disability as measured by DASH scores and latent trigger points none of that score’s variance, this does not mean that that trigger points are not clinically important. Future studies of chronic shoulder pain examining the total number of trigger points and their pressure sensitivity in the muscles studied could substantially impact the magnitude of the effect of presence of myofascial trigger points on shoulder pain and disability.” We have added the sentence to the text.

4.4 Again this section appears to overstate the significance of the present studies findings by implying active trigger points are an alternative explanation for non-specific shoulder pain despite the weak correlations (9% variance found). The authors discuss importance of latent trigger points yet never comment that their study found a negative correlation between presence of latent trigger points and DASH score (presence of latent trigger points improves shoulder function or at least does not influence it). Thus, I think the data does not support the conclusions of paragraphs 3 and 4 in this section that trigger point therapy should precede most other commonly practiced interventions for shoulder pain. We have tried to formulate the conclusions very carefully, based on the high prevalence of pain producing MTRPs (‘If MTrPs are one of the main reasons for shoulder pain……then anti-inflammatory treatment and muscle strengthening exercises should not be the treatment of first choice.’ and ‘If the above hypothesis is true, the treatment could provide an innovative, promising therapy for shoulder pain.’).

5 Conclusion
I think given the issues with the weak correlation of presence of active trigger points (r2=0.09) with DASH and pain scores, the “poor” correlation of mTrPs (r=0.29) with DASH scores, and the negative to near zero correlation of presence of latent trigger points with DASH and pain scores, the present conclusion is still misleading to the casual reader and tends to overstate the magnitude of the potential clinical importance of the presence of trigger points in shoulder pain, at least as far as the present data provides, nor does the present data provide compelling evidence that trigger point therapy should be more a first line treatment than NSAID or PT interventions. The message of this paper is that there is a high prevalence of MTrPs. We have mentioned a number of reasons that may explain the moderate correlation. It is a challenge for future research to search for other factors that may help to explain shoulder pain. Whether these factors are related to MTrPs or not is still unclear.
Hopefully future studies with total number of trigger points and/or their sensitivity might enhance the correlations with DASH and VAS but the weak correlations noted in the present data of presence of active trigger points and VAS scores and symptom duration make even that outcome uncertain.