Reviewer's report

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

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Reviewer: Peter P T Dorsher

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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.

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.

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. 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.

Introduction:

Paragraph 3. Whether a twitch response (jump sign) 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.

Did not see Figures 1a-d, only saw a Figure 1 in the revised paper and think the authors meant Figures 1-3 instead.
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.

2.4 The link to the DASH score produces an error message

3 Results: Flowchart is now Figure 4 as labeled on the manuscript

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

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

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. 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”. The BDI scores were better predictors than the trigger point presence of DASH scores. The negative correlation of latent trigger points to DASH scores is not reported. 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. “Figure 5” should be “Figure 7” in the last sentence.
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?

Would more trigger points found in those 17 muscles that had negative correlation or weak positive correlation increased the prediction of DASH score? 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.

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.

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).

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.”

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.
5 Conclusion

I think given the issues with the weak correlation of presence of active trigger points ($r^2=0.09$) 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. 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.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I have no competing interests