Author's response to reviews

Title: Pressure pain sensitivity maps of the neck-shoulder and the low back regions in men and women

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Pressure pain sensitivity maps of the neck-shoulder and the low back regions in men and women
Asbjørn T Binderup, Lars Arendt-Nielsen and Pascal Madeleine
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We thank the editor for the positive evaluation of the paper and the efforts for improving the manuscript. Find below our answers and revisions in relation to your comments.

Reviewer: Associate Editor’s comments:
Reviewer’s report:
COMMENT 1: The p-values given for gender differences are statistically significant. This is surprising in view that 11 women and 11 men tested.
ACTION: Thanks again for the constructive criticism of our manuscript. The reported p-values are the outcome of the statistical analysis performed in SPSS (version 17). We want to underline that despite the low population size and effect size, these statistical differences are in line with studies assessing PPT differences among gender (see e.g. Chesterton et al. 2003, Ge et al. 2005, 2006, Soetanto et al. 2006, Garcia et al. 2007, Hurley & Adams 2008, Rivest et al. 2010). Further, PPT assessment has been reported to the most sensitive pain modality when investigating gender (Hurley & Adams 2008). It is thus correct to mention that these differences (despite low size effect) are in line with studies assessing PPT and gender.

COMMENT 2: It is not clear to me how the 36 points of measurement were treated when assessing gender and other differences. Please clarify.
ACTION: We have clarified this point in the abstract and in the core of the manuscript. We ran a three-way ANOVA test (factors: gender, location and subdivision) for the 36 points in the cervico-thoracic region and a two-way ANOVA test (factors: gender, location) for the 27 points in the lumbar region.

COMMENT 3: In the discussion (p. 8, line 21) use say that the study confirmed significant(ly?) lower mechanical thresholds in women compared with men. The same is seen in the abstract (line 24). Do you refer to statistical significance of more general importance? If the latter, I must disagree. The effect sizes show that only 2 % of the variation in PPT was explained by gender; and you yourself say (p.11, line 1) that the effect sizes (correct wording to the former from 'size effects') were small. Based on your results it seems that the difference in PPT by gender is minor, and this should be spelled out instead of underlining such a small difference in the abstract, discussion and conclusions (both in the abstract and end of discussion).
In the same time, we understand your concern. Thus, we acknowledge that only 2 % of the variation in PPT was explained by gender. We have added this information in order not to overstate our findings (see changes on Page 2, line 24; Page 8, lines 3, 5, 9, 11 and 22-23, Page 9, line 2-3 and Page 11, lines 3-5 and 12-13). We have removed "general" when stating gender differences and deleted gender aspects from the conclusion in the Abstract.
COMMENT 4: Further, in the abstract, the aim of the study is not clearly formulated. What is to be compared?

ACTION: We have now stated the aims in clearer term. See highlighted changes.