Reviewer’s report

Title: Influence of temporomandibular disorder on temporal and masseter muscles and occlusal contacts in adolescents: electromyographic study

Version: 2 Date: 22 July 2013

Reviewer: Samuel W Cadden

Reviewer’s report:

1. The question being asked is well-defined (page 4, paragraph 3).
2. The methods seem adequate in all but one respect (see below).
3. There are some potential problems in respect of the data – particularly in the light of the EMGs supplied by the authors in their covering note (see below).
4. All the relevant results are reported – but they are not all shown adequately (see below).
5. The Introduction and Discussion are much better than in the original manuscript.
6. The authors mention limitations of the work (page 14, 2nd paragraph).
7. The authors cite a substantial amount of previous work.
8. The revised title is acceptable.
9. General: The writing is better than in the original version – even if there are still a few tiny errors in grammar/style.

Major issues (compulsory revision).

Page 7 last 2 lines: Contrary to what the authors stated in their covering note, they still state in the manuscript that normalisation was to the average RMS recorded in the three readings of MVC. This is illogical. The MVC must be the maximum, not the average, of the three recordings.

Results Section: The authors have now provided original records of electromyograms in their response to the reviewers but not in the manuscript itself. This is helpful although it is also problematic in that: (i) the records for rest position include substantial bursts of activity unlike anything I have ever seen in “rest position” EMGs; (ii) the records for MVC seem to show inconsistent levels of activity within the periods of biting, which are quite different to what one usually sees (and would expect) if a participant was making a sustained maximal voluntary effort. The authors may have an explanation for these apparently aberrant recordings but, given that they exist, the recordings ought to be shown in the manuscript with an explanation for why they are so different from records
obtained in other laboratories. If the authors cannot give such an explanation, it would call into question the validity of many of the findings.

Pages 9 and 10, Section 3.4: In my opinion the revisions to this section have made it worse. It is arguably inappropriate to talk about non-significant correlations and it is certainly inappropriate to talk about correlations with a P value of 0.74! I suggest returning to the previous, briefer, version of this paragraph - simply stating that there were no significant correlations.

Page 10, final paragraph: Despite my comments on the previous version of this manuscript, the authors still make no reference to the fact that the physiological basis of the mandibular rest position is one of the most controversial areas in oral physiology. It may indeed - as the authors suggest - involve reflexes in anti-gravity muscles, but that is far from being a universally-accepted explanation. Some acknowledgement of the controversy is required.

Minor issues (essential revisions).

Page 8, 2nd paragraph: Given the authors’ response to my previous comment about the use of both non-parametric and parametric tests, I suggest a slight re-wording of the sentence “The nonparametric Kruskal-Wallis test and one-way analysis of variance with Dunn’s post hoc test were used to examine differences . . . ” to “Depending on the normality or otherwise of the data, either one-way analysis of variance with Dunn’s post hoc test or the nonparametric Kruskal-Wallis test were used to examine differences . . . ”

Discussion, 2nd paragraph, last 5 lines: The authors should acknowledge here (as they do in their covering note) that they are referring to factors which may have occurred prior to and/or during the experiments.

Page 13, 2nd paragraph: Either the comparison with the study by De Santis et al should be omitted or some comment should be made about the fact that the ages in that study and in the present one were very different.

Page 13, last 3 lines: “Electrical activity is directly related to muscle strength and an improvement in muscle condition leads to enhanced chewing capacity and a consequent increase in electrical activity in the muscles of mastication.” It is still not clear what the authors mean by this. EMG activity is dependent on so many things - particularly when using surface electrodes - that suggesting that one can tell much (or indeed anything) about muscle strength from a surface EMG is very dubious. Given that this statement adds little to the overall sense of the paper, I would suggest either omitting it or reducing it to "It is known that an improvement in the condition of the muscles of mastication leads to enhanced chewing capacity."

Discretionary issues/revisions.

Page 6, final paragraph: I suggest changing “The participants were instructed to remain seated in a chair, feet apart, shoulders relaxed and hands resting on
thighs, such that the head was on the Frankfurt plane parallel to the ground. The participants received no visual feedback of the signals registered on the computer.” to “The participants were instructed to remain seated in a chair, feet apart, shoulders relaxed, hands resting on thighs and with the head such that the Frankfurt plane was parallel to the ground. The participants received no visual feedback of the signals being registered on the computer.”

Page 14, lines 8-9: I suggest changing “more consistent” to “more conclusive” or “firmer”.

**Level of interest:** An article of limited interest

**Quality of written English:** Needs some language corrections before being published

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

**Declaration of competing interests:**

I declare that I have no competing interests