Author's response to reviews

Title: Does orthodontic treatment provide a real functional improvement? A case control study.

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Version: 4 Date: 7 May 2013

Author's response to reviews: see over
Reviewer's 1 answers

Background:
Before “Bothelo et al” please insert this statement: “Saccucci et al. assessed by surface electromyography (sEMG) in 28 young subjects, the changes in upper and lower orbicular oris (OO) muscles produced by a preformed functional device in subjects with Class II, division 1 malocclusion, deep bite, and labial incompetence. He suggested that the use of a preformed functional device in interceptive orthodontics induces a significant increase of the sEMG activity of the lower OO muscle at rest and of the upper OO muscle during mandibular protrusion.

For references list:
Saccucci M, Tecco S, Ierardoa G, Luzzi V, Festa F, Polimeni A.

We have inserted the article suggested in the bibliography.

2.4 sEMG
In my opinion is better if you include this sentence: “The repeatability of the recording protocol was investigated for the test conditions, by asking the selected subjects to repeat the sEMG recording two times, with a gap of 15 min between the two recordings. The results of the first and second set of experiments showed a repeatability of measurements.”

We have included the sentence suggested in the materials and methods section.

Reviewer's 2 answers

1. Orthodontic treatment for Class II patients can include the orthopedic treatment as the author(s) mentioned in the MM section. In addition, treatment modality using fixed orthodontic appliance (braces) goes usually with use of intermaxillary elastics or mechanics which can exert intermaxillary forces. These kinds of treatment modalities usually lead to change in the vertical dimension; if treatment modalities can be applied without intermaxillary forces, iatrogenic change in the vertical dimension would NOT be occurred. As the author(s) might agree, change in the vertical dimension can affect the sEMG result. Therefore, the treatment modality (with or without intermaxillary mechanics, extraction or non-extraction, etc.) used in the study samples should be well defined and the clear explanation on this issue should be included in the manuscript. Furthermore, the results in this article cannot be a representative result for the whole orthodontic treatment. The author(s) should not refer the result of this article to the general situation for all orthodontic treatment cases as well as specific situations for Class II div 1 treatment cases. Although the author(s)’s assumption may be correct, the study design used can NOT support it. More detailed design or larger number of samples are needed.

Our patients were subjected to non-extraction orthodontic treatment with fixed appliances multibrackets and use of Class II elastics.
The question if orthodontic treatment lead to change in the vertical dimension is discussed in literature.
Despite many studies have investigated this issue from various perspectives, the factors that affect vertical dimensions have not been clearly determined. (Gkantidis N, Halazonetis DJ, Alexandropoulos E, Haralabakis NB. Treatment strategies for patients with hyperdivergent Class II Division 1 malocclusion: is vertical dimension affected? Am J Orthod Dentofacial Orthop. 2011 Sep;140(3):346-55.)

Our study has certainly limitations. It is our view that further studies made on samples of higher dimension are needed to confirm our results. Surely the results are only related to our study and do not cover all cases of orthodontic treatment.

2) Although there is a criteria for sample selection without TMD, the sample might have CO-CR (centric occlusion – centric relation) discrepancy. Furthermore, Class II patients are more vulnerable to have CO-CR discrepancy at the end of treatment compared to Class III patients. The patients with CO-CR discrepancy tend to have sEMG change. Therefore, the result of this study might be obtained due to NOT orthodontic treatment BUT CO-CR discrepancy.

The CO (centric occlusion) does not usually coincide with CR (centric relation) in the general population. For this reason, the lack of coincidence CO-CR was not included among the exclusion criteria for sample selection.


3) The evaluation of functional improvement can NOT be confined to the result of sEMG. There are a lot of other ways dealing with functions: occlusal force, occlusal contact at CO and other excursions, efficiency of chewing, and so on. Although neuromuscular balance is important for functional improvement, it is still one of the components of functional improvement. The author(s) should draw a conclusion in more careful way.

We agree with the objection. In this study, however, we have chosen to evaluate the rest position rather than the clenching because it is not a physiological function and the mandible is usually in rest position during the day. Furthermore resting sEMG is not a static activity. Compared to clenching, which is a phasic activity, resting position is given by the tonic relation between gravitary and antigravitary muscles.

Reviewer's 3 answers

By excluding patterns of Class II div. 1 malocclusion from the sample (control and treated), such as those with extended crowding or crossbite, we may add bias to sample selection. What we treat in everyday practice is quite often what was excluded in this study, and thus, even if selection bias is not evident, the results apply to limited cases. Furthermore, certain inclusion criteria are really wide criteria and not specifically defined and this might also imply selection bias. In any case, these issues have to be addressed at least in the Discussion. Also, for this reason the results should be reported in a less strict manner.
Surely our results are limited to the examined cases. Even if dental crowding is frequent in Class II we have excluded these cases from the sample because the treatment should not provide extractions.

2) In certain parts of the manuscript the authors report the conclusions of various studies as being evidence based knowledge. For example, the reference of Wang et al (No 6) is used to support the opinion that orthodontic treatment causes TMJ disorders. This is a study with high risk of bias and is in contrast to various other studies that do not support a causative relationship between orthodontics and TMJ problems (See also: Luther et al. Orthodontics for treating temporomandibular joint (TMJ) disorders. Cochrane Database Syst Rev 2010;(7):CD006541.). Such opinions, if stated, should be stated according their level of validity in order not to “expose” the reader to misleading, not evidence based conclusions.

We are aware of the limitations of some of the articles reported, in fact we have included in the discussion the sentence “longitudinal studies are needed to clarify these issues”. To avoid giving misleading informations we have also included the study of Luther et al. suggested.

3) Regarding the interpretation of the results, differences in sEMG values were evident only in 2 out of 8 parameters, only for open eyes condition, and these were not highly significant. If we consider that when applying multiple comparisons there is an increased risk for false positive difference and we apply a Bonferroni correction to the level of significance, these differences will become insignificant. Considering this fact and the limitations of the study reported on the Discussion section (sample and experimental considerations), I would argue that the data suggest no difference actually (as stated in the Conclusions in the Abstract) and not increased sEMG activity in treated patients.

The objection is made right. However, the statistical analysis shows a statistically significant interaction between group and treatment. The post-hoc analysis, with adequate correction of the significance level for multiple comparisons, did not allow us to highlight statistically significant differences between groups.