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LETTER TO THE EDITOR

Response to: Criticism of the systematic Review (SR) 'centric relation-intercuspal position discrepancy and its relationship with temporomandibular disorders'

Sir,

Regarding the letter to the editor sent by Professor Zonnenberg, AJ. to the journal Acta Odontologica Scandinavica, we would like to state the following:

In our systematic review (SR), there is no paragraph indicating or suggesting that the terms 'malocclusion' and 'occlusal interference' are equivalent terms. On the contrary, in our manuscript we point to the following: *'In the past, some studies suggested that malocclusion and occlusal interferences were considered as the main factors for predisposition, initiation and perpetuation of Temporomandibular Disorders (TMD)'* [1]. The intention of the paragraph was to establish a background where in the recent past the presence of malocclusions and occlusal interferences were considered as risk factors for TMD. This is part of the discussion where current evidence shows that occlusion would not be associated directly with the pathophysiology of TMD and would even be considered as a gnathological paradigm [2].

Unfortunately, the article published by Goldstein et al [3] could not be consulted in our study. If the publication dates were reviewed, it could be rescued that both works were being published in parallel. Despite this, the results of the analyses in our review are very similar to his. Dr. Goldstein himself points out in his study *'The definition of centric relation has been both controversial and divisive, with little consensus'*. This was mentioned by us after definition of the variables and the problem, where we established that *'Currently, there are about 26 definitions of CR. However, its definition needs to be oriented clinically to reduce confusion and controversy'*. As indicated in our study, this is not very different from that evidenced by Goldstein, who also, based on his results, indicates: *'Although clinicians and educators refer to CR, there is a lack of agreement on which definition of CR is best. In the current study, only 29% (19 of 65) of the responders chose the definition proposed as the new one to be used in the 9th Edition of the Glossary of Prosthodontic Terms'* [3]. In an introduction, it is not possible to refer to all the authors or previous approaches, so we consider it relevant to rely on the evidence available through the search. In addition, this does not exclude what the authors cited by Dr Zonnenberg indicate. So, what is this criticism based on?

Regarding the criticisms of the systematic review

The studies he points out were effectively discussed. The authors grouped the results according to the association with the discrepancy into three groups: (a) Muscular disorders and CR-ICP discrepancy; (b) Joint disorders and CR-ICP discrepancy and (c) Muscular and joint disorders and CR-ICP discrepancy. All the studies that established a positive association between CR-ICP discrepancy and TMD were mentioned in the results and analyzed according to the following parameters: Type of discrepancy (three planes of space), associated pathology, orthodontic or non-orthodontic patients and quality of evidence according to the Newcastle-Ottawa Scale (NOS). What the author of the letter points out seems strange to us, since the results and tables 1,2,3 and 4 detail this item.

On the other hand, we think that professor Zonnenberg AJ supposes that the evaluation of his article was focused on the method of recording the centric discrepancy, but the objective of the NOS scale does not consider this item. Therefore, he expresses a possible data collection bias, since he considers that his method would be a gold standard. Our SR only tries to group and present the registration methods and does not criticize the validity of each of them [1], so a critique of this has no basis. Therefore, the assertion that he makes *'based upon the clinical method to locate centric slide, worthless for this SR'* is inadmissible.

Our study was not limited to the methods to measure the centric slide because the amount of evidence available at the beginning of the study was unknown and it was not within its objectives, but it used the results. Limiting the search by measurement method of centric slide would only limit the amount of evidence, considering the scarcity of results, so looking for methods that differentiate what Zonnenberg AJ points out: *'<2 mm, considered the normal variation of centric slide and $=$ 2 mm or >2 mm'* does not seem relevant to us.

Regarding the referenced article by Manfredini et al [4], there is a confusion regarding the analysis. The study according to the NOS Scale [5], *'measures the quality of the evidence for case-control and cohort studies, assigning a score ranging from 0 to 9 points'. For case-control studies, there were three categories. (1) Selection (4 points), (2) comparability (2 points) and (3) exposure (3 points). To determine the quality of cohort studies, there were also three categories with a level of evidence ranging from 0 to 9 points. The categories were (1) selection (4 points), (2) comparability (2 points) and (3) outcome (3 points)'*. These three domains do not evaluate what Zonnenberg AJ points out, and what he is referring to, which would be a data collection bias. As a current proposal, a better evaluation could be done with a tool such as The Grading of Recommendations Assessment, Development and Evaluation (GRADE) system [6] in the 'Other bias' item. The evaluation of

the methods should be made through a system that assesses diagnostic accuracy methods such as the QUADAS-2 criteria which is a revised tool for quality assessment of diagnostic accuracy studies [7]. We think that the criticism made by Professor Zonnenberg does not fit into the scope and objectives (clearly) established in our study and even when referring to his study [8], he does not focus it in terms of the NOS system, but on the evaluation and determination of centric slide. Further, the systematic review was conducted according to Preferred Reporting Items for Systematic Reviews and Meta-analyses (PRISMA) statement [9] in the established domains, so the assertions such as *'any scientific article must be so clear that any interested researcher must be able to repeat the procedures and methods to come to the same conclusion'* are arbitrary and light and lack scientific basis, as he points out. Professor Zonnenberg suggests that the study did not follow the guidelines of an investigation, however, he did not realize that all our study is supported by the PRISMA guidelines [9] as a systematic review study should be carried out.

Conclusions

- The quality of the evidence is low because none of the reviewed articles obtained the highest score based on Newcastle-Ottawa Scale with an average score of 3.36 and a median of 3.13. In addition, the designs and methodologies of the studies were heterogeneous which makes the comparison between them difficult. Again Professor Zonnenberg AJ, confuses the NOS evaluation system with the risk of bias in the data collection systems and focuses his criticism only on this point.
- The systematic review carried out is adjusted to the PRISMA statement, which allows it to be carried out and replicated by other researchers using the same methodology, which meets the parameters of evidence-based medicine.
- The difficulty of this topic is not the lack of preparation and knowledge of the authors. On the contrary, it is the controversy between different philosophies and ways of approaching it, in addition to the new evidence that is changing some paradigms regarding the risk factors for the development of TMD, as the occlusion.
- We think that the criticism for the lack of expert peer reviewers in the Journal Acta Odontologica Scandinavica is unfounded, because in our revision and publication process, the suggested corrections were accurate and produced a revision according to the established guidelines. We invite Prof. Zonnenberg AJ to publish in this journal so that he verifies it by himself.
- Finally, the authors believe that it is not necessary to be a peer reviewer in a journal with an impact factor greater than 2 when the arguments that support their criticisms are not well established and generally underlie a prejudice of a line of research and argumentation rather than the evidence itself supporting them, even further when he refers to himself as a 'gold standard'.
- Professor Zonnenberg AJ should consider making critical arguments based on scientific evidence, with the aim of contributing positively to the construction and evaluation of dental science and not formulating a critique supported by comments with little basis and lack of knowledge, because it makes this discussion look like an outburst about their personal position and interests.

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No potential conflict of interest was reported by the authors.

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