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Authors' response

We are grateful for the interest that our recent study produced in the orthodontic community and we appreciate the opportunity to address concerns.

The first concern is related to the study design classification “retrospective longitudinal.” This kind of questioning is not new, even in *AJO-DO*.^{1,2} A study that is done on past records collected by following the sample for a particular period has been usually called retrospective longitudinal by the scientific community.²⁻⁵ Likewise, longitudinal research may take numerous different forms, including retrospective longitudinal studies as a variant. This variation could be used if participants in an identified cohort have already experienced events related to the interest variables. Then the data are collected and examined retrospectively.⁶ As stated in the Material and Methods section of our study, CBCT images of 45 patients with maxillary impacted canines treated in a private orthodontic clinic were obtained before treatment and after canine traction, clearly stating the retrospective and longitudinal nature of the study.

Regarding the second concern, the question was raised whether only maxillary canines with favorable impaction were included, because if so, they might not cause incisor root resorption. We want to reinforce that the main objective of this study was to evaluate the influence of impacted maxillary canine orthodontic traction complexity on root resorption of incisors. Therefore, 2 groups were established according to the level of orthodontic traction treatment complexity: low-complexity group and high-complexity group. Specifically, the high-complexity group included patients with impacted maxillary canines in impaction sectors 3, 4, or 5 (ie, near to midline) according to the classification of Ericson and Kuroi.⁷ In cases with canines in sector 3, only those impacted canines with α angle $>40^\circ$ (ie, with horizontal position tendency) were included. Clinically, these criteria demonstrate the proximity of impacted canines to the roots of the incisors and corroborate the initial complexity of this group. We do not agree that all unfavorable impacted canines that caused

root resorption of incisors or might cause resorption during traction should be extracted. It would depend on each specific case and on the biomechanics planned for their traction. Orthodontic traction of unfavorable impacted canines, which caused severe root resorption before treatment and were successfully tractioned, has been previously included, as well in a study that evaluated root resorption.⁸

We thank you for your comments and the opportunity to clarify and discuss them.

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“Please don’t move”: Cone-beam computed tomography and obstructive sleep apnea hypopnea syndrome

We read, with great interest, the article entitled “Computerized measurement of the location and

value of the minimum sagittal linear dimension of the upper airway on reconstructed lateral cephalograms compared with 3-dimensional values" (Alwadei AH, Galang-Boquiren MTS, Kusnoto B, et al. *Am J Orthod Dentofacial Orthop* 2018;154:780-787). Alwadei et al used cone-beam computed tomography (CBCT) and described significant correlations between the minimum sagittal linear dimension on reconstructed lateral cephalograms and both the minimum cross-sectional area and the airway volume. This article was a valuable contribution to the evolving debate on the diagnostic tools for obstructive sleep apnea (OSA).

Indeed, we have recently witnessed the appearance of a large number of protocols using linear or volumetric measurements through the use of CBCT. Compared with traditional time-consuming manual cephalometries, the partial automation provided by CBCT saves considerable time and eliminates some of the operator-dependent errors. However, this is an evolving field, and a number of issues must be discussed before affirming it as a diagnostic instrument for OSA.

An illustration of this is how the operator should deal with gravity. As mentioned by the authors, there are notable modifications in the position and form of pharyngeal structures in response to postural changes. Thus, the total volume and the cross-sectional area decrease as a natural phenomenon when the patient is lying down. Moreover, ventilation involves complex mechanisms that vary throughout the respiratory cycle. Breathing significantly modifies the shape and dimensions of the airway, a variable that has not been addressed in most protocols. In general practice, the patient is asked to not to breathe, move, or swallow, and to keep the tip of the tongue behind the maxillary incisors. As a consequence, the position of the base of the tongue changes, which generates a tone that does not exist when the patient is asleep.

The head posture also has a strong influence on the posterior airspace between the base of the tongue and the pharyngeal wall. Positioning of the patient for the acquisition is therefore of paramount importance. Most CBCT systems acquire images in both standing and sitting positions. But, for better accuracy and reproducibility, it has been considered to be important that the Frankfort plane be horizontal, which does not reproduce the actual clinical situation. Finally, the morphology of the upper respiratory tract varies during sleeping, but CBCT examination is routinely performed on awake patients. Although part of the anatomic abnormalities that could be detected during sleep persist during the day, this may considerably interfere in our decision-making process.

Short acquisition times reduce movement artefacts and will certainly enable some normalization of results and the convention of more precise protocols in the future. These improvements, however, may not surmount all of the aforementioned weaknesses. Among them, we consider that there is a fundamental conceptual concern: Are we moving toward static morphologic diagnostic criteria for a condition that is dynamic by definition?

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Author's response

The issues brought up in this letter are issues that many other researchers and clinicians bring up. However, I think that what should be considered is what these radiographic images are used for. In their letter, the authors referred to "the evolving debate on the diagnostic tools for obstructive sleep apnea." In my opinion, radiographic imaging of the airway will always be a screening tool, not a diagnostic one, with the exception of some images of the adenoids in children. Therefore, studying images of patients in an awake and upright position is proper because it mimics how we take these images in our everyday practice.

All of our orthodontic patients have either a 2D or a 3D image. Studies and contributions that aim to improve the screening process will provide us with a more sensitive method for referring patients with higher probability of OSA for sleep studies. This will allow for timely diagnosis and then management if an OSA diagnosis is confirmed.

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Erratum

Correction to: Gudhimella S, Ibrahim AY, Karanth D, et al. A rodent model using skeletal anchorage and low forces for orthodontic tooth movement. *Am J Orthod Dentofacial Orthop*. 2019; 155:254-263.

In the above-mentioned article, Figure 5 should appear as follows.