

# Practices that Can Impact Proper Assessment of the Upper Airway Volume

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Upon reading the title “Differences in Volume and Area of the Upper Airways in Children with OSA Compared to a Healthy Group” by Rossi et al.<sup>1</sup> in the July 2016 issue of the *Journal of Dental Sleep Medicine*, I was very excited. However, some of that excitement faded after reading the article. In my humble opinion I thought the study had multiple flaws, but I wanted to focus on three major issues:

1. The objective of this research was to “*verify the differences in the volume and areas of the UA among children with OSA who have had adenotonsillectomy but continue to have persistent OSA, and a control group of healthy children.*” Seeking that, the authors stated that in the study group “*all the patients had undergone adenotonsillectomy or had been excluded of having hypertrophic tonsils; but they all had OSA symptoms.*” This statement might just need clarification but from what I understand, there were patients with OSA symptoms who have “*not undergone adenotonsillectomy*” but were included in the study group because the tonsils were not hypertrophic. I hope my interpretation is wrong because if this is true then this causes major flaws:

- No definition of hypertrophic tonsils was used. An objective measure such as a standardized palatine tonsillar hypertrophy grading scale could have been used.<sup>2</sup> Followed by exclusion of subjects with 2+, 3+, and 4+ tonsils.
- Simply excluding subjects with hypertrophic tonsils does not exclude subjects with enlarged adenoids. It is true that they are both lymphoid tissues and their sizes should hypothetically be correlated; however, this has not been shown to be the case. Hypertrophic tonsils and adenoids do not necessarily co-exist, and the size of the tonsils cannot be used to predict the size of the adenoids.<sup>3,4</sup> Furthermore, there are surgeons who do not remove the adenoids completely and remnant tissue is left behind, in addition to surgeons who only remove the tonsils and leave the adenoids untouched. This should have been checked on the CBCT.
- It contradicts the objective of the article since subjects without adenotonsillectomy were included in the study group.

2. In most anatomical books and papers the nasopharynx “*lies behind the nasal cavity above the soft palate.*”<sup>5,6</sup>

The inferior limit in the current article extended far inferiorly that it included the soft palate. Putting that atypical definition aside, the soft palate thickness may increase as a result of vibration or inflammation when snoring.<sup>7</sup> Since the authors in the current article concluded that “*children with persistent OSA symptoms after adenotonsillectomy present with narrowing of the nasopharynx*” and the nasopharynx they used contained the soft palate, the soft palate might have played a role in the persistence of the symptoms in addition to the narrowing of the nasopharynx and should have been discussed.

3. The authors stated that subjects were “*placed in the tomography room in a sitting position with their head parallel to the Frankfurt plane.*” How can the head be parallel to the Frankfurt plane? An important factor affecting airway analysis is head position.<sup>8,9</sup> The method to orient the head should be clearly described.

## CITATION

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