

Letters From Our Readers

To: Editor, *The Angle Orthodontist*

Re: Nasal obstruction causes a decrease in lip-closing force. *Angle Orthod.* 2011;81:750–753, by Sabashi K, Washino K, Saitoh I, Yamasaki Y, Kawabata A, Mukai Y, Kitai N.

Patients with nasal obstruction (increased nasal airway resistance) tend to have weak lips. This relationship is quantitatively established by Sabashi et al. in a recent publication. This article interested me as it raised many thoughts in my mind. Often the relationship between mouth breathing, lip incompetence, lip closing force, and nasal obstruction are overlooked in the clinical diagnosis of the patient. However, establishing a firm relationship would guide us to a more goal oriented treatment planning and execution with less potential for relapse.

There is currently no evidence suggesting a positive correlation between mouth breathing, lip incompetence, and lip closing force. But mouth breathers tend to have increased nasal airway resistance.¹ Possibly, there are some unanswered questions such as: Do incompetent lips present with weak lip closing force? Is there any normal lip closing force for an individual? How the tonicity of the Orbicularis Oris and Mentalis muscles in a growing subject interact with craniofacial development? Why there is a mismatch between skeletal and soft tissue development? It is imperative to consolidate the facts for a better treatment care in young patients who are prone to environmental insults.

Regarding the present study details, I have few concerns that may need clarification.

- The details pertaining to the strain measuring device are inadequate although the source references are quoted in the text. The reliability and calibration of the device is crucial for the understanding and extrapolation of the results.
- The 54 patients who participated in the study had normal skeletal and dental development. What is the status of their lip competency?

Nevertheless, the study establishes a relationship between reduced nasal airway resistance and lip closing force. With this information in hand, I hope future studies will narrow the gap between physiologic and anatomic relationship.

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REFERENCE

1. Fujimoto S, Yamaguchi K, Gunjigake K. Clinical estimation of mouth breathing. *Am J Orthod Dentofacial Orthop.* 2009;136:630.e1–7.

To: Editor, *The Angle Orthodontist*

Re: Effects of mandibular incisor intrusion obtained using a conventional utility arch vs bone anchorage. *Angle Orthod.* 2011;81:767–775, by Esen Aydoğdu and Ömür Polat Özsoy.

The authors Aydogdu and Ozsoy have taken a sincere effort to study the dentofacial effect of mandibular incisors intrusion using conventional intrusion utility arch and mini-implants. The study is very important to the clinician and the authors' effort should be commended except for few concerns.

Concern 1: Intrusive tooth movement was completed when the incisors reached the same level as the mesiobuccal cusp of the mandibular molars (which acted as reference). But I seriously doubt that the vertical position of the mandibular first molar could remain unchanged during incisor intrusion when using conventional utility arch. In fact, this is evident in the results of the study. In Group II (conventional utility arch), Xi- Pm/Mand 6,mm: median (min-max) is increased from 11.5(10–17) at pretreatment to 12.5(10–19) post intrusion. Consequently the duration of treatment could be different with 5mo for group I and 4 mo for group II (as the molar change has contributed relatively).

Concern 2: It was concluded that incisor intrusion achieved using mini-implant was no different than the movement achieved with a conventional intrusion utility arch. But I am confused about this statement when the mean amount of change of 0.4 mm/mo for the incisor tip (implant) and 0.25 mm/mo for the incisor tip (utility arch), was noted. There was no explanation mentioned in the article as how the authors derived at these values. I sense there are definite changes between the groups.

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