Letter to the editor*

Low-level laser therapy

We read the article on low-level laser therapy in the April 2020 issue with great interest (Mistry D, Dalci O, Papageorgiou SN, Darendeliler MA, Papadopoulou AK. The effects of a clinically feasible application of low-level laser therapy on the rate of orthodontic tooth movement: a triple-blind, split-mouth, randomized controlled trial. Am J Orthod Dentofacial Orthop 2020;157:444-53).

This article is informative and creates great interest among readers. The effect of low-level laser therapy on the amount of maxillary canine distalization has been studied on a clinically feasible application period of 4 weeks, which coincides with the routine recall period, and hence, no additional treatment visits were required, which makes this study clinically important. However, we observed a few things during our reading that need clarification.

The study primarily aimed to investigate the effect of a 4-week application of low-level laser therapy on the rate of tooth movement. However, there are no tables or figures or any mention in the text regarding the rate (ie, millimeters per month or millimeters per week) of tooth movement.

In the Material and Methods section, under the subheading of Participants, Eligibility Criteria, and Settings, regarding the selection of eligible patients, points 3 (no previous dental or orthodontic treatment of the maxillary arch) and 4 (no previous orthodontic treatment) seem similar. Is this a repetition of the same point, or is additional deliberation required?

Under Eligibility Criteria, there was no mention of the amount of crowding, underlying malocclusions that required the extraction of maxillary premolars, growth pattern of the selected patients, presence of crossbites, etc. All these potential confounders may affect the choice of mechanotherapy and the rate of orthodontic tooth movement.

Under Interventions, the rationale behind using a modified Nance transpalatal arch (as seen in Figs 1, *B* and 2, *B*) banded to second molars and the acrylic button placed in the second premolar–first molar region was not clear; why a transpalatal arch soldered to maxillary second molars was placed is not clear.

Figures 1, *A* and *B* and 2, *A* and *B* showed that bite blocks had been placed on first molars to open the bite for canine retraction, but this is not mentioned in the manuscript. What was the requirement of this bite-block? Was it given to all patients? Was this bite-block given to facilitate canine retraction in patients with crossbites? All these factors need clarification as the bite opening may influence the rate of canine retraction and quantum of anchor loss.

Figure 1, *A* showed that the mandibular arch had not been bonded. The canines were in Class II malocclusion and molars were in Class I malocclusion, and the overjet appears to be reduced. How this patient was treated for nonextraction in the mandibular arch was not clear, as the retraction of the maxillary anterior segment would lead to a dental crossbite in a patient with these attributes. Was it done in all patients?

We request that clarifications be provided for the above questions for the benefit of the readers. We compliment the authors for their efforts and hope these points will add to the impact of this lucidly written article.

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Am J Orthod Dentofacial Orthop 2021;159:404
0889-5406/$36.00

https://doi.org/10.1016/j.ajodo.2020.11.028

Authors' response

We thank the authors of this letter for their interest in our study.

Regarding the rate of tooth movement, Table I reports the change from day 0 in the time-points that tooth movement was measured. As the measurements were performed every 4 weeks, the numbers reported in Table I corresponded to a rate of millimeters per month (day 0–day 28), millimeter per 2 months (day 0–day 56), and millimeter per 3 months (day 0–day 84), respectively. Dividing tooth movement into millimeters per day or millimeters per week would have given minuscule numbers that would be of no importance. We considered that reporting the numbers, as in Table I, was more meaningful for all involved in the clinical process, meaning treating clinicians and those receiving treatment.

Regarding the second point, we agree this is a minor repetition. We could have included in 1 sentence that there was not any previous orthodontic treatment in the maxilla and overall.

*The viewpoints expressed are solely those of the author(s) and do not reflect those of the editor(s), publisher(s), or Association.

As far as the eligibility of the patients is concerned, extraction of maxillary first premolars was required as part of their orthodontic treatment irrespective of the underlying malocclusion. Researchers involved in clinical trials can appreciate the difficulties involved in patient selection and recruitment. It is thus obvious that it is not feasible to find and recruit patients with identical crowding, underlying malocclusion, skeletal pattern, etc. Extractions can also be part of various types of malocclusions of dental or skeletal origin, including patients who might require orthognathic surgery. By broadening the type of included malocclusion, the generalizability of our results is also broadened and applies to any case that might need canine retraction. The choice of mechanotherapy was completely standardized for all patients during the study. Patients agreed to participate in the study, with canine retraction being the first part of their treatment, and all other orthodontic issues were addressed at a second stage. It is reported in the Interventions section that leveling and alignment were performed before canine retraction and that day 0 was the day that canine retraction commenced. This means that all the *potential confounders* could not have affected mechanotherapy and rate of tooth movement during this 3-month trial.

As mentioned above, leveling and alignment of the maxillary dentition was performed before canine retraction. Experienced clinicians are aware that, very often, the maxillary first molars are rotated, crowded, or even ectopic. As a result, if these teeth are not leveled and aligned, it is impossible to insert working stainless-steel wires in their tubes when using the straight-wire technique. It goes without saying that sliding mechanics for extraction space closure are performed in stainless-steel wires, and all units involved (active and reactive) have to be effectively engaged to the working wire. We also know that increasing the number of teeth in the reactive unit increases its anchorage capability.[1,2] By adding the second molars to the reactive unit, stabilizing them with a Nance-transpalatal arch, and ligating together second molars, first molars and second premolars on each side, we aimed to preserve anchorage for patients who required this for their second stage of treatment while simultaneously allowing for unimpeded leveling and alignment of the first molars. Another option would have been to use temporary anchorage devices (TADs) to reinforce anchorage.

Nevertheless, TAD success can be device-, patient-, site-, and technique-sensitive, with success rates ranging between 56% and 100%.[3-6] In addition, TAD complications such as damage to the periodontal ligament, cementum, or dentine are usually of minor importance but can still sometimes lead to pulp necrosis or irreparable damage to the underlying tissues.[7,8] Considering all the above, possible TAD complications would delay treatment and possibly cause harm when placed into crowded interradicular spaces. This issue would have introduced another unpredictable factor that could have violated the standardization of treatment. During clinical decision-making, the design of the appliances is defined by treating clinicians and not vice versa. Although a Nance-transpalatal arch is commonly used on the maxillary first molars, it is neither the device nor the *usual* applications that dictate where an appliance will be anchored. Subsequently, it was decided to apply dentally supported anchorage and cement the Nance-transpalatal arch to the second molars to increase the size of the reactive unit, level and align the first molars and have a more predictable anchorage modality.

Regarding point 5 by the authors, it was mentioned in the Interventions section that "Occlusal stops were placed on the first molars to prevent any interferences during retraction." This mild bite raising was applied to all patients, even to those who might not obviously need it for standardization reasons. When we refer to "interferences," this also includes any cross-bite present on the path of canine retraction like the one shown in the illustrated case.

Finally, the illustrated case was treated with extractions in the mandibular arch, although this was irrelevant to the present study's objectives. For consistency and hospital clinic management reasons, maxillary fixed appliances were first bonded for all patients for the study's duration, and after the study completion, all further necessary procedures were performed for completion of their treatment on the basis of an individualized plan and to the best of patient care.

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Am J Orthod Dentofacial Orthop 2021;159:404-406
0889-5406/$36.00

https://doi.org/10.1016/j.ajodo.2020.12.010

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Posterior arch defects

Hearty congratulations to the authors for their publication on posterior arch defects (Selvam NP, Hansen M, Kashtwari D. Incidental findings of posterior arch defects of the atlas in orthodontic patients: a case series. Am J Orthod Dentofacial Orthop 2020;158:35-9). It was noteworthy that they recognized that the orthodontist might be the first to see, recognize, and report such findings, being the first to take cephalometric films to identify such anomalies. Highlighting the clinical significance of these entities, the authors' stated primary goal was well done, as well as pointing out how "the diagnosis of these defects could lead to a major lifestyle change." That is a correct assessment.

As doctors of orthodontics, we all attended courses in anatomy and physiology, and thus, should be very familiar with structures of the head and neck areas. Nevertheless, it is rather rare to see the recognition of cervical pathologies included in the diagnosis of our patients. Again, my congratulations to the authors and to the instructors who them to recognize these pathologies.

I do have a concern that was not mentioned in the article. The vertebral arteries, left and right, arise from the subclavian arteries and course upward through the outer lateral extremities of the transverse process of each cervical vertebra. At the upper extreme of the first cervical vertebra, these turn medially and upward, entering the foramen magnum, where they run anteriorly to join with the internal carotid to form the circle of Willis. Disturbance of any cervical vertebral structures can disrupt vascular supply to the internal cranium.[1] This results in anoxia to the brain. Varying degrees of headache are the customary disturbing result. Did the patients report cephalgia? Did the patients complain of cervicalgia?

The absence of the posterior arch of the first cervical artery would seemingly, or undoubtedly, alter the blood flow to the brain and dura mater, but no reference to this was made in the article. It would be most interesting to hear whether this was noted and reported in some part of these patients' records. What was the immediate complaint, if any, for these patients?

The authors did point out that these patients may be more prone to spinal cord injuries and should be cautioned to avoid contact sports. This being true, I am wondering what the authors' council is when straightening or reverse curve of the cervical spine is noted (kyphosis, also now referred to as "text-neck")? This was also noted in the cephalometric films displayed in your article. This is very common now in our cell phone, video gaming world.

Guzay's quadrant theorem[2] displays that the center of rotation of the mandible is between the first and second cervical vertebra. Did the absence of the posterior arch of the first cervical vertebra alter the function of the mandible in any way? This might be a substance for a future article. Please keep them coming. It was a pleasure to see the study of orthodontics expanded beyond teeth to the anatomic areas observable in our films that are also so important to the health of our patients.

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Am J Orthod Dentofacial Orthop 2021;159:406
0889-5406/$36.00

https://doi.org/10.1016/j.ajodo.2020.11.024

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