Letters

Bruxing and craniofacial growth

We are writing in reference to the article "The dentofacial morphology of bruxers versus nonbruxers", by Drs. Susan E. Menapace and coworkers [1994;64(1): 43-52].

We read this article with great interest, since it deals with an area important for understanding the factors influencing craniofacial growth. However, although the intention of the authors was to compare two groups with different functional demands on their masticatory systems, the selection criteria were not appropriate to guarantee this difference. The criteria used were both subjective (awareness of bruxism) and objective (wear faceting of the teeth). But both these criteria provide only a vague idea about bruxism. This is because most people are not aware if they grind their teeth or not[1] and because faceting of teeth is a normal process in most individuals, especially in a group in which the age limit is extended to 55 years.[2-3]

We therefore believe that the conclusions drawn in the paper by Dr. Menapace et al. can be misleading for the reader/researcher, that no difference was found in the dentofacial morphology between bruxers and nonbruxers. These results are in disagreement with previous works where measurements performed in groups with more distinct criteria (with advanced dental wear, i.e. an indication of an intense function of the masticatory muscles) have shown a relationship between facial morphology and intense bruxism. Individuals with advanced occlusal tooth wear had a brachycephalic "rectangular" facial morphology, a small angle between the mandibular and palatal planes and a small gonial angle.[4-6]

Stavros Kiliaridis, LDS, Odont Dr
Associate Professor

Gunnar E. Carlsson, LDS, Odont Dr
Professor and Chairman
Department of Prosthetic Dentistry
Goteborg University

References

1. Rugh JD, Ohrbach R. Occlusal parafunction. In Mohl ND, Zarb GA, Carlsson GE, Rugh JD, editors. A textbook of occlusion. Chicago, Quintessence Publishing Co., Inc. 1988:249-261.
2. Ekfeldt A. Incisal and occlusal tooth wear and wear of some prosthodontic materials. An epidemiologic and clinical study. Swed Dent J 1989, Supplement 65.
3. Johannson A. A cross-cultural study of occlusal tooth wear. Swed Dent J 1992; Suppl 86.
4. Krogstad O, Dahl BL. Dentofacial morphology in patients with advanced attrition. Eur J Orthod 1985;7:57-62.
5. Waltimo A, Nyström M, Könönen M. Bite force and dentofacial morphology in men with severe dental attrition. Scan J Dent Res 1994; 102:92-9.
6. Kiliaridis S, Johansson A, Haraldson T, Omar R, Carlsson GE. Craniofacial morphology, occlusal traits and bite force in individuals with advanced occlusal tooth wear. Am J Orthod Dentofac Orthop. In press.

Author's response

Drs. Kiliaridis and Carlsson offer several valid challenges to our paper. Their point that the bruxist and non-bruxist groups in our study may not have been sufficiently characterized – perhaps because people are unaware of their grinding – is arguable. We generally agree with them and addressed this issue in our manuscript in two separate parts of the paper. In our Discussion, we wrote: "One possible explanation (for no difference between the craniofacial morphology of bruxers versus non bruxers) may be that the bruxist and nonbruxist groups were not sufficiently differentiated". We also wrote on page 51, lines 1 through 7: "However, another explanation could be that the nonbruxers were unaware and/or denied their bruxism. Attanasio reported the prevalence of bruxism to be 15% - 90% in adult populations and 7% - 88% in chil-

dren, however, he reports that only 5% to 20% of the population were aware they brux.

In defense of our study and particularly the characterization of the two groups (i.e., bruxers versus nonbruxers), we took what we believed were the most "highly" differentiated bruxers (i.e., most wear facets and undeniably grinders) from our bruxist sample and compared only these individuals with the most "highly" differentiated nonbruxers (i.e., no wear facets and undeniably did not brux). Statistical analyses were then performed and these results were published in our paper, on page 48 under the heading "Faceting versus Craniofacial Indices". Interestingly, we still found no difference in the craniofacial morphology of our most highly differentiated bruxers versus nonbruxers. We performed this additional comparison and analysis because we, too, were influenced by the results of studies that demonstrated a difference between craniofacial morphology and intense bruxing and this was our *a priori* "working hypothesis".

Parenthetically, in the above regard, Drs. Kiliaridis and Carlsson cite results of studies, some of which are their own, that demonstrate a relationship between craniofacial morphology and bruxism, but fail to point out that there are perhaps as many contending papers, results, etc. to support the counterpoint view.

Donald A. Rinchuse,
DMD, MS, MDS, PhD
Associate Professor
University of Pittsburgh

Incisal bite force

The research article by Wood et al. on incisal bite force and condylar seating (1994;64(1):53-62) was well-done and proved something that has been known for over 20 years. It answered questions that previous studies did not and thus is a great contribution.

I was a student of Dr. Charles E. Stuart, who is referenced, for over 25 years until his death in 1951. I taught with him, and I know what he taught and believed. It is definitely not what is presented in the introductory paragraph of this article. Several points need clarification.

The references to Stuart and McCollum (#2, 3, and 4) were all published in "A Research Report" (reference #4) and comprise most of that publication. They were written in the 1930s and were primordial to what Stuart was teaching when he died.

The authors lump Bennett, McCollum, Stuart, and Posselt together as "gnathologists". Bennett and Posselt, although they wrote 60 years apart, were anything but gnathologists in the sense of understanding lateral border movements of the mandible, especially transtrusions of the rotating and orbiting condyles.

Secondly, none of these four authors ever talked about centric relation in terms of the condyles being "fully seated rearmost and midmost in their respective fossae." X-rays would be needed to confirm this, and they did not take joint x-rays on patients. Bennett did not have a clue and Posselt only talked about a terminal hinge centric relation with the condyles "centered in the fossae" (he did not take x-rays). McCollum and Stuart, in the articles referenced, talked about rearmost condylar positions for CR. They did not know anything about internal derangements. After McCollum died, Stuart never talked about the condyles when he discussed centric relation. His favorite quote was, "I don't care if the condyles are made out of mush and milk, my concern is their effect on the occlusal surfaces." He defined centric relation as the "rearmost, midmost hinged position of the mandible" (not the condyles).

The wax interocclusal record that Stuart taught[1] accomplished the same objective as the one used in this study and it was much simpler. Before he died, he developed equipment to be used on his computer (articulator) and proved the accuracy of his bite with similar scatterplots. Unfortunately, this work was never published.

The final point I would like to make has to do with an alterable centric relation. This, I believe, is what the authors meant to discuss in their second paragraph when they referenced Spahl, Witzig, Yerkes and Gelb. The authors did not postulate the problem correctly. It is not the dichotomy that they think it is, because if CR and CO do not coincide, functional appliances can be used to gain coincidence. This concept has yet to be presented in a scientific article, but it is a daily fact of life for many "functional" orthodontists.

Jack L. Hockel, DDS
Walnut Creek, California

1. Described in "Orthopedic Gnathology" by Hockel et al., Quintessence, 1993.

Author's response

Thank you for your interesting insight into the men and their beliefs through your personal relationships with some of the authors referenced in our article. It is very difficult for a young author like myself to be privy to the things you mentioned about Dr. Charles Stuart since I did not personally know him but relied only upon my interpretation of the literature.

My understanding of Dr. Stuart's first interocclusal wax record was that it used a tongue blade with compound material and a hard anterior stop and the teeth were registered with ZOE paste on ash metal. Later he moved to three pieces of 10X wax which granted is easier to use, but did not incorporate a hard anterior stop. Therefore, as a result of our article, Stuart's later interocclusal record may not allow complete seating of the condyle unless the patient has been fully deprogrammed from his or her occlusion by extended full-time wear of a centric relation splint.

I must admit that I have not had the kind of success with functional appliances that other researchers and clinicians have reported. This may be due to my own inability to work with and choose the appropriate cases for treatment with functional appliances. My own personal experience with functional appliances is that, once put into full-time wear of a centric relation splint as described by Roth, the end result is a significant CR-CO discrepancy. I have since chosen other treatment modalities such as extractions, headgear, restorative dentistry and orthognathic surgery. However, a well-designed study of post-functional appliance cases that have been stabilized after full-time wear of a centric relation splint for a minimum of 6 months should show the efficacy of functional appliances. I too, look forward to someday seeing such a scientific article, using the sample described above, in the literature.

David P. Wood

1. Roth RH, Rolfs DA. Functional occlusion for the orthodontist. Part II. J Clin Orthod 1981;15(2):100-123.

Extraction versus nonextraction

Once again the ubiquitous question of extraction vs nonextraction surfaced in the comments raised by Dr. Marvin Rosenthal (1993;64(2):84) regarding the case report by Dr. Gary Wolf (1993;63(4):251). Treatment of a complex orthodontic problem cannot be evaluated by a single criterion. Treatment goals, establishment of a problem list, and organizing the treatment modalities to address those problems seems to be the most efficacious way to approach patient care – something Dr. Wolf did admirably.

Publication of these complex case reports is fraught with difficulties. It takes time to treat these cases effectively and then more time to get them ready for publication. Rapidly changing technology affects the current reader list – which is positive – but the retrospective application of current practice guidelines or concepts may be confusing when the work in question was started 10 years ago, as in this case.

Three stars to Marvin Rosenthal for the courage and conviction to address his concerns and opinions in a public form; five stars to Gary Wolf for his excellently treated case and thorough redress of Dr. Rosenthal's remarks

John E. Grubb
Chula Vista, California

Editorial

continued from page 243

molars. Although the differences in periodontal attachment between previously impacted and contralateral control canines were small, a relatively high frequency of pulpal change was observed in the previously impacted teeth. The maintenance of posttreatment alignment was also noted as a problem. Although some concern for periodontal health is warranted after reviewing the results of this study, perhaps more attention should be paid to pulpal health and retention.

The impact of these two papers on my practice will be felt from the onset of diagnostic findings well into retention.