

Letters

Commentaries

I read with great interest the recent editorial announcing the expanded critique concept for enlarging the reviewer's influence regarding original research articles. This creates a great deal of responsibility for the reviewer to maintain objectivity, as well as Gestalt conceptualization, while reviewing the myopia of much research.

Few recently published articles are of more contemporary significance to the development of occlusion than Stanley Crawford's (Condylar axis position, as determined by the occlusion and measured by the CPI instrument, and signs and symptoms of temporomandibular dysfunction. *Angle Orthod* 1999;69[2]:103-116). The accompanying review by Straty Righellis was quite incisive for much detail of Dr. Crawford's material, but it failed to acknowledge the irrelevance of the CPI instrumentation to the conclusions of the study.

Scientifically speaking, any statement is only as valid as its most limited assumption. The CPI instrumentation has no quantifiable correlation with any anatomic point for an individual patient. The imaginary connector extrapolated between the points indicated by the two styli of the CPI instrumentation has been suggested to represent the horizontal transverse axis of the patient. The horizontal transverse axis of the patient is, by definition, also a virtual axis with no correlation to any identifiable

anatomy of a patient.

The implied correlation of the CPI instrumentation to specific anatomic location is merely a mingling of time-honored approximations. The facebow transfer apparatus, both arbitrary and true hinge axis type, gives nothing more than a guesstimate of general vicinity of gross condyle location. The wide range of variation between operators is well documented in the literature. As well, the centric bite registration, no matter what the technique, gives no indication of specific anatomic correlation to condyle location within the glenoid fossa. The centric bite registration is merely a recording of the relationships of the upper and lower teeth, at some degree of separation in space, coincidental to the doctor's subjective determination of when the centric relation conditions occur within the condyle/fossa complex. It is the doctor's clinical judgment that determines when the centric relation conditions are met, regardless of the registration technique used. Until the two styli of the CPI instrumentation are correlated to the glenoid fossa as well as the mandibular condyle, one is still "lost in space"!

Therefore, the clinical judgement of the doctor—subjective assessment if you will—is the limiting factor for the accuracy of the coordination of coincidence of centric relation occlusion and maximum intercuspation of the teeth. Any conclusions regarding the comparison of the samples examined must

be correlated to the clinical skill of the doctors and not to the instrumentation employed. Any suggestion that the use of CPI instrumentation will impart some protection to our patients against the development of TMJ symptoms is a fallacious leap of faith!

The CPI instrumentation may be a useful tool for many practitioners, but usefulness does not impart scientific validity. To perpetuate the misinterpretation of scientific validity implied by association with superior clinical results achieved by highly competent practitioners fails the test of critical thinking and common sense.

Additionally, Dr. Righellis has clearly crossed the line of impartial objectivity with his suggestion that it is appropriate to develop techniques that divide practitioners into groups that do "good dentistry" and by exclusion, those that do not. This kind of attitude is not in the best interest of our patients. The American Dental Association's Code of Ethics creates a bond between doctor and patient that allows us great latitude in judgment when treating our patients. The defining criterion for qualification of judgment is whether or not it "is in the best interest of the patient" or as it pertains to our specialty as a whole, "in the best interest of our patients." The promotion of techniques that are reserved for an exceptional few or that divide us into groups with the juvenile mentality of "I'm better than you are," clearly

fails this test. Our professional responsibilities mandate that we constantly strive to develop concepts and methods of evaluation that allow the greatest number of our colleagues to produce the highest caliber of treatment, thereby improving the quality of care for all our patients.

Evolved science and technology allows for a transcendence of the limitations imposed by the CPI/MPI conceptualization so that all practitioners are able to produce the quality of result reported by without being part of an elitist group! Empowering practitioners with knowledge and with the ability to easily quantify appropriate anatomic relationships with accuracy and reproducibility will define quality treatment for the profession as a whole in the future. Addiction to a technique of the past that was used by "superior" practitioners only distracts us from the importance of Crawford's message.

The article in the same edition by Drs. Karl and Foley (The use of a deprogramming appliance to obtain centric relation records. 1999;69[2]:117-125) also received an expanded critique. The review content and physical juxtaposition of these two articles compel comment. The stated purpose of the Karl and Foley study was "to investigate the difference between centric relation bite registrations taken before and after use of an anterior deprogramming appliance." Their data clearly showed that there was a difference in bite registrations taken before and after use of an anterior deprogramming appliance.

Undoubtedly, much controversy was generated regarding the deprogramming appliance design and the duration of wear. Dr. McLaughlin's review in this regard was insightful, enlightening and clarifying. His recommendations for further study were prudent and

appropriate.

The remainder of the data captured with their selected instrumentation, an estimated hinge axis facebow and Panadent articulator (Panadent Corp, Lake Oswego, Ore), provides only speculative clinical applicability. The Panadent instrumentation does not represent any individual patient anatomy landmarks. As well, the Panadent articulator is incapable of reproducing individual patient chewing strokes. This part of the data is consistent with Lysle Johnston's reference to gnathology as "the science of how articulators chew."

Unfortunately, McLaughlin chose to overlook this. His clarification of the clinical impact of the finding of "a difference in bite registrations taken before and after the use of an anterior deprogramming appliance" notwithstanding, he then chose to expand his review into a treatise on the merits of "mounting orthodontic study models on an articulator prior to diagnosis and treatment planning." There is no indication of what type of instrumentation McLaughlin is referring to when he uses the term "articulator," so one is left to assume that he is referencing the same class of instrument as the Panadent instrument used in the Karl and Foley study. This would be the arcane group of instruments based on the arcon concept of geometric movement.

McLaughlin listed eight advantages to mounting study models prior to orthodontic treatment planning and diagnosis. Advantages number 1, 2, 4, 5, and 6 address the instability of holding the study models in one's hand. Clearly he is right on here, as the articulator provides a much better handle for stability. Who among us has not dropped and damaged study models at some time? Even if the articulator is dropped, the metal housing may provide signifi-

cant protection to minimize and possibly even prevent damage to the models themselves. Aside from damage, McLaughlin suggests that the instability interferes with one's ability to assess occlusal relationships from a three dimensional perspective. This disadvantage could easily be eliminated by cutting a hole in our consultation table and covering it with clear Plexiglas. This should allow us to walk all around the models as they sit on their heels and to crawl under the table for our lingual inspection without fear of instability. We would then only have to decide what type of misinformation we want. Do we like the misrepresentation of movement created by arcon articulators or do we like the misrepresentation created by moving casts on their heels? The advantage to hand held models is that their limitation is clear. I don't know of one single dentist who thinks that it is possible to reproduce many functional movements of a patient with hand held models. There may very-well be some poor unsuspecting dentist who makes the erroneous assumption that the movements created by arcon articulators have something to do with individual chewing stroke movements for an individual patient.

Advantage number 3 definitely has merit, as long as there is no distortion created by the twisting motion of tightening the bite fork to the facebow. Too much tightening and we may get distortion at the time of recording. Too little tightening and the bite fork may move in the laboratory before or during the mounting process. The posterior-anterior cephalogram is a better source for this information.

Advantages 7 and 8 may be the most attractive yet, as they speak to economics. Hopefully, the majority of us make decisions allowing "the patient's best interest" to

maintain priority over economics. Mounting models should be done for the insight and accuracy provided to our diagnosis, treatment planning, and prosthesis construction. This would be congruent with our code of ethics and "in the best interest of the patient." If this mounting provides anatomic truth, as can best be quantified, and functional accuracy dictated by the anatomy-generated determinates of the individual patient, the benefits of mounting casts for certain patients would be unquestioned. If the mounting of these cases provides otherwise, the efficacy must be questioned and personal liability for the therapeutic repercussions are seemingly increased.

With adherence to the mandate for mountings representing anatomic truth, every word of McLaughlin's final paragraph is not only insightful and deserving of respect but, hopefully, prophetic.

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Clinical research

As a researcher, teacher, and clinician involved in the field of TM disorders, I found the recent article by Stanley Crawford to be seriously flawed. This article violates so many fundamental rules of clinical research methodology that both the results and the conclusions drawn from them become practically meaningless. This situation is compounded by publishing a commentary on that article from a reviewer who either ignores or forgives the obvious problems regarding the methodology, results and implications of this study. However, as the Editor points out in his editorial, readers must determine whether the two groups compared in this study have been properly selected "before deciding whether these findings will stand up in the long run." This is indeed

the primary issue to be criticized here, since no paper can stand up if both the experimental group and the control group are improperly selected, the chosen outcome measures invalid, and the significance of the results extrapolated beyond the outcome of the study.

It is clear that both Crawford and Righellis believe that a significant breakthrough has been made here in studying relationships between occlusion and TM disorders. Crawford decided to investigate the importance of condylar position (CR) rather than looking at occlusal relationships, so he feels free to dismiss all previous occlusion/TMD studies as being essentially worthless because "One would not expect that studies purporting to evaluate occlusion, but failing to accurately assess condylar position, would find definitive evidence of the relationship between occlusion and TMD." This comment echoes criticisms of the current TMD literature expressed by many advocates of specific occlusal philosophies. However, this paper fails to provide evidence that would convince anyone to dismiss 20 years of carefully performed clinical and epidemiological studies, most of which have shown that there is little or no relationship between most occlusal or condylar position variables and TM disorders.

There are so many serious methodological errors in this study that it is difficult to know where to start—but let's begin with a concession to the one part that seems to have been done correctly. The results of the CPI axis locations, as shown on the graphs in Figure 5 and statistically analyzed in Table 2, appear to be accurate and valid. No surprise here: the 30 patients who have been deprogrammed to CR and then reconstructed to a gnathologically ideal occlusion all have their CR dots clustered around the center-point of the graph, while the con-

trol subjects have dots all over the place. The problems arise in the TMD end of this study—beginning with the choice to use a modified form of the Helkimo index as the diagnostic tool. As Greene and Marbach¹ showed in 1982, this index has limited applications because it tends to find "positive" TMD results in a large percentage of asymptomatic people when used as a population screening device; any alteration of the HI (as was done in this study) only further diminishes its reliability and validity. Since numbers are assigned to the severity of each sign and symptom in the HI, investigators were able to make quantitative analyses and comparisons among various groups, but it never was clear how many people in each group actually were "sick." After an initial burst of overuse in the early epidemiologic studies of the 1970s, in which frighteningly high numbers of the normal population were found to be afflicted with TM disorders, the Helkimo index became somewhat discredited. Now it is mainly used as a before-and-after recording form in clinical TMD treatment situations, but only rarely in quantitative clinical studies.

Crawford seems to be unaware of these significant methodological limitations, so he was not surprised or skeptical when he found that 76.6% of his "untreated control" population had TMD symptoms, or that 16/30 controls had a "severe" TMD symptom score and 11/30 had a "severe" TMD clinical examination score. Incidentally, his control population is completely inappropriate for any sort of research project, being comprised of 30 people "arbitrarily selected from the patients and staff of local dental offices and from pretreatment records from the author's practice," matched for sex but not for age. This control group deviates so far from conventional research stan-

dards that any data obtained from it cannot represent anything but a set of descriptions. Needless to say, the comparisons that are made with such a group lack any semblance of meaning or validity.

Meanwhile, Crawford's patient group is highly selected for their occlusal and condylar perfection after reconstruction by "expert clinical gnathologists," but no further description of this group's pretreatment occlusal or TMD status is provided. All we know is that he could not find 30 people in the normal population who have these perfect characteristics, which ought to raise some questions about the words "perfect" and "normal." Since he did not have pretreatment Helkimo index data on these patients, he asks them to remember their TMD signs and symptoms from an average span of 10 years ago (some over 20 years ago) in order to contrast them with their current HI scores. Using this absurd baseline, Crawford concludes that these patients now have an 84% improvement in TMD symptoms. Even Righellis recognized this obvious methodological flaw, and he added that perhaps only "happy" patients were recalled for this study, but he excused all of this by saying that it was probably typical for most retrospective studies.

So now we have three groups of numerical data—the highly accurate CPI measurements, the unverifiable HI memory scores for the 30 patients pretreatment, and the current HI scores for both groups. All of the comparisons between the groups as well as the statistical computations are based on the presumed validity of all of these numbers. The description of the statistical analysis performed and the statistics published makes one wonder how the data obtained in this study was actually analyzed. We need to know if the author performed regression analysis or cor-

relation, because interpretation of the results will vary depending on the analysis performed. However, it is important to note that independent of the analysis used, none of these results can provide us with an explanation of the causal relationships between these variables.

Since the ultimate point of this paper is that the patient group is much healthier TMD-wise than the control group (and that this health is directly related to how close each subject's TMJ is to perfect CR, as shown in Figure 4), we have a real problem if the HI data does not represent the TMD health status of either of these groups. Yet Crawford wants orthodontists to believe that they "could have a positive impact on TMD symptomatology" by getting CO to be as close as possible to CR. This may or may not turn out to be true in some future study, but it certainly has not been demonstrated by this one. In the meantime, the orthodontists who read this journal will have to continue to struggle with the occlusion/TMD enigma, while continuing to strive for stable and functional orthodontic results as a goal unto itself—not for the sake of a happier and healthier TMJ.

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References

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Value of CPI, MPI

Having spent several years teaching and lecturing on the topic of gnathic function, and having practiced orthodontics for 27 years with functional goals in mind, I found Stanley Crawford's article to be important and timely. His approach to improving comprehen-

sion of TMD as it relates to the occlusion is unique in the prevailing literature. The MPI and CPI instruments he uses are relatively new and provide significant improvement over previous means of measuring changes in condylar position. Obviously the TM joint is not readily available for measurement, and methods used heretofore, such as clinical examination, history, imaging, and articulators per se, have been inadequate.

Besides using advanced instrumentation, the study is clever in its design. It is a basic tenet of health science that one must know healthy form and function in order to properly diagnose and treat malform and malfunction. Crawford's work serves this axiom by bringing forth a sample that is close to ideal, and thus he provides a foundation for comparison with other groups. Most current studies, for example, epidemiologic ones, provide no such frame of reference. In many disease processes, a group of "average" individuals will not fit the definition of "healthy."¹

There are those in the profession who assert that there is no significant functional/dysfunctional relationship between the dentition, neuromusculature, and joints. No doubt they will cry "foul" in response to this study. But Crawford's work meets the test of basic scientific epistemology.² Science can be thought of as a search for corroborated generalizations, starting with hypothesis, working through theory and law, toward (but rarely achieving) absolute.³ Science is achieved through a progressive accumulation of support for generalizations (hypotheses).⁴ Crawford's study clearly fits this criteria as it tests the generalization: There is a cause/effect relationship between form and function of the gnathic system and symptoms of TMD. He extrapolates this thinking to a new and

more refined domain and tests it there. Thus, by using the same thought process as those before him, but a different (and more sophisticated) method of observation, he documents a relationship and strengthens the generalization. The study isn't perfect (none are), but it is scientific.

There are some widely accepted basic principles regarding form/function of the gnathic system that embellish Crawford's study:

1. The gnathic system is a musculoskeletal system wherein the joints, neuromusculature, and dentition are intimately linked, thus a change in one element (the dentition) is likely to affect the others.⁵⁻⁹

2. The dentition, or occlusal joint as Moffett¹⁰ refers to it, and the temporomandibular joints share the same bone and are served simultaneously by the same musculature, and condylar position is determined by the occlusion at the intercuspal position.¹¹

3. Crawford cites several studies indicating that the ideal sagittal position of the condyle is forward and uppermost against the eminence when the joint is loaded by the elevating musculature. There are several sources in addition to those Crawford cited.¹²⁻¹⁷ Okeson¹⁸ and McNeill,¹⁹ in separate forums, encouraged orthodontists to use this condylar position as a treatment goal.

Controversy arises as to whether or not achieving a near "ideal" condylar position reduces the incidence of TMD.

Condylar distraction away from the eminence constitutes a joint dislocation^{20,21} and violates the important principle of joint function emphasized by Sicher: "...sharp contact between the articulating bodies is always maintained. This means that the condyle, disc, and eminence are in contact in rest, in all movements, and in all positions."²² That this dislocation can be

caused by the occlusion and is an etiologic factor in TMD has been recognized by numerous authors.^{7,23-28} Though describing it differently, all observed essentially the same phenomenon. McNeill²⁹ and Roth³⁰ identified the problem more succinctly by emphasizing the role of posterior teeth in distracting the condyle downward and backward. Luceke and Johnson³¹ found condylar displacement during orthodontic treatment to be associated more with posterior tooth movements than incisor movements.

Heretofore, dentists have thought in terms of the "centric slide," assuming that the condyle slides downward and forward along the eminence as the teeth reach the intercuspal position. Lee estimated this to occur in only about 10% of cases; more often the condyle is distracted (fulcrumed) away from the eminence when the mandible is closed to the intercuspal position.³² It is important to emphasize that condylar distraction down and back is a common finding, and Crawford's data indicating 70% is consistent with other studies he cited. If the condyle is distracted down and back, the possibility for numerous dysfunctional conditions may occur: loss of juxtaposition of the condyle disc and eminence, opening of the space between the condyle and the eminence, which tempts disc displacement,³³ stretching and denervation of the temporomandibular and collateral ligaments³⁴ muscle hyperactivity and/or incoordination.³⁵

It should be emphasized there is extra value in this paper that may go unnoticed due to the focus on TMD and occlusion. The stability of the ideal sample is truly remarkable—the orthodontist's dream! If the teeth had moved significantly, the centric occlusion dots measured on the CPI would not have been within 1 mm of centric relation. This finding contradicts cur-

rent thinking that the morphology of the system begins changing after treatment and continues to do so throughout life. Crawford's study tends to support Hansson³⁶ and Slavicek,³⁷ both of whom suggested that proper function can maintain form.

Celenza³⁸ is often cited, as he also reported on gnathological restored cases and showed the hinge axis location to have changed significantly with time. Crawford's findings, on the other hand, suggest that this is not necessarily the case. As an orthodontist, the idea of improving stability excites me. If it can be done once, it must be possible! It's discouraging to see a beautifully treated case return to the office a few years out of treatment with extensive attrition and heavy wear facets. Crawford's sample contained several cases that were reconstructed due to "severe attrition." Apparently these patients did not reabrade their dentitions despite the long time since their restorative work. If they had worn the restorations significantly, then at least in some of the cases, the centric occlusion dots would not have been within the 1 mm range that Crawford found.

Some argue that since this nearly ideal condylar position rarely occurs in the general population (Crawford found 5 out of 30), then it is not "normal" and should not be sought as a treatment goal. I suggest a different line of thinking. What is commonly found in dentitions of the general population is varying degrees of decline of the structures (often severe decline in adults). Thus, the "norm" is not a proper treatment goal. In order to achieve the lofty stature of "health," one must shoot for an ideal. Thus we are provided the opportunity to elevate orthodontics beyond the level of tooth alignment and cosmetics.

Hats off to Crawford. This is an

important study. The magnitude and quality of his efforts are to be applauded. We need more like it.

Andrew Girardot,
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(Note: The references cited in this letter are available on our website, www.angleorthodontist.org)

Author's response

Thanks to Drs. Greene, Leever, and Girardot for their letters.

Leever begins his letter by claiming that the commentary author, Straty Righellis, failed to acknowledge the irrelevance of the condylar position indicator (CPI). The reason Dr. Righellis made no such acknowledgment is simple—CPI is relevant. CPI accurately measures deflection of the condylar axis away from the seated position (centric relation, or CR) when the teeth are in maximum intercuspation (centric occlusion, or CO). It is this deflection, or CR-CO shift, and its relationship to TMD that I studied. For reasons explained in my paper, the axis can be used as a representation of the condyle. Any change in the position of the condyles creates a corresponding change in the axis as well; therefore, condylar movement away from CR will be reflected by axis movement. One is not "lost" in space if the axis is not correlated anatomically to the glenoid fossa and the condyle.

Leever does recognize that the location of the axis in CR is technique-sensitive; however, I think he does not really understand the concept of CR. By definition, CR is the most seated position of the condyle in the fossa and is determined by the structures of the joint. It is repeatable from operator to operator and has been shown to be stable at the clinical level, in healthy joints, over a long period of time by many, myself included. This is why it has been used successfully as a reference position. The reason some of

the literature shows large variations between operators is that CR and the axes were not located correctly in the first place. Patients were not deprogrammed prior to the procedure, and the procedure itself is technique-sensitive.

I think Leever misinterprets Righellis as suggesting that certain techniques are used to divide dentistry into good and bad dentists. Use of a certain technique or instrumentation does not define good dentistry or a good dentist, and Righellis does not imply that. His message is simple: If condylar position is important (and my study shows that it is) then its evaluation must be included in the diagnosis and treatment of our patients. To date, the instrumentation used in this study is the most effective means available to us.

Green's critique deserves some comment as well. My study was prompted by concern for our specialty. Researchers are finding that there is little or no relationship between occlusion, condylar position, and TMD; and because the position of the condyles has been deemed unimportant, we have no universal goals relating to condylar position. Our specialty is currently using this research to absolve itself from any responsibility for adversely affecting the health of our patients. This is in direct opposition to what clinicians, both in restorative dentistry and orthodontics, have been observing for years. Absence of scientific proof in the literature does not mean that it does not exist.

Greene dismisses the study in its entirety as "practically meaningless." I believe the study was done fairly and in adherence to accepted research protocol, and it was evaluated by qualified reviewers before being published in a refereed journal. Admittedly, the methodology is not perfect; however, the evidence presented is strong and should not be ignored.

The two groups in the study were first compared with each other, then the axes were combined to eliminate any influence due to sample selection. Comparisons were made that were not included in the study because the numbers were too small, but they might be of interest. Individuals within the ideal range were compared with those outside it. Subjects in the control group whose axis locations were within the ideal range had very few TMD symptoms and compared closely with those of the ideal sample. Forty subjects were collected for the ideal group, 10 of whom failed to qualify because the magnitude of their condylar distraction exceeded the ideal range. The index scores of the 10 were much higher and were close to those of the control group with regard to symptoms. It made little difference how the comparisons were made, the data always pointed in the same direction.

The Helkimo index was modified and used in this study only as a convenient means of quantifying the exam and test scores so they could be compared statistically; to declare the findings unreliable due to the use of this index is a stretch.

The pretreatment scores of the ideal group were included in the study for interest only; it was not included in any of the statistical analyses or in any of the conclusions. This point was apparently missed by Greene.

Greene seems to believe that teeth have no relationship to the TMJ or masticatory musculature. Such a concept would be laughable if it were not such a roadblock to proper understanding of signs and symptoms of many disorders, including long-term instability of the teeth, excessive tooth wear, and other damage done by disharmony of the neuromuscular system, problems that are missed when one focuses only on symptoms.

Fortunately, there are many in academia who are aware that the position and condition of the TMJ is directly related to how the teeth come together. Also, there are many in the scientific community who have shown, with excellent research, what happens within the masticatory neuromusculature when there is disharmony between the teeth and the TMJ.

I challenge the academic community to become familiar with the skills and techniques of observing and measuring condylar position so they can devise a study with a methodology more to their liking.

Stanley D. Crawford, DDS
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Commentators' responses

Thanks for the opportunity to respond to Dr. Leever's letter. As a commentator, my responsibility was to highlight the merits and limitations of the research paper; it is up to the reader to determine the value of the information.

It is clear that Leever's interpretation of the paper and its commentary conflict with his philosophy of occlusion. As a result, I find it unnecessary to address each of his points; I would, however, like to clarify a couple of comments for other readers.

Leever wrote that "the horizontal transverse axis of the patient is, by definition, also a virtual axis with no correlation to any identifiable anatomy of the patient." The hinge axis has been an accepted concept in dentistry for decades and the hinge axis is measuring the approximate arc of closure of the mandible in the rotation phase of opening and closing!

He also states that it is the "...doctor's clinical judgement that determines when centric relation conditions are met, regardless of the registration technique used." Yes, this is true. The paper simply describes a wax bite technique that

captures a centric relation position minimizing proprioceptive input. Many orthodontists eventually recognize this position after initial leveling wires are placed during active orthodontic care. The wax bite technique is an easy way to see what you are treating to.

Leever's additional inference that the reviewer's comments divide practitioners into "good dentistry," and by exclusion, those who do not do "good dentistry" is an utterly inane interpretation of the commentary. He sprinkles his letter with phrases of "I am better than you" and "elitist group," a totally inaccurate interpretation.

Straty Righellis, DDS
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The letter by Dr. Leever demonstrates the passion and friction that can be evoked when centric relation and articulators are discussed. I apologize if my commentary offended anyone in this way, as it was not intended to. I discussed the paper and the commentary with Dr. Tom Foley after publication and we remain good friends.

There are a few areas of confusion and contradiction in Dr. Leever's letter that I would like to address. He correctly pointed out that the facebow device provides only an estimated location of the maxillary cast on the articulator. It is unfortunate that we cannot be more precise, but this is the case. There was also concern over the tightness, or lack of tightness, of the bite fork. With some practice, this can be overcome and should not be a stumbling block in the process of mounting study models. The wax bite recording is fraught with difficulties. With the best of recording techniques, one cannot be assured that the condyles are in the ideal position in the fossae. The skill and judgement of the clinician are important and subject to significant variation. Also, the condition of the

patient's musculature and joints may make it impossible to take an accurate wax bite, and splint therapy may be needed before a recording can be made. However, these difficulties are the same whether one is mounting study models or using hand-held models. (The exception would be if no wax bite is deemed necessary and the models are trimmed in maximum intercuspation.)

Another accurate statement was that semiadjustable articulators provide only a static view of "the relationship of the upper and lower teeth, at some degree of separation in space" (preferably minimal separation). Such articulators are provided with only average condylar path devices, etc., and are of little or no help relative to accurate functional movements. This is a well-known fact. The most puzzling part of the letter was the frequent reference to "chewing strokes," "misrepresentations of movement," "geometric movement," "functional accuracy," and "anatomic truth." Just so we are all clear, these articulators only record estimated static relationships.

This does not seem adequate reason to abandon an attempt to determine if there is a difference between maximum intercuspation and centric relation.

There was some concern about the type of articulator I used. Since, in my opinion, most semiadjustable articulators provide essentially the same static information, I didn't feel it necessary to state which one I used. However, I use the Denar articulator.

Most enlightening, was the reference to the ADA code of ethics and the concern about "the best interest of our patients." Dr. Righellis and I surely need to review the code so as not to step over the referred to line in the future.

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The Edward H. Angle Research Prize

1999

James C. Boley

Facial changes in extraction and nonextraction patients
The Angle Orthodontist, 1998; 68(6):539-546

1997

Robert E. Rosenblum

Class II malocclusion: Mandibular retrusion or maxillary protrusion?
The Angle Orthodontist, 1995; 65(1):9-62

1995

Phillip M. Campbell

Enamel surfaces after orthodontic bracket debonding
The Angle Orthodontist, 1995; 65(2):103-110

1993

Sheldon Peck

The gingival smile line
with co-authors Lena Peck and Matti Kataja
The Angle Orthodontist, 1992; 62(2):91-100

1991

Ram S. Nanda

Effect of orthodontic forces on blood flow in human gingiva
with co-authors Kazunori Yamaguchi and Terushige Kawata
The Angle Orthodontist, 1991; 61(3):193-203