Letters

Case Report WI: Molar extraction in adults

I especially enjoyed Case Report WI in the recent edition of *The Angle Orthodontist* (Artun J, Mirabella AD. Case Report WI. Angle Orthod 1994; 64(5):327-332). Information on the success of molar extraction in adult treatment is indeed limited. One reason for this is the widespread use of fluoride during the tooth development years. Perhaps a second reason is the difficulty in managing these cases with the edgewise appliance. The authors are to be commended for their treatment of this case.

Within a month of my graduation from the orthodontic program at the University of Buffalo, Tick Beg paid a visit to my then-associate Sid Brandt. Dr. Begg spent a week in our office and I distinctly remember discussing molar extractions with him. Of course, the "bombed out" molar was relatively common at that time of the 1960 AAO meeting that had brought Dr. Begg to our shores. He watched as I bent a double-back wire with lingual crown torque incorporated in it, as would be the case with edgewise technique. He very tactfully said to me, sotto voce, "Now what on earth would you do that for?" He was right, of course. The molar extraction case became routine for us when molars were short-lived because of caries or extensive restorations. A 1975 article in the American Journal of Orthodontics (68:15-41) illustrates several molar extraction cases.

Fortunately, these situations are now less prevalent. However, there is another largely overlooked instance where maxillary first molar extraction is an excellent choice. I refer to the stubborn Class II case that just will not reduce to Class I despite magnets, headgear, TPA, elastics, or removables. This type of case is often resolved by removing two maxillary first premolars. This sometimes leads to relapse in the form of extraction spaces reopening and/or deepening of anterior overbite.

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The CR-CO discrepancy

The article, "The CR-CO discrepancy and its effect on cephalometric measurements," (Shildkraut M, Wood DP, Hunter WS. Angle Orthod 1994; 64(5):333-342) was a poorly conceived investigation. The premise of the paper is muddled and unsupported by hard scientific data. Because this study is descriptive rather than experimental or observational, a sound theoretical basis is imperative. However, the general notion for the paper is two or three decades old; only the definitions have changed. Instead of relating CR-CO discrepancies to TMDs, the authors have attempted to relate them to cephalometrics. The scientific data over the past two decades has generally demonstrated no relationship between TMD and CR-CO discrepancies (or any other functional occlusion variable), and the "gnathologists/occlusionists" have sensibly abandoned their claim of a causal relationship. In this regard, the Shildkraut et al. article appears to be an ill-founded attempt to relate a lot of nonsense to orthodontic diagnosis. Having read this paper, one is tempted to ask, "So what?"

I would like to address several of the paper's more important weaknesses. First, the authors have sought to compare the proverbial apples and oranges by juxtaposing CR and CO records; the terms are not comparable. CR has always meant a condylar position, i.e. the position of condyles in the glenoid fossa, while CO has always been an interocclusal position of opposing maxillary and mandibular teeth.

The term CRO emerged in the dental literature in the 1960s and '70s to denote the interocclusal position of the teeth when the mandibular condyles were in CR.

Second, the authors fail to define CR and CO. Some might view this as a frivolous, overcritical point, but I think it is important because the definitions of CR and CO have changed over the years. *The Journal of Prosthetic Dentistry* has published six editions of the "Glossary of Prosthodontics Terms." In the first edition in 1956,[1] the editors advocated a CR position that was posterior and superior, i.e. "retruded," within the glenoid fossa. By the fifth edition in 1987 they had endorsed an anterior and superior position for CR. The definition of CO has also undergone a change, from "the centered contact position of the lower occlusal surfaces against the upper ones; a reference position from which all other horizontal positions are eccentric" in the third edition, to "the occlusion of opposing teeth when the mandible is in centric relation. This may or may not coincide with the maximum intercuspation position" in the fifth edition. Interestingly, in the sixth edition, they defined maximum intercuspation as "the complete intercuspation of opposing teeth independent of condylar position." I assume that when Shildkraut et al. use the term CO, they really mean maximum intercuspation, but they do not define the terms and the text gives little help.

The authors rely on work published in the 1970s, including Roth (1973), Wood (1977), Williamson (1978), and Aubrey (1978), so one might assume they accept the older CR position of posterior and superior. However, on page 340 of their discussion they write, "By definition, the condyle cannot move forward without an inferior vertical component if it has been seated superiorly and anteriorly against the disc initially." Based on this, they appear to advocate an anterior and superior CR position. Hence, there seems to be a contradiction in terminology in the paper. The reader needs to know what definitions of CR and CO the authors ascribed to and recorded in their paper. If they stand by the older definition, then their paper could simply be considered outdated. Conversely, if they used the newer definition, they have compared works by different authors without considering the differences in definitions those authors used. And regardless of what their working definitions are, they rely far too much on anecdotal reports. Very little of their discussion is supported with experimental evidence.

Third, although few in the profession would question the need to evaluate orthodontic patients for "Sunday bites," there is no persuasive argument to support the notion that patients should be treated to any particular condylar position. [Editor's note: The author cited 30 references here; they are available upon request.] It appears that the range of normal variation in the human species in regard to condylar position is quite large. Most humans appear to function within a range from the concentric condylar position to the anterior, superior position. Few individuals function in the posterior, superior condylar position so it makes no sense to diagnose and treat patients to a particular condylar position. Until Shildkraut et al. can substantiate the benefits of treating patients to a particular position, the premise of their paper will remain simplistic and without merit.

Dr. Lysle Johnston Jr.[2] observed that "The speciality of orthodontics has for years been badgered by a variegated assortment of gnathologists, occlusionists, and the like, who argue that orthodontic treatment should produce a so-called 'centric relation occlusion'... Unfortunately, I know of no convincing evidence that condyles of patients with intact dentitions should be placed in centric relation, or that once having been placed there, the resulting improvement on nature will be stable...."

Finally, why should the orthodontic profession be once again mislead? In the 1970s and '80s, Dr. Ron Roth preached that orthodontic patients should be treated to the "retruded" CR position. Today, Dr. Roth admits he was wrong and argues in support of the anterior and superior CR position. Many orthodontists were captivated by the views of the self-proclaimed experts of that era and did not demand proof in support of the new theories.

Finally, what meaningful information or inferences can be derived from the findings that CR varies from CO more than 2 mm for a se-

Correction

In Eric Loberg's Letter to the Editor [Angle Orthod 1994;64(6):404] regarding the dosage of Thyroid, the correct dosage should have read half-grain (not gram). *The Angle Orthodontist* regrets the error.

lected few individuals? The accuracy of such data is suspect for numerous reasons. Notably, what validates that the CR record established in the Shildkraut study assures that the condyles are truly in the anterior and superior CR position (or the posterior and superior position, if this was advocated)? The authors do not furnish the TMJ or MRI data, or any other TMJ imaging to confirm subjects' condylar positions. Furthermore, they do not cite even one reference that validates their CR record. In addition, they fail to discuss the reliability, validity, sensitivity, specificity, measurement and instrument error of the articulator and MPI instrumentation/method, CR and CO registrations, face-bow technique and transfer, use of average values for the articulator, "hinge axis," etc. The instrument and procedure error from the above must be calculated and factored against the study's obtained values in order to derive overall, "absolute" values. When the authors consider millimeter and fraction of a millimeter difference as clinically significant, it is imperative that the accuracy of their data be assured. Curiously, Shildkraut et al. performed a reliability test for ceph tracing error, which is by now a "known" in research endeavors, but they totally disregarded the potential error(s) of their other recordings and measurements—i.e., articulator, MPI, CR, and CO recordings, etc. Nevertheless, if we assume the accuracy of their "CR-CO" data per se, what clinical significance do we make of CR-CO differences? In this regard, I know of no convincing scientific information that would indicate that CR-CO differences of any magnitude are indicative of TMD or a premorbid flag for such.

Quite puzzling to me is the fact that although these authors seem to accept the anterior and superior CR position, their findings and data are somewhat comparable to data from the 1970s and '80s, when retruded CR was used. The migration of retruded CR to an anterior and superior position would have wiped out most, if not all, CR-CO slides. Would there not be several millimeters difference from posterior, superior CR to anterior, superior CR? And is this not about the magnitude of CR-CO differences reported in the 1970s and '80s? What explanation is given for this paradox? Can this also be explained away by molar fulcruming?

In conclusion, the paper by Shildkraut et al. has many limitation that make the validity, and therefore, the conclusions, suspect. The paper could present a danger for those not trained in research or for those who only perform a cursory review, since they may accept the face-value claims of this paper without challenge.

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Author's response

It is unfortunate that Dr. Rinchuse found our paper to be a poorly conceived investigation. He seems to be upset because we didn't try to relate CR-CO discrepancies to temporomandibular disorders. This was never our intent. Perhaps Dr. Rinchuse found our study poorly conceived because he has been an opponent of anything to do with functional occlusion in orthodontics. He is obviously a well-read, well-intentioned person with good reasoning powers; however, in reading his discussion of the paper, it is evident that he is unfamiliar with an articulator or a mounted orthodontic case. In our paper, we tried to correlate what a growing number of orthodontists who routinely use articulators see on their mountings to what a corrected ceph would show—nothing more and nothing less.

For almost 10 years now I have mounted all my orthodontic cases on an articulator. This practice came about because of a patient I treated in graduate school. She had a full cusp Class II malocclusion and all cephalometric numbers indicated she was a good grower. After 18 months and with the help of cervical headgear and Class 1 elastics, her 7 mm overjet had turned into 2 mm, and the full cusp Class II relationship had became an Andrew's Class I. She finished treatment and I assumed that was the end of it. But later, after I went into private practice, I would occasionally run into her around town and she would complain that her "overbite" was returning and that the new graduate students had her in a bionator to "grow" her lower jaw. I had recently attended a course given by Dr. Ron Roth, and I felt I knew the reason for her complaint, so I offered to have a look. I had learned how to take a proper CR bite registration, and she was the first case I mounted on the SAM articulator I had just purchased. Her 7 mm overjet had became a 10 mm overjet after 3 months full-time wear on a CR splint. Furthermore, a Class II relationship worse than what she started with was finally revealed: she was fulcruming off of the second molars that were never banded, and the 80% overbite was now a 10% openbite. Had I been privy to the information derived from our paper on converting a CO ceph to CR, and had I used an articulator mounting instead of handheld models, I would not have totally misdiagnosed this case. Nor would I have spent 18 months of comprehensive orthodontic treatment pulling her jaw out of the socket and erupting her back teeth. I subsequently retreated her for free with 20 months of comprehensive orthodontics, the removal of two maxillary bicuspids (she didn't want surgery), a postorthodontic gnathological equilibration on the true hinge axis, and building up of the cuspids that were worn flat by her bilateral balancing interference.

I tell this story because it was a major turning point in my orthodontic career. My goals changed almost overnight from just straightening teeth to providing a gnathologic occlusion and becoming a better diagnostician. I have been in the trenches long enough to see how a case treated with CR holds up compared with other treatment modalities that result in worn, shifting teeth and TMJ problems. Our orthodontic patients' teeth should last a lifetime, not wear out before age 30.

The study of occlusion requires clinical discipline and knowledge in addition to textbook mastery of procedures. Traditionally, the best clinicians in the world are those who adhere to the disciplines established years ago by the McCollums, Stuarts, Thomases, Lees, Pankeys, and Roths of the world. These are the people who can show repeated long-term successes and can teach their followers to do the same. Yes, the clinical area of occlusion is "technique sensitive" as is all of dentistry and orthodontics. Can we reduce the clinical practice of orthodontics to "hard science"? I think not. But that doesn't mean we shouldn't try. This is precisely what was we attempted in this paper: to look for a scientific base for the occlusion concepts that work clinically. The job is difficult enough without having detractors throw rocks in the path.

The methodology used in this study and its repeatability was verified in a thesis by David Lavine at the University of Detroit Department of Orthodontics. The instrument error for the instrumentation was referenced (Wood and Korne) and the sample came from my practice. I took CR bite registrations and did the MPI recordings myself. One would hope that after 10 publications and the Chairmanship of a graduate orthodontic program, I would know something about experimental design.

Dr. Rinchuse is correct about one thing: the issue of terminology. The "powers that be" in the American Association of Prosthodontists who are in charge of terminology have changed the definition of terms that had been in common usage for decades. Dr. Rinchuse devoted a great deal of space to his discussion of terminology and he uses terminology as a major part of his argument. With the changes in definitions that have been instituted recently, it can be difficult to convey what one is talking about relative to occlusion.

Centric occlusion used to mean "habitual closure," "acquired centric," or "maximum intercuspation regardless of condylar position." It now means "maximum intercuspation with they condyles in centric relation." The term "centric relation" refers only to condylar position. What used to be called "centric occlusion" is now called "maximum intercuspation" (regardless of condylar position). Whatever the well-intentioned reasons for changing this terminology, it has created a great deal of confusion. It was difficult enough before to describe occlusion relative to condylar position, and vice versa; now it is a nightmare. The terminology in the paper is "old school," and as technically incorrect as that may be, it is clear enough to those who have a background in occlusion.

Centric relation as defined by Stuart is the rearmost, uppermost, and midmost position of the mandible at the most closed vertical dimension. This is not a reference to condylar position. Williamson defined centric relation as the condyle seated in the center of the disks at the posterior superior slope of the eminentiae and centered in the transverse plane. This is not inconsistent with Stuart's definition, it is just that Stuart did not have the imaging capability to look at the condyles.

In regard to quotes from the 1970s and '80s, Dr. Ron Roth never "admitted to error" in the use of Stuart's original definition. In fact, in 1975 at the IOK Congress in Munich, Germany, he presented a paper published in the IOK journal defining centric relation position as a superior position of the condyles. This really was distressing to his discussor, who had come to shoot down "retruded position."

Perhaps it would be advisable to forget the quibbling over semantics of who said what, when. Let us look instead at the literature and its value. How many studies in the literature support condylar position and CR and how many have shown it to be unnecessary? Several support the need for CR position of the condyle, and not one bases the claim that it is unnecessary on a hard "scientific basis." In fact, there are hardly any studies that properly examine the occlusion. Most studies use samples that are way too small, rely on chin point manipulation rather than articulator mountings, and almost none deprogram the subjects first with a CR splint to find the centric relation position of the condyles. It has been shown that x-rays are not accurate enough or fine enough to define a three dimensional relationship in a two dimensional medium. At best, the literature is inadequate and if the game is to search the literature to support your own viewpoint, it can be done.

Until you have clinically mastered the procedures relative to studying the occlusion and mandibular movement and studied the occlusion relative to condylar position, you have no right to say that condylar position doesn't matter. McNeil, at the University of California San Francisco, spent several years assembling experts in the basic and clinical sciences regarding all aspects of the masticatory system and his work has resulted in two texts that deal with the subject of occlusion and treatment goals for all of dentistry. Dr. Rinchuse seems to think that orthodontics is exempt because he chooses to interpret the literature with his bias and without looking at the clinical disciplines involved in studying the occlusion. In his closing remarks he suggests the paper presents a potential danger for those not trained in research. I think his attitude is a potential danger for those yet to be trained in clinical treatment protocols. It may be comfortable for orthodontists to think they need not worry about occlusion and condylar position, but it won't be very comfortable for the patients!

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Repelling magnets versus super elastic nickel-titanium coils

I would like to comment on the article. "Repelling magnets versus super elastic nickel-titanium coils in simultaneous distal movement of maxillary first and second molars," by Bondemark, Kurol and Bernhold (Angle Orthod 1994;64(3):189-198.)

Sm_2Co_{17} Modular Magnetics magnets have been used to treat thousands of cases since 1984 and have demonstrated the following:

a. Molars can be moved 5 to 6 mm within 3 to 6 months.

b. These movement rates are achieved with reactivations every 3 to 5 weeks.

c. To my knowledge, there have been no reported complaints of discomfort. In fact, the mobility and discomfort of the moving molars seemed to be noticeably decreased compared to conventional forces.

d. Over a 6-month treatment period, the distal movement demonstrated mostly bodily movement.

The authors repeatedly refer to the Modular Magnetics magnet as $SmCo_5$. However, the correct alloy is Sm_2CO_{17}. There are substantial differences in force characteristics and flux density bioeffect dosimetry between the two. Because of this, additional magnetic physical properties should have been provided in the article, (e.g. energy product, Br, Hc, coating, etc.), to enable replication of this test.

Some of the reasons for contradictory magnetic bioeffects reported in the literature are: incorrect or inadequate reports of physical properties, incorrect target flux density dosimetry, and failure to simulate clinical conditions where force (perturbing the local equilibrium) and magnetic field bioeffect coexist. These factors make replication and comparison of results impossible.

Alternative side changes for magnets and coils may not be a statistically approved method for random distribution in the small sample used in this study. It is not unusual, under conventional conditions, for unequal amounts of bilateral movement to occur when similar bilateral forces are used. Therefore, a more random distribution method is usually used.

The authors did not follow the manufacturer's directions for magnet force application. The use of the palatal tubes on the maxillary molars with a guide wire from the palatal anchorage is counterproductive because it introduces excessive friction that decreases the rate of movement. One of the advantages of the magnet design is the unguided sectional arch that permits the molars to move with the least resistance through the bony trabecular trough with minimum encounter of the buccal and palatal plates.

The authors used this palatally guided mechanism to control the alleged excessive molar distal tipping that they had experienced earlier. Here again, the manufacturer's instructions were apparently ignored. Instead of a vertical tube deep in the distobuccal embrasure of the second premolar, an eyelet was used. Activating the magnets with a ligature running through an eyelet that was possibly positioned too far occlusally could account for distal tipping. However, a vertical tube would have forced the superior ligature strand gingivally so that the free mesial end of the sectional wire would be raised up when the magnets were activated by tightening the ligature. This maneuver, with the correct force vector, would upright the molar roots while the 5 mm magnet pole faces, by creating an equidistant air gap the entire length of the repelling pole faces, would force more bodily movement.

The friction introduced by the palatal guiding mechanism dissipated the distalizing force too rapidly, giving rise to the authors' suggestion that frequent magnetic reactivations were required.

According to the manufacturer's instructions, the distal connection to the molar tubes require rigidity to aid bodily movement and control rotation. This subject was not discussed.

There appears to be an unusual overemphasis on magnetics that makes comparison with the coil springs difficult. For example, there is no mention of the amount of tipping that occurred with the coil spring prior to this test, although earlier tipping with magnets seems to justify the unnecessary palatal guidance.

Not only were the magnets the wrong alloy, but the given dimensions of the sectional archwire that carried the magnets seem to differ from those provided by the manufacturer. Furthermore, although I appreciate the authors' desire to match equal forces, I wonder whether the dimensions of the coil spring and the ribbon arch on which it was mounted are the usual configuration available commercially for molar distalization? If not, the credibility of the comparison can be challenged. The coils are either experimental or a new design, while the magnet design is obsolete.

The inside of the Nance acrylic should never be polished because it will weaken the anchorage. There is no resulting adverse effect on the palatal mucosa.

Three to 4 mm bite opening in one step is excessive. Stretched buccal mucosa could account for more discomfort on the magnet side. Most magnet molar distalizations in the past were performed without posterior disclusion.

There is no table or documentation to show pocket depth before and after treatment or to show tooth mobility. There was no elaboration of discomfort.

Most definitions of ideal orthodontic force stipulate that the desired movement be accomplished within a reasonable time with the least force. The advantages of lower forces are: decreased local pathology, less anchorage loss, and a decreased rate of periodontal aging. The magnets approximately obey the inverse square law, and the authors have shown that force decays rapidly to 100 grams and less with distance, although distal movement continues. If the authors had refrained from reactivating every 4 weeks they might have been pleasantly surprised to observe continued movement even though the force magnitude was considerably lower than that stated as necessary by noted authorities in the article. Of course, removal of the palatal guidance would have been required.

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Author's response

Dr. Blechman states that "magnets have been used to treat thousands of cases since 1984... ." However, only a few cases have been reported in the literature, and some of these are anectodal reports. We do agree with Dr. Blechman's claims that magnets can move molars 4 to 5 mm in 6 months or less with no adverse clinical effects or discomfort from the magnets. (See our article: Distalization of maxillary first and second molars simultaneously with repelling magnets. Eur J Orthod 1992; 14:264-272.) However, some side effects, such as molar tipping and rotation, have been observed in treated cases.

Because the advantages of magnetic forces have been questioned, we undertook our recent study to compare two force systems—repelling magnets and superelastic nickel-titanium coils—in a standardized, impartial way. We used a classical model of accuracy, namely an intraindividual study with a well-controlled left- versus right-side experimental design, paid maximum attention to detail, and collected a sample that was large enough to allow solid statistical analysis. We do not understand Dr. Blechman's difficulties with the test design.

We would like to stress that we have no commercial interest or other connection with the manufacturer and do not feel bound to the manufacturer's single suggestion on how to design the appliance. In fact, we proposed a new appliance with palatal tubes on the molars and a guide wire from the palatal anchorage to avoid the side effects of molar tipping and rotation. This appliance has a frontal acrylic bite plane which allows the molars to move distally, independent of lateral occlusal forces. Naturally, the palatal tube and guide wire extension arrangement results in somewhat higher friction, but we kept friction on the magnet and coil sides equal, and, in fact, the molars moved easily and without tipping or rotations with either magnets or coils.

The difference in anchorage loss between molars moved with the guiding system and molars moved without it was negligible (1.9 mm versus 1.8 mm). Moreover, the somewhat higher friction is, to us, an acceptable tradeoff to tipping and rotation; it seems more appropriate to move the molars bodily than to move them with a tipping component. The latter type of movement often requires another 4 to 6 months of rather difficult orthodontic uprighting.

We agree with Dr. Blechman that incorrect or inadequate reports of physical properties, incorrect forces, and failure to simulate clinical conditions are often reported in the literature. Therefore, we carefully matched the magnet and coil forces in the clinical situation before and during the experiment.

The main reason that nickel-titanium coils were more effective than magnets in moving molars distally is the fact that coil forces decline more slowly than magnetic forces. Magnetic forces obey the well-known Coulomb's force law ($F=1/d^2$), which means that the magnets exhibit their maximal force potential at short distances between the magnets, while nickel-titanium coils demonstrate a large range of superelastic activity with a small fluctuation of load in spite of a large deflection due to excellent springback and superelastic properties. In orthodontics, high forces are considered potentially harmful because stress can result in root resorption, soft tissue dehiscences, or loss of supporting bone. We saw no harmful effects in any of the treated cases. However, the long-term result of the tissue reorganization and tissue adaptation of both soft tissue and bone will be evaluated in a long-term follow-up study.

Dr. Blechman discusses several possible errors or mistakes in the experimental design that may call into question our more favorable report for the nickel-titanium coils. For example, he claims that "The inside of the Nance acrylic should never be polished because it will weaken the anchorage." We question whether this is a sound scientific claim. What disaster will happen with polishing the acrylic surface a couple of tenths of a millimeter? With an intra-individual experimental design with one magnet side and one coil side, it should not be difficult to determine that movement on both sides was equally related to the anchorage acrylic Nance button.

Finally, we believe it is important to evaluate orthodontic equipment and force systems clinically and biologically, in an unbiased manner and with a high degree of objectivity. This scrupulous evaluation is necessary because of increasing concern for safety by the profession and recognition that specifications and certification programs by the manufacturers often emphasize physical and mechanical properties rather than biological suitability. We think our study is a step in that direction.

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