A TOO-POLITE SILENCE ABOUT SHODDY SCIENCE: DYNAMIC STRENGTH TESTING AND BEYOND

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The title of this paper is from an article by John Maddox, Editor of *Nature* Magazine which appeared in *The New York Times* on September 26, 1988. In the article he stated that most people, especially health professionals, applaud first rate science, the type that sometimes results in Nobel prizes. However, there is such a thing as second rate science, but who will tell you what it is or where to find it? One certainly would not expect "second rate" science in a recognized scientific journal that has highly respected scientists as reviewers, who are supposed to be more familiar with the material presented or assessing scientific method than professionals who may be reading the article later on.

Maddox found that the science journal *Nature* stumbled into a controversy that left him with the impression that a substantial part of the scientific community would prefer to avoid these questions. Publicly, the convention seems to be that all science is excellent but some especially so.

The previous June, *Nature* published an article by a French physician whose research results contradicted all established principles of physical science. In the course of two years, four of the prepublication reviewers could see nothing wrong with the physician's account of his experiments. The editor insisted on an on-site review. What was observed was what the prepublication reviewers could not detect; a series of elementary procedural errors that, taken together, revealed that the claims of reproducibility were false.

Why, asked the complainants, was print space wasted and credibility given to second rate science, when there is first rate science to be published? Why did they not investigate first and publish only if the research proved valid? The answer to both of these questions is not all reviewers can identify second rate science. What is even more disturbing is that many reviewers have preconceived ideas about the subject material. These reviewers do not have sufficient clinical experience to evaluate the research according to the "gold standard" that they demand of everyone else. As a consequence, they base a good portion of their agreement on what has already been published. Since many of them do not actively treat patients with the problems described in the articles they are sent to review, they base their judgment mainly on summary knowledge of what has been published previously.

The Editor's Forum is offered to members of the editorial board to express their opinion or knowledge on a chosen subject. It is subject to space availability and is not a refereed article.

This point was brought home most aptly by an article[1] authored by two Finnish researchers last year. They were discussing a new text on occlusion[2] that stated in the preface "it is our contention that the subject of occlusion does have a biological basis, and that application of the scientific method can and should be applied to its study." This statement, they say, prompts a serious question: What then is "the" scientific method to be applied?

Kirveskari,[3] who previously reviewed the book stated that at the end of the book, "the reader learns that it is a review of intelligently interpreted research in the field of occlusion." In addition, he states, "the research referred to is to a large extent open to interpretations other than those presented by the authors."

According to Alanen and Kirveskari[1] many eminent scientists feel that the scientific method cannot be unequivocally identified and described. In practice, scientists try to recognize what is "not" scientific instead of trying to define what is scientific. They attempt to point out errors in study design, analysis, or conclusions. The actual science is where there is disagreement.

Alanen and Kirveskari end their article by stating that "a thorough analysis of the advantages and disadvantages, cost and benefits of different treatment modalities is required before rational criteria for treatment strategies can be given."

Kirveskari[3] further observed that one cannot escape the notion that the authors wish to discourage the widespread practice of occlusal therapy and recommend instead increased use of pharmaceutical and behavior treatment methods. He feels the role of occlusion is so central in clinical practice that much more evidence against it is needed before this change is indicated.

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Depending on one's point of view or one's bias, it is easy to concur with these clinical researchers, or just say their remarks are simply "sour grapes" and anecdotal.

In a recent editorial, another respected orthodontic researcher[4] was puzzled by the numerous epidemiological studies that indicated little or no correlation between occlusal factors, pain and dysfunction. He went on to say that it is now widely acknowledged that most epidemiological studies on temporomandibular dysfunction (TMD) lump all TMD cases together. He suggests that future epidemiologic studies should attempt to screen all those whose symptomatology may be either traumatic in origin (from the history) or those whose symptomatology is psychological (by use of appropriate psychologic instrument). Excluding these patients from a total sample offers the potential for discovering occlusal cause-effect relationships that clinicians have suspected for years. He concluded by stating that the door on occlusal etiology is still ajar.

In line with the aforementioned statements, I believe the time has come to call attention to two studies that appeared in *JADA* in March 1984.[5,6] These two articles, carried out at two universities in the same state are obviously flawed. They have been refuted by other studies done later that more closely conform to scientific standards. Yet, the later studies are rarely mentioned at scientific meetings. Only the two obviously unscientific ones are recognized when the subject matter is discussed. Is there a "double (gold) standard" because it serves a particular group's purpose? The double "gold" standard denies the profession the use of kinesiological science which is most useful in everyday practice.

Many dentists who treat patients with craniomandibular disorders as well as athletes, have reported increased strength and performance in their subjects as a result of changing their maxillomandibular relationships.[7,8] This concept has caused much controversy between various scientific and clinical investigators.[9,10]

The criticism directed at the group having found positive changes is: (1) adequate controls were not used in their research design (such as double-blind experiments), (2) a lack of proper statistical analysis, and (3) a lack of knowledge of strength testing.

The research that indicates that the maxillary or mandibular orthopedic repositioning appliance (MORA) is ineffective for dynamic strength increase has been criticized by clinicians because (1) the research did not allow adequate time for the MORA to work, (2) the MORA will only work on subjects with TMD or occlusal problems, and (3) it is not known whether the appliance has placed the mandible in its optimal physiological relationship.[11,12] Although many of the earlier studies[13,14] were definitely flawed from a strictly scientific view, from a strictly clinical standpoint, positive changes were noted, changes that deserved further investigation.

I have chosen to write this editorial because the inference has been made in some studies that the model analysis as described by Lieb[15] was performed on each maxillary and mandibular study cast, and then the casts were mounted on a Galleti articulator and each was adjusted to achieve the desired three dimensional maxillomandibular relationship for each subject. Then a processed mandibular repositioning appliance as described by Gelb was fabricated.

The first study that made this inference appeared in the literature in 1981.[12] In this study 14 basketball players, none of whom had clinical or historic evidence of TMD, myofascial pain dysfunction syndrome (MPDS), or posterior bite collapse, were tested in a randomly assigned order with an experimental bite opening appliance, a placebo appliance that did not alter the mandibular position or vertical dimension, and no appliance. Results clearly demonstrated no change in strength among the three groups. Interestingly, of the 14 subjects, nine were class I, three were class II subdivision 1, and two were class II subdivision 2. It was stated that the results indicated that opening the bite will not increase upper body strength of the normal subject. If we assume the study was done as stipulated, I would also agree that opening the bite or just increasing the vertical dimension of a normal subject will not increase upper body strength, but it bears no relevance to establishing the three-dimensional maxillomandibular relationship for each subject. This procedure reveals a total lack of understanding of what is being performed not only on Gelb's TMD patients but also on athletes.

Two previously mentioned studies[5,6] done at well known universities showed no comprehension whatsoever of the procedures we advocate in fabricating appliances for our patients or athletic subjects. Each of the two studies used treatment splints that were fabricated using the design recommended by Gelb, but neither of the two studies followed the protocol as described in the previous article.[12] In one of these studies,[5] the vertical dimension between the incisors was increased by a constant 2 to 3 mm for all subjects. It was assumed that if the opponents[16–18] of the Greenberg study hypothesize that the failure to show an improved performance was caused by an improper

vertical relation, a decreased physical ability should have been found in some of their subjects. None of their subjects gained or lost muscle strength through the use of the MORA. I agreed with Greenberg's findings, but not for the same reasons they claimed. I will remark on this study again after reviewing the other study that appeared in that same issue.

In that study,[6] it was stated that the treatment splints were fabricated using the same Gelb-type design. The occlusion portion of the splint occupied the subject's free way space and was adjusted to provide even contact in centric occlusion. Forty-two percent of the subjects (20) had clicking in the TMJ, but none of the subjects had palpation tenderness of the muscles of mastication or the TMJ. In addition, all subjects underwent a chiropractic-applied kinesiologic evaluation to test isometric muscle strength. Chiropractic examination showed that nine subjects would benefit from wearing a MORA. Five of the subjects received treatment splints and four wore placebo splints. This procedure is certainly gratuitous and questionable, since the bite positioners were not adjusted by kinesiologic guidance or by the chiropractors' suggestions.

Verban, summed up much of what was scientifically wrong with these two studies in a letter to the editor in *JADA* in July 1984. He states, "I would like to express my opinion concerning the issue of whether a MORA can increase strength. It is not the MORA, but rather the position obtained with the MORA that is important. This position is not universal and must be determined for each individual. Under this hypothesis, a study in which each MORA is constructed exactly the same could not prove a benefit of statistical significance." Yates, in the June 1984 JADA, responding to another letter to the editor shows his lack of understanding by stating "I should like to remind him that our testing procedures come from the claims of MORA supporters[19] who show pictures in their publications indicating that strength is increased simply by inserting the MORA." This could not be further from the truth.

The effect of a MORA on shoulder strength was also examined by Verban.[20] Twenty college students volunteered for the study. Three bite conditions were tested for each subject: centric occlusion, centric occlusion with a placebo splint, and the position obtained with the MORA. These were the same conditions employed in the other studies previously mentioned. The placebo splint did not alter centric or vertical occlusion, while the MORA repositioned the mandible in three dimensions as described by Gelb.[21] The data for each subject was taken as he or she was seated in a stabilized chair and the data was collected using a Cybex II dynamometer. Measurements were recorded for each of six shoulder movements; abduction, adduction, flexion, extension, external rotation, and internal rotation.

Statistically significant results were obtained among the bite conditions for shoulder extension peak torque; shoulder extension, average torque; and external rotation, average torque. Tukey's HSD post-hoc tests indicated that the MORA bite condition yielded significantly higher strength scores than did the normal bite condition on each of these shoulder movement measurements. No significant differences were observed between the placebo and the normal bite condition.

A study[22] using 23 varsity athletes compared mandibular position with appendage muscle strength. The study compared upper and lower extremity muscle strength of three different jaw positions: (1) centric occlusion; (2) supported rest position, and (3) a 5-mm extended vertical dimension position. The interocclusal registration simulated the method prescribed by Gelb[21] and Smith.[7] All four appendages were tested: the arms in abduction and adduction movements, and the legs in extension and flexion movements around the knee. Results indicated that mandibular position affects appendage muscle strength and may be important to total body well-being. However, there was considerable individual variability of optimum muscle strength both by muscle groups and mandibular positions. The combined arm abduction and adduction scores did show the strength in the supported rest position to be significantly greater than in centric occlusion. The lower extremities did not show a statistically significant difference between the mandibular positions.

Still another study[23] tested MORAs with 11 weight lifters and football players to determine change in muscle strength and efficiency (power) during various exertions. An average increase of 5% in vertical jump was noted for all 11 subjects. All of the subjects showed an increase in the grip test with a mean of 17.3%. However, there was no significant increase in strength recorded for the maximum hip sled or the bench press test.

A controlled double blind field study[24] was conducted to see the effects of the MORA on football players at an eastern college. Players were randomly divided into two groups, one wearing the MORA and the other wearing the conventional mouthpiece. The MORA repositions the mandible generally to an anterior position, increases the vertical dimension, and

changes the head-posture relationship. They were tested to see the effects of the MORA on performance, number, type, and severity of injuries, three measures of physical fitness that included strength, jumping, ability, and balance and agility, as well as satisfaction with the mouthpieces. Overall results were positive and in favor of the MORA. Significant findings favored the MORA in severity of injuries, knee injuries, strength, and satisfaction. No significant findings favored the conventional mouthpiece.

Starting in 1977 with a clinical report by Stenger,[25] it was indicated for the first time that a lack of posterior bite support and malocclusion were factors concerned with limited athletic performance. As a result of this report various investigations were performed that produced conflicting results and unopposed criticisms of the findings supporting Stenger's proposal. Forgione et al.[26,27] surveyed 20 experimental and clinical studies and two review commentaries. Their study attempted to organize what has been found to date, identify the inappropriate use of terms, point out questionable statistical practices, question the conclusions of faulty experimental designs and scrutinize the unfounded generalizations that have resulted. They state that if Stenger's original proposal is proved to be correct, it will have implications, not only for athletic performance but for the more central role of occlusion in health and behavior.

In their review they found that a commentator, reviewer, and the author of three of the studies made emphatic general statements critical of the original results and later studies that supported Stenger's original proposed relationship in spite of the following: "(1) most of these experiments used subjects with no apparent malocclusion or lack of posterior support and others, mixed occlusions; (2) most researchers set bite appliances by techniques other than kinesiological guidance, a functional technique, assuming or implying that all MORA's are equivalent; (3) researchers used data showing no increase in isokinetic tests of strength to criticize studies of isometric strength while commenting upon "strength" unqualifiedly; (4) some researchers employed either questionable statistics, experimental design, or both; and (5) some authors and a commentator have invoked placebo as a criticism of evidence which supported Stenger's proposal even though the placebo effect has not been demonstrated in any of the studies which employed a placebo control condition. The belief that the placebo effect is omnipresent has even fostered an explanation for its lack of appearance." In addition, they mention the possible role of body test position and its consequent influence on the bite in affecting results.

This manuscript deals not only with a review of the current literature on variation in strength of extraoral muscles as a function of bite relationship, but also includes reversal design study using a K-MORA (kinesiologically determined using the isometric strength of the deltoid muscle), a deflection appliance, and a placebo appliance.[26,27]

In their research study of the effect on isometric strength of biting on three different intraoral devices and habitual occlusion, it was concluded that a relationship does exist between bite and isometric strength. They also found that the previous speculation about the role of the placebo effect was not substantiated by the data gathered in their experiment.

In an article in *The New York Times* (April 24, 1990) entitled "Cultures in Conflict: M.D.'s and Ph.D.'s Battle over Research Tactics", Paul J. Friedman, a radiologist and Dean for Academic Affairs at the School of Medicine of the University of California at San Diego stated, "Doctors are the engineers of biomedical science. They're practical and goal-oriented, and they want results that will help their patients."

"The physician-researcher focuses on finding treatments for individual patients, and the scientist focuses on the abstract mechanisms of an abstract disease process," said Dorothy Nelkin, a sociologist of science at New York University, "That can make scientists seem awfully cold and ivory-towerish sometimes."

The clash existing between doctor and scientist is becoming ever more visible as the differences between applied and basic biological research narrow, and the time lag between a fundamental discovery and its clinical application shrinks. In a number of cases, the opposite is true. Clinical application of value to the patient may outstrip scientific verification because of differences in training. Scientists spend five to 10 years of postgraduate training learning to conduct research, whereas physicians devote the bulk of their training to patient care. This is also true of dentists.

The time has come for dental clinicians and scientific researchers to develop a greater degree of respect for one another, so that we can ultimately deliver better care to our patients sooner and with less suffering.

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