A RESPONSE TO THE SPEIDEL-STARR ARTICLE

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I will respond to the authors' points in the order in which they appear in their article.

1. The meaningfulness of this confrontation between opposing views on the role of the speech pathologist in the treatment of patients with tongue thrust has been diminished by the decision of Speidel and Starr to limit their discussion to open bite malocclusions. In my private practice, which is probably typical in terms of types of malocclusion represented, patients having open bite as their primary malocclusion only comprise 8.6% of my caseload, whereas 86.4% have overjet as the primary problem. A survey of 1000 of Barrett’s patients (9) revealed a similar distribution: 8.9% had an open bite, and 73.8% an overjet. Speidel and Starr ignore the most commonly treated condition and confine their remarks to a troublesome but relatively infrequent dental malocclusion seen by oral myologists.

2. In their review of the literature, the authors refer to conclusions of Fletcher (2) and Weinberg (13), which state that no identifiable “pattern” or “syndrome” has been found. Efforts to label tongue thrust as a syndrome have been abandoned for many years by most researchers and clinicians, in the face of evidence that associated characteristics, such as degree of masseter contraction and hypercontraction of circumoral muscles, are inconsistently found in tongue thrusters. There is no consistent pattern of symptoms, only a single, consistently identifiable behavior, that of resting or pushing the tongue against the anterior teeth to an abnormal degree.

3. The authors stress the need for cause and effect research. I agree with this need, but I see in the authors’ paper what I see in so many similar articles—a dual standard regarding that need. They repeatedly cite investigators’ and writers’ conclusions regarding the spontaneous effect of the disappearance of tongue thrust due to the (cause) normalization of oral structures, but none of the sources they cite in fact studied cause and effect. When Mason and Proffit (6) state that correction of the malocclusion wld usually result in a disappearance of the tongue thrust swallowing pattern without any particular therapy directed at the tongue thrust, they are merely expressing an opinion. The opposite opinion has been expressed by at least 20 orthodontic writers in the literature. The same criticism has to be applied to the Sassouni and Nanda study (10) cited by Speidel and Starr. The skeletal abnormalities found in the patients studied in no way justify the authors’ conclusion that local interference was not a primary factor in open bite. That is only an assumption.

4. I cannot understand why these writers seem willing to accept the hypothesis that thumb sucking can maintain an open bite, but are unwilling to believe that tongue thrusting may do so. The tongue is larger than the thumb; it is infinitely more muscular; and its resting postures are much more consistent than is the posturing of the thumb inside the mouth in most thumb suckers. Most children who suck their thumbs do it at night and during selected times and activities during the day. Writers who doubt the influence of the tongue on the teeth often defend their point of view with the statement that teeth are moved effectively with light, constant pressures, and are affected very little by stronger, intermittent pressures. Interdental tongue resting postures persist constantly during the...
day and during the night in most patients. It is reasonable to assume that the tongue can be at least as effective in maintaining malocclusions as is the thumb.

5. It is consistent with the authors' inconsistency in their statements regarding the importance of basing conclusions and treatment rationale on research that they categorically affirm that "cribs" are the treatment of choice for thumbsucking. This assertion is neither supported by research nor by the vast number of clinically-oriented articles in the literature.

6. The authors allude to a spontaneous decrease in unusual tongue activity at age 10 or 11 years, following a high incidence of such activity at ages six to eight. They cite four sources (2, 5, 12, 15). Nearly all incidence studies have found that the nearly 100% incidence of tongue-thrusting in infants gradually diminishes through the age of 11. Even by the age of five years, approximately 50% of the children who were tongue-thrusting at birth have changed to a normal swallowing pattern (4). Speidel and Starr warn against treatment of tongue thrust while morphology is changing. Oral morphology continues to change into adulthood. The same argument might be proposed in favor of postponing orthodontic treatment until morphology changes cease.

7. At about the middle of their article, Speidel and Starr assign a secondary role to "local influences" and suggest that elimination of tongue pressures is not likely to result in complete spontaneous closing of the bite. That such pressures are secondary influences is conjecture. No one knows the extent of their effect on the dentition. Many prominent writers, including Moss (7) believe that skeletal tissue growth is secondary to functional influences.

All skeletal tissue growth is a secondary, compensatory and mechanically obligatory response to changes in the temporally prior demands of the specific functional matrices. ... it is now quite certain that, with the exception of only a very few and very rare diseases, there is no direct genetic control of skeletal tissue growth, either of cartilage or of bone. Indeed, the best available data strongly indicate that the direct effect of genes is exerted on surrounding tissues (functional matrices) only through these nonskeletal tissues, and hence indirectly to the skeletal tissues themselves. Nevertheless, I have repeatedly contended that the purpose of oral myofunctional therapy is not to correct malocclusions, and certainly not to affect overall facial growth patterns.

8. The authors state, "There is no evidence supporting the idea (that tongue habits may induce post-treatment changes)." I assume they mean no evidence from controlled research. One important reason there is no such evidence is that there has been no such research. A reader may mistakenly conclude the research has been done which has proven otherwise.

A similar statement is made later in the article: "... speech clinicians who attempt to alter functional patterns of deglutition are doing so in the face of evidence which is unsupportive of the validity of tongue thrust as a clinical entity." I challenge this statement. Close agreements among several incidence studies, done independently (2, 4, 14) support the validity of tongue thrust as a clinical entity. Studies by Case (1), Overstake (8), and Stansell (11) demonstrate objectively that therapy can be effective in eliminating tongue thrust. In the absence of controlled research to test the effectiveness of therapy in contributing to post-orthodontic stability of occlusion, and the presence of controlled research that shows that therapy does actually change the nature of tongue postures and movements, the wisest course would seem to be to encourage the research without discouraging the therapy.
Although idealists continue to demand that therapeutic involvement in any human behavioral disorder be preceded by research to test its appropriateness or effectiveness, history does not demonstrate that this has ever been the order of things. If speech pathology had waited for research to justify therapy, I contend the profession would have progressed very little over the past 50 years, and indeed may have died long ago.

9. I remained relatively calm as I read Speidel and Starr's paper until I read their completely unreasonable assertion (fourth paragraph from end of paper) that "... provision of tongue thrust therapy to permit correction of the open bite or to maintain stability of a corrected open bite is essentially accepting responsibility for the patient's orthodontic result." Patently absurd!

The speech clinician who receives a referral from an ear, nose, and throat doctor whose patient has or has had a vocal nodule, and subsequently provides therapy for that patient accepts no responsibility for any surgery either preceding or following the administration of voice therapy.

When a child is referred to you for a stuttering problem by an elementary school teacher because the stuttering is interfering with the child's progress in class, you do not assume any responsibility for the child's success in school by providing therapy for him.

Many such analogies could be drawn. In the two illustrations given, as well as in the case of the tongue thrusting patient referred by an orthodontist, the clinician hopes that what he does for the patient will help to facilitate or make permanent the work of others, who are treating or teaching the child, but he in no way assumes any responsibility for the treatment provided by others. We are punished for our own sins in this case.

10. The pedantic warning of the writers to speech clinicians (third paragraph from end) I find offensive. We have no need to be reminded of our ethical responsibilities by Speidel and Starr.

If they are implying that speech pathologists who treat tongue thrust are doing so due to their sacrificing ethical and scientific considerations to pressure from patients, they are grossly in error.

11. (Second to last paragraph). "As noted in the body of the paper, appropriate advice to the patient is dependent on your ability to appraise the patient's overall facial growth patterns, evaluate the dental developmental stage, appraise the contribution of local factors impeding the eruption of teeth, predict naturally occurring changes as the patient matures and estimate the importance of factors beyond the clinician's control.

Not at all! The speech pathologist or oral myologist need not be a dentist. He does not need any of the five abilities described by Speidel in order to give advice to the patient. What he does need to do is to restrict his advice to the aspect of treatment he is qualified to provide. For information concerned with dental development, facial growth, etc., he need merely refer the patient (or himself) to the appropriate dental specialist.

Those speech clinicians who feel more comfortable with the traditional scope of their profession have every right to avoid involvement in the admittedly controversial and peripheral area of oral habit disorders. Others, who after studying both sides of the issues, and considering their own interests and abilities and the needs of their community, decide to stray from the mainstream, should avail themselves of the specialized training they will need to prepare themselves to provide oral myofunctional therapy. I have found it to be a thoroughly enjoyable endeavor, and have received for 14 years the respect and appreciation of over 30 orthodontists who are convinced that my services have made their treatment easier and their results more consistently permanent.


