Commentary

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The Joint Committee's Statement on Myofunctional Therapy— Pros and Cons

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This paper was read at the ASHA Convention, Washington, D.C., November 1975, as part of a panel discussion on myofunctional therapy.

A little over a year ago the Joint Committee on Dentistry and Speech Pathology-Audiology issued a statement which has had a very serious effect on research and training in the field of oral myofunctional disorders. Although the statement encouraged research and discouraged therapy, the result of its publication in various speech and dental journals has been a reluctance of members of both professions to get involved in the field in any way.

The *Asha Journal* has recently been running a series of articles on the history of the American Speech and Hearing Association. I would like to quote from one of those articles:

One of the primary reasons for the existence of a professional organization is the establishment and maintenance of high standards for that profession. The founders of ASHA were strongly motivated toward this goal. They were acutely conscious of the damage to the reputation of speech pathology caused by the activities of untrained, unethical charlatans who claimed to be "speech correctionists." A sizable number of such individuals operated private clinics in the 1920s where, for exorbitant fees, they guaranteed to cure speech defects by the use of secret methods. There was no formal way in which these persons could be identified by unsuspecting clients. But if a strong Association could be established whose members were known to be ethical, trained professionals, this would be a means of separating them from the pretenders. Consequently, a chief concern of the founders was the formulation of the particular requirements to be set for membership.

I believe that this same condition exists today in the area of oral myofunctional disorders, and that this is the real reason for the statement from the Joint Committee. Too many poorly trained people have been doing an ineffective job in administering therapy. It is not that tongue thrust has been found to not exist, or that therapy to treat it has been proven unsuccessful.

It was because many of us doing therapy for tongue thrust had apprehensions concerning the poor quality of therapy being administered across the country that we met

four years ago in San Francisco to form an Association, the aims of which were the same as those which prompted the founders of the ASHA nearly fifty years ago. We have called it the International Association of Oral Myology. Already much work has been done by this Association toward establishing training standards, certification requirements, and therapy guidelines for oral myologists. As the present president of that organization, I have communicated with training institutions across the country to offer the services of our members in teaching courses and assisting in the training of oral myofunctional therapists. I have great faith in the future of this organization, and in the future of therapy for oral myofunctional disorders. I began doing therapy in the field 12 years ago, with a great deal of skepticism. The skepticism long ago faded away, as I saw dozens of patients whose former orthodontic treatment had ended in relapse, and who, after receiving therapy for tongue thrust, retained normal occlusion, and as I received feedback from orthodontists who consistently found their relapse rate drastically reduced since they began referring their patients for therapy.

Dr. George A. Kopp often used to quote to his students, "Honest men may hold differing opinions." I believe that members of the Joint Committee, before issuing their statement, reviewed an impressive amount of literature. I would like to state very emphatically that I have reviewed the same literature, and then some, and that what I see in the literature, coupled with my own research and experience in the field, leads me to conclusions that completely differ from those of the Committee. I believe strongly that tongue thrust exists, that it does affect dental occlusion, and that it is amenable to training.

I would like to review with you the Joint Committee statement, and give you very briefly my reactions to its contents.

The first part of the statement questions the validity of the diagnostic label of tongue thrust. Several independent incidence studies have been done. The agreement among three of them, by Werlich, Fletcher, and Hanson, as to the incidence of tongue thrust at various ages, and as to decline of incidence as age increases, seems to me to be evidence that several researchers are identifying the same problem using generally the same criteria.

Second, the statement says that the contention that myofunctional therapy produces significant consistent changes in oral form or function has not been documented adequately.

First, regarding changes in oral form: The purpose of such therapy is not to change oral form, but to (1) prevent malocclusions from becoming more severe; (2) keep the tongue out of the way during orthodontic treatment; (3) *assist* in the remediation of articulation problems; and (4) help to prevent orthodontic relapse. If improvements in occlusion occur during therapy, we are encouraged, but that is not the purpose of therapy.

With reference to changes in function: Several studies have shown tongue thrust therapy to be effective in changing function.

The third point made in the statement is that there is insufficient scientific evidence to permit differentiation between normal and abnormal patterns of deglutition, particularly as such patterns might relate to occlusion and/or speech.

With respect to differentiation between normal and abnormal swallowing, reliability coefficients of 0.90 and higher are consistently reported by trained observers on independent judgments of the same swallows.

Regarding tongue thrust and occlusion: Barrett's classification system relates type of swallow to type of malocclusion. Each type of swallow is clearly distinguishable from a normal swallow. Studies by Rix, Werlich, and Rogers all found strong relationships between type of occlusion and type of swallow.

Concerning speech: Several studies have found significant relationships between tongue thrust and speech defects, some regarding the coexistence of the two problems in given children, and others regarding improvements in one being accompanied by improvements in the other.

Another sentence in the statement reads as shown: There is unsatisfactory evidence to support the belief that any patterns of movements defined as tongue thrust by any criteria suggested to date should be considered abnormal, detrimental, or representative of a syndrome.

First, referring to the word, "abnormal." If normalcy is defined as that which is done by more than 50 per cent of the population, then, according to the incidence studies I referred to earlier, tongue thrust becomes abnormal after the age of five, and gets progressively more abnormal through adolescence.

Second, whether there are harmful effects of tongue thrust needs to be determined by research. There seems to be strong clinical evidence that tongue thrust contributes to relapses in corrected occlusion.

Third, tongue thrust is not a syndrome. It is a behavior, involving the resting or pushing of the tongue against the anterior teeth. This behavior may or may not be accompanied by facial grimaces, relaxed masseter muscle, etc.

The Committee members state: The few suitably controlled studies that have incorporated valid and reliable diagnostic criteria and appropriate quantitative assessments of therapy have demonstrated no effects on patterns of deglutition or oral structure.

I do not know to which studies they are referring, because I have never been able to find them. I do know that a number of studies, including those by Case and by Overstake, have demonstrated definite changes in swallowing patterns, measurable changes, as a result of therapy.

Finally, the statement says there is no acceptable evidence to support claims of significant, stable, long-term changes in the functional patterns of deglutition and/or significant consistent alterations in oral form.

Studies by Robson, Barrett, and Von Dedenroth, Toronto, and Christofferson have all shown stable long-term changes in swallowing patterns and in orthodontically corrected occlusion. In these studies, the subjects served as their own controls. This weakens the significance of the results, but does not negate them. All of the subjects were evaluated separately, before therapy by at least two persons, the referring orthodontist and the speech pathologist, and after therapy by two trained observers, and the agreement on judgments was extremely high.

In three of the pre-post therapy studies, assessments of occlusion and swallowing were made *five years* after the completion of therapy. You do not find this kind of study in the literature on stuttering, or voice problems, or aphasia, or on any other kind of speech or language disorders. In fact, you find very few studies on the effectiveness of therapy at all, and most of the ones that have been done compare one type of therapy with another, rather than therapy with no therapy. With the help of some graduate students, I searched through all the issues of *JSHD* and *JSHR*, since their beginnings, and came up with the following table (Table 1).

Notice the lack of long-term studies. I defined *long-term* as any study which re-examined the speech of the client three months or more following the completion of therapy. I may have missed a study or two in my review, and I did not include a review of other professional journals, but I would think that those journals would reveal a similar picture. The studies on tongue thrust do not come from the *ASHA* journals, of course, but from other journals.

Table 1. Research on the effectiveness of treatment for tongue thrust

Study	No. With Normal Control	No. With Own Control	No. With Control With Same Problem	No. Short Term Studies		No. Long Term Studies	
				Effective	Not Effective	Effective	Not Effective
Subtelny		X			X		
Case			X	X			
Barrett/Von Dedenroth		X				X	
Stansell			X			X	
Overstake	X					X	
Robson		X				X	
Toronto		X				X	
Christofferson		X				X	

Table 2. Research on the effectiveness of treatment for various disorders*

DISORDER	No. With Normal Control	No. With Own Control	No. With Control With Same Problem	No. Short Term Studies		No. Long Term Studies		Total
				Effective	Not Effective	Effective	Not Effective	
Articulation	3	15	6	22	2			24
Aphasia		5	3	5	3			8
Cerebral Palsy		1		1				1
Miscellaneous	1	2	2	.3	2			5
Stuttering	2	19	3	20	3	1		24
Voice		3		3				3
TONGUE THRUST	1	5	2	1	1	6		8

*This data was compiled by the University of Utah department of Speech Pathology and Audiology and encompasses all issues of JSHD, JSHR, and IJOM.

The comparatively large number of studies on the effectiveness of therapy for stutters and for articulation problems mainly represent comparisons between therapy approaches. Notice, also, how many in each of the two areas have subjects serving as their own controls.

There is no doubt but what we need more long-term, well-controlled research on tongue thrust. We need them at least as surely on all the disorders we treat as speech pathologists. But considering the relatively few number of years since tongue thrust was first widely recognized as a problem, the amount of research done on it has been comparatively great (Table 2).

I see the tongue thrust problem as a foster child. Speech pathologists, dentists, dental hygienists all know something of the problem from their training, but in order to understand the total problem, they need to get learning outside their area of training. In the past much of this extra training has come from commercialized courses. The early history of its treatment parallels that of stuttering. Those of you who studied Hahn's book on stuttering remember that the contributors included about equal numbers of M.D.s, psychologists, and speech pathologists. In the 1940s and 1950s there were still a number of commercial treatments for stuttering. In spite of a lack of good, substantial, well-controlled studies demonstrating that stutters who receive speech therapy overcome their problem more often or more quickly than do those who receive no therapy, significant advancements have been made in the treatment of stuttering. Most of the advancements have come from clinical experimentation, not from controlled research. I say that we ought to begin to admit that most of our testing is done clinically and informally, for all the disorders we treat, and stop deprecating what we learn from clinical experience. At the same time we need to do a better job of testing results experimentally, in tongue thrust, in stuttering, in articulation, and in all the other disorders we treat and we need to do a better job of training clinicians.

It has been apparent for me for some time that although nearly all the orthodontists in the Salt Lake area refer their patients who tongue thrust to me, they are being quite selective in doing so. Only a portion of those with tongue thrust, according to the incidence figures, are being referred. Cooperative research by Dr. William Proffitt, Dr. Ray Ball, and I was begun in August, when Dr. Ball examined the orthodontic records of some 40 patients I had seen for therapy, in order to determine what kinds and degrees of problems were being referred, and to investigate the relative effectiveness of orthodontics with these patients.

I am presently considering several hypotheses to test in future joint research. One of these concerns differences between total lingual labial, and buccal pressures exerted during rest, chewing, swallowing, speech and other activities by tongue thrusters and those pressures exerted by normal swallowers. I would hypothesize that light, constant, resting forces of the tongue, coupled with stronger, intermittent pressures during chewing, swallowing of food, and the moving posteriorly and swallowing of saliva are capable of moving teeth, especially when labial pressures are abnormally weak or inconsistent.

We are anticipating the cooperation of the University of Iowa School of Dentistry in conducting a longitudinal study involving three groups of patients: (1) those with malocclusions and tongue thrust who receive neither orthodontic nor swallowing therapy; (2) patients with tongue thrust and malocclusions who receive orthodontic treatment but no therapy for tongue thrust; and (3) those who receive both types of treatment.

Dr. Henry Jann and I are also doing background work to prepare for cooperative research having to do with the effectiveness of therapy for tongue thrust.

Now, one more brief comment about the joint statement: everyone I talk to who has some knowledge of the intent of those who formulated the statement contends that they did not want to discourage treatment of tongue thrust, but only to encourage research. I wish they had worded it differently. On the optimistic side, many oral myologists who were luke-warm in their convictions have now become aroused and are trying a little harder to keep good records in order to be able to document their success in therapy.

Those of you who believe in therapy for tongue thrust, as I do, I encourage to add your strength to the united effort of the membership of the IAOM by allying yourself with that organization. Let's take the ball that others have punted, or fumbled and run with it.

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