

Review Article

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Marvin L. Hanson

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Tongue Thrust: Another Look At The Evidence: Part II

Marvin L. Hanson, Ph.D.
Salt Lake City, Utah

Dr. Hanson is on the faculty at the University of Utah

The second of a two-part article. Part I listed 16 statements from a paper by Mason and Proffit (1974) on tongue thrust with which the present author was in essential agreement, and discussed five statements from the same paper with which he disagreed. This concluding portion deals with another nine points on which this author holds opinions differing from those of Mason and Proffit.

6. "... it still appears that function adapts to form much more than form adapts to function (Subtelny, 1970)" (p. 121)

If this belief were a proven fact, which it is not at the present time, at least it would describe the *relative* reciprocal effects of form and function. The authors do not contend that function *does not* affect form. It is my belief, and this is also unproven by well-controlled research, that the effects *are* reciprocal, and until such as time as oral habits are found to be of negligible importance in the etiology or maintenance of malocclusions, we ought not close our minds to the possibility.

The Subtelny conclusion referred to by Mason and Proffit is Subtelny's answer to two questions posed at the beginning of his 1970 article:

If there is an abnormal oral or dental environment and a concomitant abnormal pattern of muscle activity did the muscular structures create the abnormal environment or are the muscular structures adapting to the environment? If the muscular structures are adapting to the contiguous orofacial environment, what is the pattern of function relative to that particular oral environment? (p. 170)

Subtelny's answer:

... the indications are that functional movements of orofacial muscular structures adapt to the variables of the form of the oral environment.

This conclusion is based on data from four groups of ten subjects each. One group had normal occlusions; another, open bite; a third group, class II, Division I malocclusions; and a fourth, maxillary deficiency, including some cleft palate subjects. Ages of the subjects were not given. This is an excellent study, well-designed and carefully done, and is cited often in the literature. One of the important contributions of this research was the conclusion that one of the characteristics traditionally associated with tongue thrust, i.e.,

lack of molar occlusion during swallow, was found to be present in subjects with normal occlusion as well as in those with malocclusions. As long as the author describes the results of the research, his comments are valid and important. His inferences regarding cause and effect, however, are not substantiated by the research he reports. Nevertheless, the words "seemed to," "an impression is gained," and "indications are" are wisely used.

When variation in the swallowing procedure was to be seen in the different types of oral environment, it was observed in the area of particular interest to the orthodontist, the anterior region of the oral cavity. Here the patterns of movement seemed to adapt or vary in function according to the configuration of the anterior malocclusion. (p. 181)

Later in the same paragraph:

An impression is gained that the tongue tip may be adapting to a specific anterior oral environment to achieve a seal during swallowing. Furthermore, the tongue tip seemed to maintain a close functional relationship to the lips during most of the swallow.

Function also seemed to adapt to differences in "form" in other ways. In the maxillary deficiency cases, a "molar apart" swallow seemed prevalent, possibly in an effort to compensate for a lack of development in the vertical aspect of the maxilla. (p. 181)

The inferences are, in my opinion, logical, but not proven by this research. Neither does the author claim they are proven. My disagreement is not with his answer, but with the dichotomous nature of the question, "... did the muscular structures create the abnormal environment or are the muscular structures adapting to the environment?" Observations indicating the possibility of cause and effect in one direction do not in any way rule out the possibility of cause and effect in the other direction. There is nothing in his research that demonstrates in any way that tongue function does not alter arch form. Nor is there in any other research in print. The only way to effectively demonstrate the relative effects of form and function upon each other would be to conduct longitudinal research with at least three groups of subjects. Group I would have normal occlusion and no tongue thrust, and would receive no treatment of any kind. Group II would have a malocclusion, preferably Class II, Division I, along with a tongue thrust, and would receive orthodontic treatment but no therapy for tongue thrust. Group III would also have a Class II, Division I, and would receive therapy for tongue thrust. The groups would be followed from the age of eight through fifteen, to allow for all orthodontic treatment to be accomplished, and a retention period to occur.

Ideally, even more groups would be included, representing other types of malocclusions, and therapy involving dental cribs. The difficulties of conducting such research are apparent, but unless a carefully controlled study is done, the literature will continue to abound with inferences and suppositions.

7. In a section titled, "What anatomical and developmental factors predispose to tongue thrusting?", Mason and Proffit discuss relationships between growing patterns of tongue and mandible. The tongue is reported to approach maximum size at or near eight years of age, whereas some mandibular growth extends even into the twenties. "The clinical implication of the growth differential between the tongue and mandible is a natural tendency for the tongue to be positioned relatively high and forward in the oral cavity in the early years of growth." (p. 123)

I am not certain I disagree with this tendency of the tongue. In order to assess the relationships between the habitual resting place of the tongue and the development of the mandible, however, it is necessary to consider as well the development of the maxilla. Maxillary growth continues until the age of 18 (Bloomer, 1971). Tongue size in relation

to maxilla size also varies from the age of eight to 18. Even if there is more space available to the tongue in the upper arch than in the lower arch during pre-adolescence, why should this promote an *anterior* positioning of the tongue?

8. It becomes important, therefore, to evaluate possible predisposing factors which may require a child under 12 to carry the tongue anteriorly but which will allow spontaneous remission of the anterior tongue positioning with further growth and development. This is highlighted by the finding of a developmental factor which produced spontaneous correction of 80% of the simple open bites in a large sample of children. (p. 126)

With this sentence, Mason and Proffit terminate the section on anatomical and developmental structures. Since they give no reference here, it is assumed they are again referring to the Worms, Meskin, and Isaacson research on Navajo children, which has been described in Part I of this paper. The objections to the reporting of "simple" open bites only have already been explained. If this is the study referred to, it is also presumptuous to call differences in incidence of open bites at different ages in non-longitudinal research "spontaneous corrections."

The Hanson-Cohen research (1973) did relate several factors to the spontaneous modification of tongue thrust in the 178 children it followed over a five-year period.

When compared to children who were tongue thrusters during the initial examination but changed to a normal swallowing pattern at some time during the 5-year period (transitional thrusters), those children who retained a thrusting pattern at age 8 years 2 months (1) contracted the masseter muscle less during swallows; (2) contracted circumoral muscles more during swallows; (3) dentalized linguo-alveolar consonants more frequently; (4) had larger tonsils; (5) had higher, narrower palates; (6) had fewer allergies; (7) did more digit sucking; (8) did more mouth breathing; (9) had less buccal cross-bite; (10) had greater maxillary arch circumference; and (11) had less anteroposterior distance at the level of point A on head films. (p. 76)

It should be emphasized that none of the above criteria was consistently found to be related to the persistence of tongue thrust during *all* the years of the research. The reader is again cautioned not to generalize these data of Worms et. al., wherein the authors "infer" that there is "apparently" a spontaneous correction of open bite, to other types of malocclusion, much more prevalent among the caseloads of oral myofunctional therapists.

9. "The neural control of speech and nonspeech activities in the central nervous system appears to be different; hence, swallowing is probably not neurologically related to speech, or vice versa." (p. 126)

There are obvious differences between the neural control of speech and swallowing. The entire posterior phase of swallowing is reflexive rather than voluntary. The anterior phases, however, are under varying degrees of voluntary control and utilize many of the same muscles and bones as those used in speech. The muscles of mastication and those of expression, as well as the extrinsic and intrinsic tongue muscles are all involved in both types of activities and are subserved by the same branches of the same nerves in both functions.

10. When tongue thrusting is associated with lisping, the speech clinician should be encouraged to correct the speech problem in the elementary school years, using articulation therapy techniques. Such therapy can be initiated according to the regular considerations employed by speech clinicians, and without regard for whether the child will eventually require orthodontic treatment. (p. 126)

It is possible to correct a frontal lisp when tongue thrusting is present without attention to the tongue thrust. This is one manner of treating a total problem, by concentrating on one of its phases. Another approach, as described by Overstake (1970), is to correct the

tongue thrust first, then observe whether spontaneous correction of the lisp has taken place. Overstake found that after nine months of swallow therapy, without concomitant speech therapy, 24 of 28 children who had had interdental /s/ speech defects before swallow therapy developed and used normal /s/ sounds in unguarded conversational speech.

Still another approach is to attend to more than one, or to all aspects of a problem. We need more research before we are equipped to say which approach is best for which children. I would prefer that the writers say that the approach they advocate is *an* effective approach, and then to substantiate that claim with evidence from research. Furthermore, correction of the more noticeable /s/ defect may not have any effect on the more subtle dentalizations of other lingo-alveolar sounds, which may well be a part of a more recalcitrant total tongue thrust pattern.

11. Related to the above point, the authors continue:

Where the prepubertal child with speech errors also exhibits a tongue thrust swallow, it may be tempting to work on swallowing patterns concurrent with speech therapy. It has been our experience that this is usually unnecessary and contraindicated. Articulation therapy promotes repositioning of the tongue tip at rest and for the initiation of speaking and swallowing tasks in young children. Most of the time, the tongue thrust swallow will correct itself with additional maturity. (p. 126)

It is not possible to quarrel with their experience. The only argument that can be offered is that this author's experience differs from theirs. It is important, though, to observe that there is no research to substantiate either claim. Nor am I familiar with any textbooks or published approaches to articulation therapy that pay any attention to habitual tongue resting postures in ordinary therapy for articulation problems. It certainly is not a routine procedure.

12. In a section titled, "Tongue thrust with malocclusion but no speech problem," the authors give the choices available to the orthodontist. He can do the orthodontic work first and hope the tongue thrust will disappear in response to changes to form, or he can have a speech clinician change the tongue thrust before orthodontic work is initiated. They then state:

The first course now seems to have been demonstrated to be the better clinical orthodontic approach, however. Correction of the malocclusion usually will result in disappearance of the tongue thrust pattern without any particular therapy being directed at the tongue thrust. If any therapy aimed at altering the swallowing pattern is employed in these patients, it is better that it be done in conjunction with orthodontic treatment rather than preceding it. (Reitan, 1969; Graber, 1969; Begg and Kessling, 1971; Hamilton, personal communication, 1973). (p. 127)

Referring first to the first two sentences in the above reference: I have often wondered why orthodontists such as Straub became interested in tongue thrust in the first place, and why tongue thrust therapy preceding orthodontic work has gained such wide acceptance. I know of no survey which indicates whether most orthodontists in this country, or in the world, prefer to take one course or the other, but certainly the referral by the orthodontist of a patient with tongue thrust and malocclusions to an oral myologist or speech pathologist for treatment preceding orthodontic treatment is widespread. At any rate, again there is no well-controlled research that establishes the greater effectiveness of one course over the other.

Regarding the advisability of providing tongue thrust therapy concurrently with orthodontic treatment, the authors cite four writers. A careful reading of the first three authors reveals that none of the three alludes in any way to concurrent orthodontic and tongue thrust therapy, let alone recommends it. Perhaps the references were meant to pertain to an earlier sentence in the same paragraph. Most readers, nevertheless would naturally

assume the four references cited would support the contention that it is better to carry out the two types of treatment concurrently. Since I have not seen the personal correspondence of Hamilton, no statement is possible about its contents.

Reitan, the first author cited (1969), does state, "The interesting fact is that the position of the tongue may be altered as a result of tooth movement. In many cases, tongue function will no longer influence tooth position." (p. 147) But he continues on the same page,

Generally speaking, the anterior teeth react differently. In spite of a precise positioning of the anterior teeth by fixed appliances, and even after a retention period, these teeth may frequently tend to migrate toward their original position when adverse muscular pressure exists . . . As we know, relapse occurs particularly where there is strong tongue function and weak orbicularis oris muscle complex.

Graber, in his chapter on Diagnosis, simply states categorically, "Experienced orthodontists know that the establishment of normal morphology is often followed with normal speech, making speech therapy unnecessary." (p. 8) On the same page he continues, "It has been pointed out repeatedly in textbooks that whenever there is a struggle between muscle and bone, bone yields. The role of soft tissues is important." He makes no mention of any timing relationship between tongue thrust therapy and orthodontic treatment.

The third authors cited, Begg and Kessling (1971), say relatively little anywhere in their book about tongue thrust. They refer to it briefly as a factor to be considered in relapses:

The only type of Class II malocclusion in which we have observed buccal teeth to return to Class II relations since using the light wire technique is the type which is associated with tongue thrusting during swallowing. The thrusting of the tongue also causes the anterior teeth to be unstable in their positions after active treatment. In some cases of this type all of the upper teeth move farther anteriorly as age advances. (p. 653)

It would appear that at least three of the four references cited are inappropriate to the point being made by Mason and Proffit.

12. When tongue thrusting is related to airway problems, the tongue is expected to adapt with a forward gesture to initiate a swallow so that the bolus of food can be accommodated through the faucial isthmus (Moyers, 1958). This is a natural adaptation rather than an abnormal behavior for such a child. Therefore, swallowing exercises should be especially avoided in cases where faucial isthmus size is reduced. (p. 127)

I have two objections to this recommendation:

- (1) The airway problems, when present, constitute an abnormal, unwanted condition. The "therefore" indicates the authors assume that given an abnormal oral environment, with resulting adaptive tongue behavior, it is not advisable to retrain tongue function. Much of the work of speech pathologists involves teaching adaptive movements necessitated by abnormal structures. It is not inherently inadvisable to do this.

- (2) This is another example of the unwarranted use of dichotomies. The existence of a restricted airway involves varying degrees, not an "either-or" situation. If the isthmus size is slightly, or even moderately reduced, but the tongue is not abnormally large (and it usually is not), and there are no neurological or muscular deficiencies, and the oral cavity is of adequate width and height, retraining of the tongue presents no serious difficulties. If the tonsils and adenoids are extremely large and conditions are such that their removal is inadvisable, and there is a strong mouth-breathing habit, the prognosis for therapy is, in my opinion, poor. Nevertheless, in my own experience most adverse airway problems can be remedied, either medically or by habit-training.

13. Also pertaining to tonsils and adenoids: "If the tonsils and adenoids are removed for medical reasons, we suggest giving the child time to adapt to a normal adult swallow up through the puberty range before recommending swallowing therapy." This is a reasonable statement, if the reader agrees with the authors that orthodontic work should accompany or precede therapy for tongue thrust. "Through the puberty range" could represent a long period of time, nevertheless, in which the habit of tongue thrusting might gain strength. If the T & A were done at eight years of age, or earlier, and the child's tongue thrusting persisted through puberty, there might be as much as a nine-year period of time during which the tongue thrust continued, in spite of an often essentially normal-sized oral cavity. If it is true, and it has not been demonstrated not to be so, that therapy for tongue thrust facilitates effectiveness of orthodontic treatment, and should often precede the latter, the waiting would be a serious mistake.

14. Mason and Proffit question the utility of swallowing therapy with elementary school age children. They list three references, Tulley (1961), Shriner (1966), and Speidel, Isaacson, and Worms (1972). They refer to the variability in swallowing patterns of young children and to the normal transition from tongue thrust to "adult" swallow patterns as age increases. They again call attention to the spontaneous remission of open bite, to which I have referred in numbers two and eight.

The Hanson-Cohen longitudinal research (1973) found a positive correlation between arch dimensions and persistence of tongue thrust in younger children. In general, the conclusion reached was that whether the tongue was crowded antero-posteriorly or laterally, the tongue thrust was more likely to persist through the mixed dentition stages. Instead of considering the patient with open bite, seen by the speech pathologist relatively infrequently, but referred to so often in the Mason-Proffit article, let us apply this finding to the most frequent problem seen for tongue thrust therapy, the child with an overjet. Consider, for example, a child of ten years of age. If he does not get therapy for tongue thrust, but his anterior maxillary teeth are moved or tipped posteriorly orthodontically, the antero-posterior space available to the tongue will be decreased. It seems defensible that this will facilitate the anterior linguo-dental contact of the tongue thrust rather than to cause it to yield to another pattern wherein this contact is not required. The same type of reasoning that leads to the conclusion that antero-posterior crowding by hyper-trophied tonsils promotes a tongue thrust because of limited space at the posterior portion of the oral cavity should apply when the anterior teeth are moved posteriorly, causing as much as a full centimeter of antero-posterior decrease in the maxillary arch.

Examining the writers referred to, we first find Tulley, well-known for his survey of 1500 eleven-year-olds in whom he found only 2.7% to have a tongue thrust and malocclusions. His incidences of these problems is so low and so at variance with all other incidence studies as to make his recommendations concerning the necessity of therapy for tongue thrust of very doubtful value.

The Shriner reference is a letter to the editor of JSJD, reviewing a study by Ronson (1965) in which 32 tongue thrusters were found among 60 children with lisps in the second, third, and fourth grades. Shriner cites the Ward, Malone, Jann, and Jann study (1961), which found that approximately 75% of the children enrolled in the early years of elementary school exhibited "abnormal swallowing." His conclusion, then, is that Ronson's incidence of tongue thrust among lispsers was lower than Ward et. al.'s incidence of tongue thrust in the total school population for the early grades. Hence, a "retraining program for visceral swallowing for the correction of lisping has not been established." (p. 207)

The reason for the relatively high incidence of tongue thrust found by Ward et al., is probably to be found in their definition of tongue thrust, found in another article referring to their same research:

... typical swallowing is characterized in the following way: During the mylohyoid state of swallowing, the posterior teeth are brought together into contact. The tongue remains within the dental perimeter of the oral cavity. The orbicularis oris and other circumoral muscles affect an effortless oral seal without assistance from the tongue. Typical swallowing is considered to be a reflection of more highly selective activity of the orofacial muscles. *Deviations from this pattern of swallowing are considered to be atypical or visceral* (underlining mine). (p. 425)

Research by Subtelny (1970), Hanson et al. (1970), and Long (1963) has agreed that the only consistent characteristic of tongue thrusting is the anterior positioning of the tongue. Many non-tongue thrusters, according to these researchers, purse or tighten the lips, fail to occlude the molars, over-contract the mentalis, or display many of the other characteristics once thought to be peculiar to the tongue thrust pattern.

Thus, two of the references cited here represent, or present as the basis for their rationale, incidence studies at either extreme, rather than several studies which are in essential agreement that approximately one-fourth of the children in the second, third, and fourth grades are tongue thrusters. For example, the Fletcher, Casteel, and Bradley study (1961) examined 1600 school children from 6-to-18-years of age, and found incidences of 52.3% at six-years and 38.5% at eight-years. Andersen (1963) studied 450 first, sixth, and twelfth-grade students and found incidences of 21.3% and 14.6% in the first and sixth grades, respectively. Werlich (1962) found an incidence of 37.3% among children with a mean age of 6.6-years, and 27.6% in children with a mean age of 11.5-years. Hanson and Cohen's longitudinal study followed 178 children from the mean ages of four years nine months, to eight years, two months. Incidences at six years, seven months, seven years, five months, and eight years, two months, respectively, were 51.7%, 35.4% and 35.0%.

In all of these studies a steady decline in incidence was manifested with increase in age. By interpolation it can be inferred that there is general agreement of an approximate 30 to 50% incidence among children in the second through fourth grades. According to these studies, the incidence of 75% used by Shriner as a basis for refuting the validity of Ronson's study is unrealistically high. His conclusions, therefore, that the rationale for retraining the swallow as a step toward the correction of lisping has not been established is arrived at based on evidence not in agreement with most survey results. For that matter, even had he established that few tongue thrusters are lispers, it would not have ruled out the merits of working on the whole tongue thrust pattern, including rest position, as a first step toward correcting a lisp.

Mason and Proffit's questioning concerning the utility of tongue thrust therapies with younger children is extended by Subtelny and Subtelny (1973) to include all children who receive therapy before orthodontic work is done. Subtelny has conducted two studies to determine the efficacy of attempts to modify swallow patterns. With Sakuda (1964) he took cineradiographic records of eight patients with tongue thrust before therapy, after six months of therapy, then two months post-treatment. The tongue was found to return to its original pattern of function. In this study, however, treatment consisted of the insertion of a palatal crib. Their conclusion was the retraining of tongue function is a very difficult thing to do.

The other study referred to by Subtelny involved only five subjects with "abnormal swallows." Not all of the five exhibited tongue thrust. Subtelny describes the patients as follows (1970):

Five children with various combinations of the characteristic features of abnormal orofacial muscular activity, such as tongue tip protrusive activities and/or, facial muscle activity during swallowing function, were selected for study. It should be emphasized that not all of the five subjects exhibited tongue protrusions, commonly called "tongue thrust." However, by clinical examination they showed different features, and combinations thereof, of abnormal swallows (p. 182)

He later describes one of the five as having "slight facial muscle activity and slight tongue protrusive activity." (p. 183)

The heterogeneous nature of the very small group of subjects makes the validity of this research questionable. These five children received "habit therapy" twice weekly for three months. Eight of the lessons were repeated in order that the program would be thorough and in order to prolong the program over a three-month period so that the subjects could again be exposed to radiation with safety. Comparisons were then made between before and after cineradiographs and other records. The comparisons revealed no significant changes as a result of therapy. The dentition had not been altered significantly. Subtelny concluded that therapy did not change the habit patterns, and that form was not appreciably altered by the therapy.

Aside from the small number of heterogeneous subjects (the inadvisability of calling a child a tongue thruster because he demonstrates abnormal circumoral activity has already been alluded to), two important aspects of the results should be viewed critically. First, although therapy lasted for three months, during four of the weeks the children received lessons they had already experienced. Motivationally this is an unsound procedure. Secondly, if therapy was unsuccessful, certainly no modifications in form would be expected.

Most of my colleagues make no claims about altering malocclusions with myotherapy. Their purpose is to assist in the prevention of relapse once the orthodontic work has been completed. An examination of the records of 201 of my patients with initial overjets found the following:

The overjets of 131 patients were decreased, without orthodontics, by a mean of 1.13 mm. Forty-four experienced no change in overjet, and overjets of 26 patients increased a mean of 0.65 mm. before orthodontics. Results on 19 patients with open bites were: Seventeen of the open bites reduced prior to orthodontics by a mean of 1.18 mm. Two increased by a mean of .065 mm. I am encouraged when a malocclusion is altered even slightly as therapy progresses, but I am not discouraged when it does not. Often the teeth are crowded and resist movement until two or four bicuspid are removed. Furthermore, I have no control subjects with which to compare my figures. Perhaps the same modifications in malocclusion would have occurred without therapy.

There are several studies which indicate that swallowing patterns can be modified. Short-term changes were measured by Case (1968) palatographically. Judges were able to consistently differentiate palatographs of 20 children with corrected tongue thrust patterns from those of 20 children with tongue thrust patterns.

Barrett and von Dedenroth (1967) reported success with 25 subjects in hypnotherapy for tongue thrust. Subjects were in trances from fifty minutes to one and one-half hours in from one to four sessions. One to three years post-treatment, all cases were reported to be maintaining normal swallowing patterns, with no undesirable side effects.

Stansell (1969) studied three groups of 18 subjects each, aged nine and one-half to 14 years, all of whom had tongue thrust, sigmatism, and overjet. Group I received swallowing training only; group II received only speech therapy for the lisp; and group III received no training of any kind. Before and after dental impressions and lateral head x-rays were

taken, and measures were repeated three months post-treatment. Speech training alone was found to result in a significant decrease in overjet, and tongue thrust therapy alone provided an increase in overjet. Several of the control group subjects showed an increase in overjet during the treatment time.

Overstake (1970) gave therapy for swallowing only to 28 of 48 children manifesting tongue thrust and lisps. The other 20 received therapy for tongue thrust and for the lisp. Both subgroups changed their swallowing patterns significantly in the direction of a normal pattern. After nine months of swallowing therapy only, 24 of the 28 children were using normal /s/ patterns in unguarded conversational speech. In the total group of 48 children, 39 (81%) were judged by orthodontists to have manifested positive changes toward more normal occlusion after swallowing therapy.

Three studies have dealt with the long-term effects of deglutition training. Robson (1963) saw 666 of Richard Barrett's patients, all of whom had completed tongue thrust therapy. Of the 666, 520 (78.1%) were found to be using non-thrusting swallow patterns. Of 266 who began therapy from the ages of seven through nine years, 193 (72.6%) were still swallowing correctly. Of the 252 who began from ages 10 through 12, 212 (80.9%) were still swallowing properly. All the patients had been referred by dentists, and most of them by orthodontists. In 99 cases (15%), malocclusion had improved sufficiently, in Robson's opinion (he is an orthodontist), that orthodontic treatment was not essential.

A later investigator of Barrett's success in therapy was Toronto (1970). Toronto located 50 subjects, ages 14 to 20, all of whom had completed tongue thrust therapy at least five years previously. Only two of the children were found to place the tongue interdentially during swallowing (success rate: 96%), and a total of 14 contacted the lingual surface of any of the anterior teeth during swallow (success rate: 72%). An examination of the records of these patients revealed that there was movement of the teeth toward normal occlusion without orthodontic treatment in 19 of the subjects. Seven of the 50 were able to avoid orthodontic treatment, and now had occlusion that was essentially normal.

Christofferson (1970) replicated Toronto's study with 25 of this writer's patients. Five years post-tongue thrust therapy, she found 92% of off-guard swallows. The mean age of these patients at the onset of therapy was 11.9 years.

Of the preceding studies, only Overstake's was properly controlled. The essential agreement, however, among all the studies relative to the results of tongue thrust therapy for children, and the permanency of those results, seems to outweigh by far the results reported by Subtelny of five children.

Summary and Recommendations:

Apologies are not necessary for an area of study which has more questions than answers. Such is the status of "oral myology," a recently coined name for the study of oral habits and their treatment. Only when students of the field are too eager to dichotomize, to form rigid conclusions, to over-generalize from research when research results are limited, do they need to fear making mistakes.

The present article has summarized areas of agreement and disagreement among various researchers and clinicians. As a basis for comparison, the Mason-Proffit article was referred to repeatedly. The reason for this approach was that most of the contentions of writers whose opinions agree with those of Mason and Proffit were stated clearly and logically by these two writers, and their views were supported by references to research most commonly cited in similar articles. The importance and validity of the several years' re-

search carried out by Proffit and his colleagues is recognized by this writer. The intent was not to attack the writing of one pair of writers, but rather to present another side to questions posed by various writers through an examination of the Mason-Proffit article.

The principle contentions of the present article are:

1. There is good evidence to indicate that nearly all children with open bites also have a tongue thrust.
2. There is a disproportionate amount of attention paid in the literature to open bite and tongue thrust, since patients with open bite constitute a relatively minor portion of the caseload of most therapists who work with tongue thrust.
3. Most open bites do not self-correct with maturation, nor with therapy, but only with orthodontic treatment.
4. It is important to determine, in children who have frontal lips, with or without accompanying tongue thrust swallow, whether dentalization of other lingo-alveolar sounds is present, and whether the tongue habitually rests against the anterior teeth.
5. There are no well-controlled studies which demonstrate that tongue thrust *does not* cause malocclusions or foster relapses in orthodontically-treated patients.
6. There is evidence that the anterior maxillary teeth are held in a state of equilibrium, even though tongue pressures normally are greater than lip pressures, by a delicate balance between these opposing forces.
7. There is a possibility that persistent tongue thrust during swallows, anterior resting postures of the tongue, and dentalization of alveolar consonants, in various combinations with other factors, may contribute to the development of, or relapse to, malocclusion.
8. Form and function probably affect each other reciprocally.
9. The transition from a tongue thrust swallow to a "normal" swallow in children is probably related to a number of factors, including maxillary and mandibular growth patterns, oral habits, neurological maturation, and tonsil and adenoid size.
10. Aspects of the swallowing process that sometimes are under voluntary control are related neurologically to muscle activities during speech.
11. When lisping, tongue thrust, and malocclusions occur concomitantly in a child, the decision regarding which problem to treat first, or which ones to treat concurrently, should be made only after a thorough consideration of all the factors present in that particular child. No order of approach should be considered generally unwise.
12. The presence of enlarged tonsils and/or adenoids presents a possible barrier to the effectiveness of tongue thrust therapy. If possible and warranted in an individual case, medical treatment should precede myotherapy. If not, myotherapy is often successful in spite of the untoward physical condition.
13. There are advantages to providing therapy to children who tongue thrust before they reach adolescence.
14. Several studies have demonstrated the effectiveness of myotherapy in preventing orthodontic relapse in children of all ages.

Recommendations:

The following research is needed:

1. A well-controlled longitudinal study (of eight or nine years) to determine the effectiveness of a) orthodontic treatment without myotherapy; b) orthodontic treatment concurrent with myotherapy; c) orthodontic treatment followed by myotherapy; d) myotherapy versus appliance therapy (i.e., dental cribs).

2. An expanded study similar to that of Lear and Moorrees, wherein total pressure patterns, including rest postures of the lips and tongue, pressures during the dentalization of alveolar consonants, and pressures during swallows of various media (saliva, liquids, food) are evaluated relative to their effectiveness in moving teeth.

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