

both groups received comprehensive fixed-appliance treatment. Did the authors assume that at this point the previous pattern of inherited growth reestablished itself? If so, on what grounds did they rule out the alternative possibility that the force of the "comprehensive" fixed appliance overwhelmed the differences created by the early treatment? To accept the 1 explanation without considering the other would be poor logic and therefore poor science.

A previous study of the same patients at the University of North Carolina found that the children who had 2-phase treatment had little root resorption (5%) compared with substantial resorption (20.4%) for those who received 1-stage treatment.¹⁰ This interesting finding has no obvious explanation, because both groups were finished with fixed appliances. Mavragani et al¹¹ suggested that "Roots do not resorb before closure of the apex," and I believe that, for every millimeter you move a closed apex, you will lose a millimeter of root length. Perhaps the more gentle forces of functional appliances reduced the amount of movement subsequently required by the fixed appliances, but, whatever the reason, differences of this magnitude require careful analysis.

The continuous light-wire forces that are now almost universally used in orthodontics have many advantages, but they have been blamed for significantly increased root damage,¹² which is presumably due to long-term ischemia in the periodontal tissues that we know occurs with modern nickel-titanium wires.

Most orthodontists think of Charles Tweed as a "four on the floor" man, which is why he was thrown out of the American Society of Orthodontics, but, in his later years, he decided that early treatment was essential and accepted only mixed-dentition patients in his practice. He believed that knowledge would gradually replace harsh mechanics, and that most orthodontic treatment would eventually be carried out during the mixed dentition period of growth and development, and before the difficult age of adolescence. Has that time come yet?

Many clinicians think that it is impossible to obtain good alignment without fixed archwires. Not so; I haven't used them for over 20 years. Instead, I use orthotropics,¹³ a single-phase early treatment, because of my concern that fixed appliances increase vertical growth.

John Mew
London, England

Am J Orthod Dentofacial Orthop 2008;133:784-5
0889-5406/\$34.00

Copyright © 2008 by the American Association of Orthodontists.
doi:10.1016/j.ajodo.2008.04.005

REFERENCES

1. Tulloch JFC, Proffit WR, Phillips C. Outcomes in a 2-phase randomized clinical trial of early Class II treatment. *Am J Orthod Dentofacial Orthop* 2004;125:657-67.
2. Dolce C, Schader RE, McGorray SP, Wheeler TT. Centographic analysis of 1-phase versus 2-phase treatment for Class II malocclusion. *Am J Orthod Dentofacial Orthop* 2005;128:195-200.
3. Collett T. Evidence, judgment, and the clinical decision: an argument for evidence-based orthodontics. *Am J Orthod Dentofacial Orthop* 2008;133:190-4.

4. Popper KA. Conjectures and refutations. London: Raubledge & Kea Paul; 1963.
5. Mew JRC. The postural basis of malocclusion: a philosophical overview. *Am J Orthod Dentofacial Orthop* 2004;126:729-38.
6. Mew JRC. The aetiology of temporomandibular disorders: a philosophical overview. *Eur J Orthod* 1997;19:249-58.
7. Franchi L, Baccetti T, McNamara JA Jr. Postpubertal assessment of treatment timing for maxillary expansion and protraction therapy followed by fixed appliances. *Am J Orthod Dentofacial Orthop* 2004;126:555-68.
8. Peck H, Peck S. A concept of facial esthetics. *Angle Orthod* 1970;40:119-27.
9. Platou C, Zachrisson BU. Incisor position in Scandinavian children with ideal occlusion. *Am J Orthod* 1983;83:341-52.
10. Brin I, Tulloch JFC, Koroluk L, Phillips C. External apical root resorption in Class II malocclusion: a retrospective review of 1-versus 2-phase treatment. *Am J Orthod Dentofacial Orthop* 2003;124:151-6.
11. Mavragani M, Bøe OE, Wisth PJ, Selvig KA. Changes in root length during orthodontic treatment: advantages for immature teeth. *Eur J Orthod* 2002;24:91-7.
12. Weiland F. Constant versus dissipating forces in orthodontics: the effect on initial tooth movement and root resorption. *Eur J Orthod* 2003;25:335-42.
13. Mew JRC. Facial changes in identical twins treated by different orthodontic techniques. *World J Orthod* 2007;8:174-88.

Editor's response

I appreciate Dr Mew's thought-provoking letter. I do need to respond to a few of his comments, including innocent mistakes made by all too many clinicians when discussing the 2-phase studies from the universities of North Carolina, Florida, and Manchester, United Kingdom. Dr Mew cites 1 of the original articles: "2-phase treatment started before adolescence in the mixed dentition might be no more clinically effective than 1-phase treatment started during adolescence in the early permanent dentition," and then tells us that the authors really mean "don't waste your time and money on 2-phase (early) treatment." I don't think that interpretation is quite fair. These studies were designed to evaluate the correction of malocclusions characterized by prominent maxillary front teeth (mostly Class II problems with excessive overjets). As practitioners, we all know that there are many other reasons to treat in the mixed dentition, and often very early. I know of no one who disputes the importance of correcting crossbites and functional shifts as early as possible. The potential for problems resulting from persistent injurious habits falls into this category, as does the value of preventing the development of dental asymmetries directly related to the premature loss of deciduous teeth. I could go on and on about the problems I see residents treating early in our university programs. So, let's get this straight: early treatment has not disappeared.

Dr Mew's comments regarding Tony Collett's recent article leave me puzzled. I happen to agree with Tony's comments regarding 2-phase treatment and the value of evidence-based studies. Dr Mew stated that logic is important, and he knows, based on the life sciences, that early

treatment certainly ought to be successful. Of course, early treatment for Class II problems is successful, and the 2-phase studies show this—it just takes longer and costs more, and is not necessarily the only way to correct the Class II skeletal problems. I was taught to love 2-phase treatment as a resident, and I was loyal to that concept for years, just like we all love job security. The earlier the patients appeared at my door, the longer I could treat them. Of course, I tended to overlook loss of compliance, root resorption, and enamel decalcification. I always liked to show the records of successfully treated patients. I well remember a 2-phase patient I claimed credit for treating successfully, pointing out excellent mandibular growth, until I finally realized that the patient did all the growing—I had nothing to do with it. Granted, I might have done a good job of redirecting the eruption of her teeth and could take responsibility for the dentoalveolar changes that accompanied her tooth alignment with fixed appliances. But, after thinking a long time about the correction, I no longer take credit for the mandibular growth of my favorite patients. Long-term studies to prove that we can enhance mandibular growth do not yet exist.

Finally, after reading Dr Mew's concluding comment, I have great respect for his ability to treat selected patients with the functional approach called orthotropics—but not enough to adopt such a limiting treatment approach as my own. With the popularity of fixed appliances at an all-time high, the odds of the international orthodontic community switching to a 100% functional approach is beyond my imagination.

David L. Turpin, Editor-in-Chief
Seattle, Wash

Am J Orthod Dentofacial Orthop 2008;133:785-6
0889-5406/\$34.00

Copyright © 2008 by the American Association of Orthodontists.
doi:10.1016/j.ajodo.2008.04.006

Surgically assisted rapid palatal expansion

We thank Drs Suri and Taneja for their excellent article, "Surgically assisted rapid palatal expansion: A literature review" (*Am J Orthod Dentofacial Orthop* 2008;133:290-302). We appreciate being referenced. However, for this article to be accurate and part of evidence-based knowledge, we want to correct misstatements regarding our article.

In Table II, "Chronological listing of studies reporting surgical procedures and treatment protocols (no studies used controls)," we are listed as having performed LeFort I osteotomies. On page 258 of our original article (Alpern MC, Yurosko JJ. Rapid palatal expansion in adults with and without surgery. *Angle Orthod* 1987;57:245-63), the caption for Figure 9 states, "Model of a horizontal section through the maxilla at the level of the osteotomy, with the osteotomy cut marked on the side. Note that the midline structures are untouched in the surgical procedure." Thus, this cannot be considered a LeFort I procedure. Furthermore, describing the surgical procedure, we wrote, "Utilizing a No. 702 fissure bur, a horizontal osteotomy is made

well above the apices of the teeth, parallel to the occlusal plane from the piriform aperture to the pterygoid fissure. This osteotomy is carried intranasally along the lateral nasal wall. In the area of the pterygomaxillary fissure, small curved osteotomes are used to effect separation of the pterygoid plates."

Continuing on the next page (259, second paragraph), we wrote, "No midline, palatal suture, medial nasal wall, or nasal septum surgery have been required. Patients tolerate the procedure well, with a minimum of blood loss or surgical risk. Postoperative edema is moderate and pain is minimal." This surgical procedure should not be labeled "LeFort I."

Also in Table II, the columns "Latency period" and "Postoperative protocol" indicate that this information was not reported in our article. Our article was published in 1987, and the concept of a latency period did not exist. We activated the expansion screw 6 to 8 times in the operating room. The patient turned the expansion screw once per day without interruption until adequate expansion was achieved. Then, the expansion screw was ligated to prevent it from vibrating closed. The palatal expansion with the bite plane appliance was maintained for 4 months and then removed, and a palatal bar was immediately placed. We clearly described our procedures (pages 259-61 of our article) and thoroughly discussed all our results.

This technique has been successfully followed since 1987 without any changes and without complications or sequelae.

Michael C. Alpern
John J. Yurosko

Port Charlotte and Venus, Fla

Am J Orthod Dentofacial Orthop 2008;133:786
0889-5406/\$34.00

Copyright © 2008 by the American Association of Orthodontists.
doi:10.1016/j.ajodo.2008.04.011

Authors' response

We thank Drs Alpern and Yurosko for their letter regarding their reference that is quoted in our article, "Surgically assisted rapid palatal expansion: A literature review" (*Am J Orthod Dentofacial Orthop* 2008;133:290-302). Drs Alpern and Yurosko disagree with the term LeFort I osteotomy for the surgical expansion technique outlined in their article. To clarify, we quote: "LeFort I fracture separates the maxilla from the pterygoid plates and nasal and zygomatic structures. This type of procedure may separate the maxilla in one piece from the other structures, split the palate or fragment the maxilla."¹

Thus, the procedure that Drs Alpern and Yurosko described in their article (diagram and text) and elaborated in their letter to the editor is in fact a LeFort I osteotomy. We do agree that the modification they made in the procedure—that no midline cut or separation of the nasal septum was made—should have been included in the review. However, segmentation of the maxilla, either 2, 3, or multiple pieces, does not preclude it from being a LeFort I osteotomy. We would also like to clarify that none of the articles for SARPE recommend a down fracture of the maxilla.