LETTERS FROM OUR READERS:

RE: *The Angle Orthodontist,* 1999; 69:523–528

To: Editor, *The Angle Orthodontist*

RE: Epithelial-mesenchymal transformation, palatogenesis and cleft palate

We write with regard to the article by Shapira et al[1] entitled ''The distribution of clefts of the primary and secondary palates by sex, type, and location.'' While we find great merit in the objectives of the study, we would like to clarify some points of discussion concerning palatogenesis and the etiology of cleft palate. The authors comment that clefts form when palatogenesis is disrupted including when 'the mesenchyme. . . penetrate(s) through the epithelial membranes.' Using the references of Loevy[2] and Kitamura[3] perpetuates the notion that palatogenesis is an example of programmed cell death in which apoptosis of medial edge epithelial cells occurs.

It has been shown by electron microscopy[4] and Tunel staining[5] that in vivo cell death is rare and is restricted to the periderm with basal cells remaining healthy. When 2 palatal shelves are placed together in vitro, peridermal cells slough off and are trapped in the fusing epithelial seam, inciting the appearance of lysosomes and leading to the erroneous conclusion that all cells in the seam are dying. In fact, the mechanism in palatogenesis following adherence of the 2 palatal shelves is epithelial-mesenchymal transformation.[4,6,7] This has been confirmed by Carboxyfluorescein staining at light and electron microscopic levels[8,9] and by DiI staining.[10,11] Epithelial-mesenchymal transformation of medial edge epithelium to form mesodermal confluence of the palate is now a well-recognized mechanism in palatogenesis.[12,13]

While this is not the focus of the Shapira et al[1] paper, we feel it is important to correct this point. We congratulate the authors on their epidemiologic study of the distribution of cleft palate.

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Response by the authors:

It would be hard to imagine receiving comments that are more relevant nor receiving them from individuals so eminently qualified to make them. It would also be less than honest not to mention that both authors were students of Dr Hay during their education at Harvard.

Our paper in question dealt with the distribution of the most common of all craniofacial anomalies, dento-alveolar clefts. The pathogenesis of these often-disfiguring conditions has not been explored to any detail in our manuscript. We humbly acknowledge that many authors, including Drs Hay and Lavin, are substantially better qualified to write on the subject than we are. Indeed, we would hope that for the further erudication of the journal's readers on the subject, Drs Hay and Larvin take to their pens and write a manuscript in which they would explore the current concepts of palatogenesis. They could elaborate on both discredited or rejected hypotheses and present results of the latest research they and other workers in the field have recently concluded. On our part, we have committed to detailed studies of several aspects of skeletodental anomalies, taking advantage of an unusually well documented and a reasonably large sample of clefts. We will continue reporting our findings in our hope to clarify some controversies and inconsistencies found in the previous reports.

Again, we appreciate and value comments offered by Drs Lavin and Hay.

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