Are we ready to blame imatinib for palatal pigmentation?

In reply:

We were very interested to learn that Dr. Torres-Pereira and his colleagues have observed similar palatal pigmentations in patients treated for CML with imatinib mesylate. As pointed out in our report, the question of a connection between imatinib mesylate and the observed pigmentations is to a certain extent also a matter of excluding other diseases or drugs known to be associated with hyperpigmentation. Two drugs used in treatment of CML and known to cause hyperpigmentation are hydroxyurea and chloroquine. The question from Dr. Torres-Pereira is therefore most justified.

We have contacted the patients and also reviewed the medical charts of the 2 cases (case 2 and case 3) who received treatment with imatinib mesylate for CML.

Case 2 was diagnosed with CML in 2005. The patient had not received treatment with either hydroxyurea or chloroquine.

Case 3 was diagnosed with CML in 2001. Between 2001 and 2003, she received treatment with hydroxyurea (Hydrea) 500 mg daily. She had a lot of adverse reactions and was put on medication with imatinib mesylate 400 mg daily in 2003 and has been on that medication since then. She has not received treatment with chloroquine. The patient’s regular dentist between 2002 and 2008 was contacted, and the dental records during this period do not show any comments regarding a palatal pigmentation. The patient’s present caregiver confirms the presence of a palatal hyperpigmentation in 2009.

Dr. Torres-Pereira asks if we are ready to blame imatinib mesylate for palatal pigmentations. Case 3 received treatment with hydroxyurea for 2 years between 2001 and 2003. As far as we have been able to establish, there was no palatal hyperpigmentation in chronologic connection with this treatment. Despite the fact that we cannot with absolute certainty establish the time of debut of the palatal hyperpigmentation, we still suggest that it is at least unlikely that the hydroxyurea treatment was responsible.

The only common factor we have been able to find among the 3 cases is the use of imatinib mesylate. This does not exclude the possibility that there may be other factors undetected by us which may explain the observed palatal pigmentations. Consequently, we feel it is unwise to categorically state that imatinib mesylate is responsible, but we do suggest that the drug is very strongly implicated.

Nerve imaging: is it better?

To the Editor:

Terumitsu et al.1 present a new and unique imaging modality to better visualize the inferior alveolar nerve (IAN) in patients that complained of persistent sensory disturbances. With this technique, the authors were able to identify abnormalities in the course of the IAN and the surrounding tissues in 16 patients. They also attempted to correlate these findings to surgical findings and histopathologic examination in 6 of the 16 patients. With this information, they state the following:

1. This technique “was clearly superior to other diagnostic imaging studies used to identify the underlying cause of the sensory disturbance.”
2. “The current technique was found to provide information directly relevant for clinical judgment.”
3. This technique “can significantly improve patient care.”

Although this technique is certainly an improvement in imaging the IAN (and hopefully other branches of the trigeminal system), I do not think that this technique adequately fulfills these statements by the authors.

Evaluating patients with sensory abnormalities (from whatever cause, even unknown) is challenging in clinical practice with the typical neurosensory examination techniques currently available.2 Although these examination techniques are “crude,” they are fairly well standardized in the literature with the use of clinical neurosensory testing and classifying sensory abnormalities as either level A, B, or C sensory disturbance.3

The authors’ patient population is varied regarding the classifications of either paresthesia, dysesthesia, spontaneous pain, touch-evoked pain, and “chronic pain” (not well defined). In addition, 1 patient had persistent abnormal sensation with no morphologic changes seen with this
imaging technique. Given this, they make the assumption that a very sophisticated imaging technique that can clearly identify anatomic alterations can provide information regarding the underlying pathophysiologic mechanism for the sensory findings and patient complaint. In addition, they invoke the surgical findings as being correlated with the imaging findings and clinical neurosensory examination findings and that this is indication for the surgical intervention.

Although they correctly state that microneurosurgical procedures do have efficacy in improving sensation to so-called “functional sensory return,” there are also data to show that there is poor correlation between the level of sensory alteration (A, B, C) and the surgical findings.4-7 Having said that, the authors also think that this technique will “significantly improve patient care” without presenting the outcome data on their patient population in this study.8

The authors should be congratulated on presenting the first report of a new and exciting imaging technique that will hopefully add to our ability to comprehensively evaluate, classify, and make difficult clinical decisions in patients that present with troublesome trigeminal sensory abnormalities. At this time, I am not sure that this technique sufficiently adds to our ability to differentiate nerve abnormalities any better that the current methods of a detailed history, physical examination, and clinical neurosensory testing with the use of current well established testing classification for the trigeminal system.

Steven J. Scrivani, DDS, DMedSc
The Craniofacial Pain Center
Tufts University School of Dental Medicine
Boston, Massachusetts

REFERENCES


Morphological evaluation of the inferior alveolar nerve in patients with sensory disorders by high-resolution 3D volume rendering MR neurography on a 3.0T system

In reply:

We appreciate Professor Scrivani’s positive statement regarding our work. Although we agree with the fact that an imaging method never replaces quantitative sensory testing in clinical judgment, we nevertheless think that development of a better diagnostic imaging method helps improve patient care.

The algorithm presented in our report1 represents a technique capable of much higher anatomic resolution of the inferior alveolar nerve compared with any other method hitherto available. A basic rule for any new imaging system and algorithm2,3 is that the judgment of how the new technique affects everyday clinical practice has to wait until the time when the method, including the system and algorithm, becomes available to the majority of clinicians. Meanwhile, we will continue to pursue our goal of developing microscopic resolution for disease processes.4

Makoto Terumitsu, DDS, PhD
Division of Dental Anesthesiology
Graduate School of Medical and Dental Sciences
University of Niigata
Niigata, Japan

Tsutomu Nakada, MD, PhD, FAAN
Center for Integrated Human Brain Science
Brain Research Institute, University of Niigata
Niigata, Japan

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