LETTERS TO THE EDITOR

Letter to the Editor on Chronic fibrosing osteomyelitis of the jaws: an important cause of recalcitrant facial pain. A clinicopathologic study of 331 cases in 227 patients by Goldblatt LI, Adams WR, Spolnik KJ, Deardorf KA, Parks ET

To the Editor:

The recent article by Goldblatt et al. attempts to resurrect an orofacial pain concept that has come and gone several times in the past 30 to 40 years. As they correctly point out in their first paragraph, this concept has had many different names in the past (ischemic osteonecrosis, bone marrow edema syndrome, neuralgia-inducing cavitation osteonecrosis, Ratner bone cavity, and alveolar cavitation osteopathosis). Now these authors have presented similar findings under the new name chronic fibrosing osteomyelitis of the jaws, but clearly they are still invoking a familiar theory that they believe can explain certain cases of orofacial pain. However, what they fail to reveal is that each previous version of this concept has been seriously challenged and often rebutted by experts in the field. We, therefore, have to question whether this iteration provides new evidence that would be convincing. The following are some of the criticisms of this article:

1. All of the 227 patients in this report were treated in a single endodontic practice in Indiana by 2 of the authors, and the specimens they obtained from their surgical procedures were all evaluated by the Oral Pathology Department at the Indiana University School of Dentistry. This raises questions of possible confirmation bias. This same issue was raised several years ago when it was commented that the etiology and pathogenesis of neuralgia-inducing cavitation osteonecrosis, bone marrow spaces in trabecular bone are a rather common finding. Moreover, even with cone beam computed tomography, they note that bone marrow spaces in trabecular bone may appear normal. In such cases, how is the surgical site detected? If one is unable to identify the site that would benefit from surgery, why would a surgical approach be an acceptable intervention?

2. Goldblatt et al. indicate that using cone beam computed tomography will produce more “findings” of cavitation spaces because they are 3-dimensional. However, bone marrow spaces in trabecular bone can be a result of bleeding during the procedure. Moreover, even with cone beam computed tomography, they note that bone may appear normal. In such cases, how is the surgical site detected? If one is unable to identify the site that would benefit from surgery, why would a surgical approach be an acceptable intervention?

3. The histologic findings in this study are not unique. Fatty marrow in older individuals can account for the lipid droplets observed, and the red blood cells can be a result of bleaching during the procedure. Moreover, if this is truly osteomyelitis, why are inflammatory cells absent and viable bone is present? There seems to be a contradiction in terms.

4. There is no explanation regarding how these observed clinicopathologic features account for the pain experienced by these patients.

5. Out of 227 patients, only 70 returned the follow-up questionnaires, so how can the authors claim an 83% success rate?

There is another important clinical consideration that must be acknowledged when discussing this concept. Current evidence regarding the category of persistent orofacial pain described by most patients, suggests that most patients are experiencing some form of neuropathic pain. Such patients generally require thoughtful medical management rather than surgical procedures. Additionally, it is well known that repeated surgical interventions into sensitized regions can exacerbate neuropathic pain problems (e.g., windup), so it becomes even more essential to avoid unproven, invasive surgical procedures, such as those described in this article.

The authors concluded that much remains to be learned about this condition. Until then, reviving an unproven concept under a new name only adds to the confusion and increases the risk of more patients being subjected to unnecessary surgical procedures.

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2212-4403/$ - see front matter https://doi.org/10.1016/j.oooo.2018.02.743

To the Editor:

Thank you for the opportunity to respond to the Letter to the Editor (LTE) regarding our recently published study of chronic fibrosing osteomyelitis of the jaws (CFOJ). We offer the following observations. As a general comment, we wish to assure the readers that we are not attempting “to resurrect an orofacial pain concept that has come and gone several times in the past 30 to 40 years.” Rather, we are presenting the clinicopathologic findings of 331 new cases of a specific condition that appears to respond to a specific therapy in a significant percentage of patients. We do not claim to have proof of the cause(s) but, rather, offer a hypothesis as to the pathogenesis along the lines of ischemic necrosis of the bone marrow of the jaw with an accompanying chronic inflammatory response of the surrounding bone. We look forward to further studies from other groups refining our knowledge of this entity.

We offer the following reflections on some of the points made in the LTE. (1) The conclusions of the LTE authors seem to be based largely on past studies and opinion pieces which have, in all probability, dealt with a heterogeneous group of lesions, many of which share the characteristics of atypical jaw pain mimicking odontogenic pain, but little else. Their conclusions seem to be based less on the substantial observations, data, and follow-up on the 331 new cases we have personally handled and described in our paper. (2) We attribute what the LTE authors call “confirmation bias,” to careful and consistent calibration in preoperative evaluation, surgical procedure, and histopathologic criteria. We believe this consistency adds confidence to the results. (3) The