

*In reply:*—Drs Kolokythas and Ord have commented on our report and shared their experience with the surgical approach for parapharyngeal space (PPS) tumors. As we know, primary PPS tumors are rare and account for less than 1% of head and neck neoplasms.<sup>1</sup> However, the anatomy of the PPS makes the clinical examination very difficult; thus, these tumors can achieve an extensive size before they can be identified. PPS tumors still represent a challenge to the surgeon in both the evaluation of the preoperative condition and the appropriate surgical approach.

Surgery is the mainstay treatment of PPS tumors.<sup>2-4</sup> Many techniques for surgical access have been described in published reports for the removal of PPS tumors. They have included transoral, transcervical, transcervical-transparotid, transcervical-transmandibular, infratemporal fossa, and so forth.<sup>2,3,5</sup> Each technique has its indications, advantages, and disadvantages; however, they should follow 2 basic principles: oncologic principles and minimal damage. Unfortunately, these 2 principles often conflict with each other. It is necessary for the surgeon to measure and determine the best choice according to the circumstances.

In most cases, transcervical and transcervical-transparotid are the standard approaches. Mandibulotomy might be required for vascular lesions in the upper part of the PPS or for malignant malignancies for which good access and optimal vessel control is vital. However, the specific points of the mandibular osteotomy can vary. It can be at the angle, midline, body, or ramus. If necessary, a double mandibular osteotomy can be performed. The use of rigid internal fixation is a significant advance for single and, especially, double mandibular osteotomies. As the commentators mentioned, it allows for immediate function with no limitations as long as the preoperative occlusion has been maintained.

From our past experience, the transcervical-transmandibular approaches should be used for very large tumors, malignant tumors, and as a part of a composite resection. We prefer that mandibulotomy be performed in the region of the mandible angle to achieve adequate exposure. One disadvantage is that the osteotomy necessarily transects the inferior alveolar nerve (IAN) in this region. We agree with the commentators' argument that transection of the IAN is not easily justifiable for access alone in benign tumors. We also pay attention to this problem and use a technique designed with IAN preservation.

Double mandibular osteotomy for the removal of tumors in the PPS was first described by Attia et al.<sup>4</sup> Their technique involved splitting of the lip, with the attendant esthetic consequences. In 2007, Kolokythas et al<sup>6</sup> reported the use of a double mandibular osteotomy without lip splitting to remove tumors of the PPS. As Kolokythas and Ord described in their letter, this technique (a vertical or horizontal subsigmoid osteotomy combined with a paramedian osteotomy) provides excellent access to the PPS without the need for splitting the lip or sacrificing the IAN.<sup>5-7</sup> We also believe this is a good method for resecting very large tumors and malignant tumors in the PPS. However, we have found it difficult to include this technique into our clinic practice until now, because it requires sophisticated tools for the osteotomy. In the osteotomy, especially in the paramedian osteotomy, it is difficult to precisely control the position of osteotome; thus, it is easy to damage the nerves and roots of the tooth. In addition, this approach increases the surgical trauma. Despite these factors, we intend to attempt this approach in appropriate situations in the future.

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## PROPHYLACTIC ANTIEMETICS IN ORAL AND MAXILLOFACIAL SURGERY: A REQUIEM?—A RESPONSE

*To the Editor:*—I wish to raise a few concerns regarding the article, "Prophylactic antiemetics in oral and maxillofacial surgery: A requiem?," published in September 2009.<sup>1</sup>

Extensive, well-conducted research has been done with regard to the incidence, causes, prophylaxis, and treatment of postoperative nausea and vomiting (PONV). Patient and anesthetic risk factors have been consistently identified as predictors of PONV, whereas surgery type has a poor relation to PONV.<sup>2-4</sup> Due to the confounding effect and distribution of these risk factors, guidelines have been published suggesting how to conduct and present research related to PONV.<sup>5</sup> The use of a PONV predictive score and the administration of antiemetic prophylaxis and treatment of patients experiencing events would be deemed the minimum in any study examining PONV.<sup>5,6</sup>

This study did not assess risk in patients for PONV preoperatively,<sup>7</sup> did not administer prophylaxis to high-risk patients,<sup>3,4</sup> did not treat patients with an existing and distressing problem,<sup>8</sup> but exposed them to a potentially high-risk and costly intervention.<sup>9</sup> This study's methods and results are to be seriously questioned. The ethics of such a study design and the use of a potentially high-risk intervention are concerning. The recommendations and conclusions fly in the face of overwhelming evidence to the contrary and cannot go unchallenged.

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*In reply:*—I am indeed happy to note the interest that Dr Rodseth has shown in our article about postoperative nausea and vomiting (PONV). I am also glad to clarify the issues raised by him.

Our anesthetist, with more than 35 years' experience, does not administer prophylactic antiemetics routinely. So it was easy to study the incidence of PONV in oral and maxillofacial surgery. For the same reason, the doubts raised regarding the ethics as well as the methods of this study do not stand scrutiny. Of the articles mentioned by Dr Rodseth (many of them quoted in our article also), the latest and most comprehensive is the one by Dr Tony J. Gan.<sup>1</sup> This is a "qualitative review—to summarize findings of the past decade and a half of PONV risk factors." It "focuses on key prospective clinical studies involving consecutive adult or pediatric patients or both, published in peer-reviewed journals—and in most cases, did not report having given anti-PONV prophylaxis." Our study fulfilled those conditions. About risk factors, Dr Gan says that "Current understanding of risk factors of PONV is incomplete, in part because much remains to be elucidated about the pathophysiology, particularly their molecular biology." About the scoring systems, he is of the opinion that "no scoring system yet has emerged as a 'gold standard' based on accuracy" and that "in other words, these scoring systems achieve a 12%-57% relative improvement over guess work" and he concludes that "however, it should be noted that these scoring systems are only moderately accurate in predictive ability."

At the end of the surgery our anesthetist routinely passes a nasogastric tube for almost all our patients and aspirating the stomach contents through it, as we did, does not exactly constitute a "high-risk intervention." (The cost of a nasogastric tube in India is Rs 16/—, around 38¢, so the question of cost also does not arise.) The "position paper"<sup>2</sup> that Dr Rodseth has quoted clearly states in its "Rationale" that "this Position Paper does not review the use of a small bore

nasogastric tube when used to aspirate stomach contents or to administer activated charcoal."

To add to all this, if the ongoing uncertainty "whether monotherapy or multidrug therapy or multimodal management is best for antiemetic prophylaxis"<sup>1</sup> is what constitutes "overwhelming evidence to the contrary," we stand by our conclusions and recommendations, until proven wrong, which the above letter has not.

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### AN EVIDENCE-BASED REVIEW?

*To the Editor:*—With interest we read the paper of Ow and Cheung<sup>1</sup> on the difference in skeletal stability and complications between bilateral sagittal split osteotomies (BSSO) and mandibular distraction osteogenesis (MDO). Evidence-based or systematic reviews are a valuable tool to gain scientific evidence by combining the results of several individual studies. However, there are strict rules that have to be obeyed when performing a systematic review; otherwise, the results of the different studies included in a review cannot be compared (see, for example, the Web site of the Cochrane Collaboration).

First, it is very important that the research used for the review has a sufficient scientific level. This means that, preferably, randomized clinical trials should be included. If there is an inadequate number of randomized clinical trials, cohort studies or patient control studies can be used. It is mandatory that all studies have control groups; otherwise, no statistic analyses can be carried out, which makes it impossible to combine the results or to make corrections for confounding and to reach any valuable conclusion. Moreover, the methods (measurements) used for assessment of the results must be reproducible and must have good validity (ie, we can draw accurate conclusions about the presence and degree of the attribute for an individual). When assessing the articles that were included in this study, they are mostly descriptions of series of cases with a few papers comparing the stability of a BSSO with different fixation techniques. In the MDO group only case reports (series) exist. Therefore, when combining these studies in a review, only a larger case report arises but with different methods for assessment of stability or nerve disturbances and other complications. Without the presence of a controlled study design, these results may never be compared.

The second concern is of bias and confounding. There is a large selection bias and also information bias concerning the selected studies (see, for example, the large difference in age and gender between the BSSO and MDO groups). Moreover, there is a huge disparity between the number of patients in the different groups (222 vs 13 and 1,109 vs 70), and patients who underwent an MDO procedure were probably very differently informed about the risks of nerve disturbances, etc, than patients who underwent a BSSO. Confounding is an even bigger problem because of the