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Letter to the Editor regarding transoral laser microsurgery followed by radiotherapy in advanced oropharyngeal cancer

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Patel and colleagues¹ are to be congratulated for demonstrating excellent outcomes in their patients treated with TLM and adjuvant therapy. However, it is important to stress that the conclusion should have read “TLM followed by RT for selected patients with advanced oropharyngeal cancer results in excellent control rates”, as this retrospective study has clear selection bias. The study design would have been far more interesting (and novel in comparison to other TLM/TORS series) had the authors queried their registry for all previously untreated patients with oropharyngeal cancer, so the reader could better understand how many stage 3 and 4 patients are selected for this therapy. Granted, as a center of excellence for this surgery, there might still be bias, as >50% of patients from their initial search had stage I and II disease, so their population might reflect patients who seek their surgical expertise.

The authors’ comparison with one chemoradiation series (Setton et al.) further highlights this patient selection.² The caveat that more patients in the Memorial Sloan-Kettering Cancer Center (MSKCC) study had T3/T4 disease is correct, but understates the difference, as Patel et al. only had 9% T3 patients and no T4 patients. Patel et al. also correctly point out that the majority of patients in the MSKCC study had T1/T2 disease, but the percent of T1 patients was only 27%, compared to 53% in the current paper. T1-node-positive patients are nominally stage 3 and 4, but most chemoradiation studies have either excluded these patients, or have very few patients with this low tumor burden, and these patients can be treated effectively with radiation with or without neck dissection.³ Additionally, while the authors discuss a prior experience with TLM alone for stage 3 and 4 patients, those patients were highly selected, and even including patients who refused adjuvant treatment, only 9 had N2b disease or greater.

Patel et al. also described a 1-yr gastrostomy rate of >6%, “comparable to rates described in definitive chemoradiation series”. This was despite eliminating chemotherapy in half the patients, and focusing on the neck only in nearly one-third of patients. Since only some patients had this significant deintensification, there are 2 possibilities. Either there was no benefit, as the rate was consistent for all patients regardless of modifications, or those who had less therapy had lower rates of tubes, suggesting that those who were not suitable for treatment modifications following surgery had higher rates. Put simply, if patients receiving TLM followed by RT have gastrostomy rates are not demonstrably improved from radiotherapy alone, why add surgery in the absence of perceived oncologic benefit?

Thus, deintensification strategies should not be limited to only identifying those patients for whom adjuvant therapy dose may be reduced, but also clearly selecting which patients will benefit most from adding surgery to their management. For example, many no longer advocate planned neck dissections post chemoradiation.⁴ However, the question of the role of the upfront neck dissection remains unanswered, yet it appears from the current series and others, that despite the emphasis on TLM (as reflected in the title and conclusions), that neck dissection is the principle driver of decisions regarding adjuvant therapy in these patients with stage 3 and 4 disease.

TLM and TORS have been established as effective oncologic treatments, and are positive additions to our therapies to treat oropharyngeal cancer, as reflected in the current series. Unfortunately, the current staging system has too broad a definition of stage 4. It is specious, for example, to equate gastrostomy probabilities from patients with T1N2aM0 oropharyngeal cancer (who might be excellent candidates for therapy deintensification by TLM+RT or RT alone) with patients who have T4N2cM0 requiring chemoradiation, as if both are likely to achieve comparable outcomes, either in local control or therapy-attributable toxicity.⁵ Consequently, we recommend re-evaluation of current staging practices, as they are inadequate for modern oropharynx patient populations.⁶

Excellent oncologic results can be obtained with TLM and TORS in well-selected cohorts, and efforts such as Patel et al. are to be applauded for continuing to define the potential parameters and clinical endpoints of interest for such approaches in future prospective trials. In the meantime, it is essential that manuscripts focusing on TLM and TORS define which subgroups of patients with “advanced oropharyngeal cancer” they have selected for these therapies, in order that we may, by strenuous avoidance of “straw man” comparisons, cogently select those oropharyngeal cancer patients with maximal probability of therapeutic gain from these exciting technologies.

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