To the Editor:

I read with considerable interest the article by Sjögren et al.\(^1\) who concluded that a typical laser treated (type I or II laser chordectomies) voice for T1a glottic cancer is characterized by mild breathiness in perceptual analysis. I would like to compliment the authors on their work, but I feel that the conclusions drawn were not made using an appropriate analysis process. As shown in Table 7 of the article, the conclusion of Sjögren et al.\(^1\) is different from the results of similar studies conducted by 2 groups of Italian doctors, Peretti et al.\(^2\) and Ledda et al.\(^3\) who disclosed roughness to be the dominant feature of a type I or II laser chordectomies treated voice. However, the calculation of the mean of GRBAS scale in Sjögren's article and the other 2 Italian articles were not performed on the same basis. In Sjögren's article, 12 patients were excluded with no GRBAS pathology in their 34 cohort cases (the denominator). If they included all patients receiving perceptual analysis as in the other 2 articles, they should have used 34 as the denominator to recalculate the mean grade of breathiness. Therefore, the grade of breathiness should be reduced to 0.82, which is closer to the result of Ledda et al.\(^3\) Also, in Table 3 of the article, I noticed there were 2 patients with intermittent roughness and their grade of roughness pathology was not counted. This classification complicates the statistical analysis and I think the mean grade of roughness will most likely be underestimated compared to other parameters (G,B,A,S).

Furthermore, the theory that breathiness comes from incomplete glottic closure is also not well supported by their statistically insignificant results ($p = .57$). In the article by Peretti et al.,\(^2\) the authors observed that regeneration of the neocord after chordectomies not extended beyond the superficial portion of the vocal muscle was associated with complete glottic closure in 89% of patients. The aforementioned glottic closure is common after surgery and may explain why breathiness is not a typical feature of patients in Peretti's study.\(^2\)

In addition, Sjögren et al.\(^1\) hypothesized that the dominant feature of roughness in 2 Italian studies might be due to cultural differences, because in other studies\(^4,5\) vocal fry may have been classed as roughness. I am not sure whether vocal fry was classed as roughness in the 2 Italian studies or not. But I noticed that in Sjögren's article\(^1\) the reduced mucosal wave was found in all stroboscope-assessable patients but 1. In my opinion, the association between reduced mucosal wave and roughness of voice after chordectomy was still unclear, based on the aforementioned imperfect study design of this retrospective article.

In conclusion, this paper is an informative article and I agree with some conclusions such as “multi-dimensional voice assessment protocol is not to be considered the ultimate way to basically assess the voice.” However, based on the limitations of a retrospective study, I propose...
the discovery of typical features of the voice in typical laser treated (type I or II laser chordectomies) T1 glottic cancer still relies on further prospective investigation.

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REFERENCES

Reply:

We thank Dr. Chen-Chi Wang for his careful examination of our article1 and subsequent comments.

With regard to his concern about the calculation of the mean GRBAS scale in our article, we would like to point out that selecting only the perceptually deviant voices was done on purpose, because we were interested in an “undiluted” profile (normal-sounding voices excluded). We believe this information has added clinical relevance; apart from being able to counsel patients about the chance of dysphonia (65%), we can also explain more accurately how the voice will sound. Comparing these data with “diluted” data published by others,2,3 it turned out that not only was the degree of the dysphonia worse, which is what we expected, but, interestingly, the profile differed as well.

As to Dr. Chen-Chi Wang’s concern that our article gave an underestimation of roughness compared to other aspects (GBAS), we can assure him that both patients with intermittent roughness (severity graded as 1) were included in statistical analyses. The relationship between the different aspects is most clearly portrayed in Figure 1 of our article (mean roughness, 0.23, is positioned approximately halfway between 0 and 0.5).

In our article, we did not seek to support or refute any theory regarding breathiness and incomplete glottic closure. We found a trend for association between these 2 aspects, which to our view was worth mentioning. Tissue loss due to laser resection resulting in breathiness is, for example, in line with the work of Jeannon et al4 who concluded: “breathiness is critically dependent upon glottic area.” Dr. Chen-Chi Wang very correctly points out that the observed regeneration of the neocord associated with complete glottic closure may explain why breathiness was not a typical feature in the cohort of Peretti et al.2 This finding does not, however, contradict a possible relationship between glottal closure and breathiness.

Concerning Dr. Chen-Chi Wang’s opinion that the association between reduced mucosal wave and roughness was unclear, we can state the following: to investigate the relationship between auditory perception and all other aspects of the ELS, all patients were included in the analysis, also the 2 patients with intermittent roughness (as mentioned above). However, no association between mucosal wave and roughness was found. As reported in our article, overall associations were poor; the only association bordering on significance was between incomplete closure and breathiness.

We support Dr. Chen-Chi Wang’s proposal that typical features of the voice after laser therapy should be investigated prospectively. In fact, we are currently running a multicenter prospective trial to investigate the functional outcome after laser therapy versus radiotherapy in early glottic carcinoma.

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