CORRESPONDENCE

Long-Term Results From Tonsillectomy in Adults

by Dr. med. Götz Senska, Dr. med. Halil Atay, Dr. rer. medic. Carolin Pütter, and Prof. Dr. med. Philipp Dost in issue 50/2015

Conflicting Study Results

With their retrospective survey following tonsillectomy, the authors' aim was to evaluate the health benefit of this surgical procedure and they reached the conclusion, after time intervals of 14 months and 7 years, that tonsillectomy "was associated with a long-lasting improvement of health and quality of life, and with lower utilization of medical resources" (1). However, why were unoperated patients with the same symptoms not also surveyed? A control group is a basic requirement when evaluating treatment, particularly if the remission rate under conservative treatment is high. Even proponents of surgery report that tonsillectomy is no longer indicated in up to 27% of cases after a waiting period of 9 months (2).

A Cochrane meta-analysis on tonsillectomy compared with conservative treatment in chronic recurrent tonsillitis was not cited (3). Despite a reduction in the number of episodes of sore throat following tonsillectomy, they concluded that: "The size of the effect is modest [...]. It is clear that some children get better without any surgery, and that whilst removing the tonsils will always prevent 'tonsillitis', the impact of the procedure on 'sore throats' due to pharyngitis is much less predictable" (3). A randomized controlled study, cited by the authors as confirmation, also arrives at a no more favorable conclusion: "The modest benefit conferred by tonsillectomy or adenotonsillectomy in children moderately affected with recurrent throat infection seems not to justify the inherent risks, morbidity, and cost of the operations" (4). The authors neither discuss these conflicting statements, nor do they attenuate the validity of their survey with the drop-out-rate of almost 40%, which represents a strong bias.

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A Comparison of Responders and Non-responders Is Lacking

It is thanks to the authors that an up-to-date longitudinal study is finally available, providing impressive evidence that tonsillectomy confers a long-term benefit in patients with recurrent or chronic tonsillitis (1). With regard to the debatable response rate of 62%, the authors did not have the impression that this created a bias. This can be easily—and also more scientifically—redressed: What is lacking, and is required, is a direct statistical comparison of all baseline parameters between responders (n = 71) and non-responders (n = 43). In addition, data is available for n = 26 non-responders at 14 months post tonsillectomy. Thus, these non-responders could also be compared with the responders at 14 month post-surgery. Although the results are clear, not all patients benefit from surgical treatment.

Due to the frequency of the procedure, predictors for better patient selection are desirable. The authors from Gelsenkirchen and Essen (Germany) are unable to identify predictors of improved long-term quality of life. We were recently able to show in our own prospective study using the Glasgow Benefit Inventory (GBI) that particular serum marker profiles are associated with a better GBI at 6 months post tonsillectomy. Pre-operative blood samples were undoubtedly taken from patients in the study by Senska et al.; it would be interesting to see whether serum marker profiles of this kind appear in the Senska et al. study.

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In Reply:

Dr. Reuther summarizes the results of our investigation in his introductory remarks. The possibilities offered by setting up a control group for studies like ours, such as a further strengthening of validity, are clear. However, we know of virtually no (adult) patients that are willing to accept years of work absences and pain as part of randomization.

Dr. Reuther rightly points out that, according to current data, a wait-and-see approach is well worth considering in children. Firstly, the controversy over this wait-and-see strategy does not exist for adult patients, and secondly, children were not the subject of

our investigation. Thus, to include the available studies on this latter patient population would have been confusing. We therefore see no conflict with the studies cited by Dr. Reuther. The possibility that the drop-out-rate might have created a bias was the first point made in the “Limitations” section.

We are very grateful to Prof. Guntinas-Lichius for his suggestions. It must be said with regard to the responder comparison that, in the course of preparing the article, we replaced the missing data from non-responders with a sensitivity analysis using multiple imputations (statistical methods to complete the data lacking due to non-response). The analyses proved to be stable under a variety of conditions. We saw only age differences with no visible effect on the results.

Due to the unavailability of drop-out patients, any subsequent investigation of serum would be associated with a bias and hence unhelpful. Moreover, this would require submitting new ethical-approval and clinical-study applications. The authors of future studies on this

subject will undoubtedly be inclined to adopt this suggestion from the outset of their deliberations and will be grateful, as are we, for these comments.

We fully endorse the important idea of investigating predictors of treatment outcome. Such an approach could reduce the number of patients that do not benefit from treatment. Our investigation, however, does not contribute to an expansion of knowledge in this area.

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Conflict of interest statement

The authors of all contributions declare that no conflict of interest exists.