Reviewer’s report

**Title:** Incidence of postoperative facial weakness in parotid tumor surgery: A tumor subsite analysis of 794 parotidectomies

**Version:** 0  **Date:** 12 Aug 2019

**Reviewer:** Maria Grosheva

The authors analyze the incidence for the facial palsy after parotidectomy for benign and malignant lesions. In their retrospective analysis, they include 794 parotidectomies during a 7 year time period from a single institution. This corresponds to approximately 113 surgeries per year and indicates a very good expertise in parotid surgery.

Besides the retrospective nature of the study, i would like to address an another methodological weak point of the manuscript. The authors cannot include parotidectomy for malignant tumors in their analysis, because it will substantially bias the outcome of the primary objective of the study. In 21 patients, a radical parotidectomy was performed because of perineurial infiltration.

Even if only patients with a malignant tumor and preserved facial nerve would be included, this would still significantly influence the statistics: These patients might be elder than the standard patients with a benign lesion; most of the patients with a malignant lesion would undergo a total parotidectomy, which takes significantly longer than usual.

Detailed review of the manuscript:

The Abstract should be shortened. Especially, the first paragraph is expendable. The authors should focus on the aim of the study. The purpose of the CT- scan is unclear throughout the manuscript. Moreover, worldwide an ultrasound and the MRI are the imaging methods of choice for preoperative diagnostics for parotid tumors. Preoperative imaging is crucial for careful selection of patients for more or less extensive surgery (ECD, partial parotidectomy, total parotidectomy), and in several cases, might indicate malignancy. Either way, the presence of a CT scan will not prevent complication after parotidectomy (especially not the incidence of the facial nerve palsy).

Background/ Introduction (page 5):

Similar to the abstract, the facts in the first paragraph are very generalized. The authors should focus on the aim of their study and explain why another clarification of the risk factors is needed.

During the last years, the incidence of the facial nerve palsy during parotidectomy, especially for benign tumors, decreased substantially because of the excellent standardization of the surgical technique, use of the microscope or a loupe and a facial nerve monitoring. For this reason, the incidences which were reported in the 80-ies and 90-ies are more than outdated. The authors refer altogether to 19 studies which were published prior to 1990 throughout the manuscript. These is
excellent research in due course. However, several manuscripts with high level of evidence and meta analyses have been published since then, which present the current data more precise.

Again, the use of the preoperative CT scan is unclear to me. On lines 87 and following, the authors criticize the lack of preoperative imaging. As previously mentioned, the use of a CT scan is not standard of care for parotid tumors. Additionally, the use of the EMG facial nerve monitoring is discussed (lines 89 ff). Whereas its use is worldwide accepted, there is no statistical evidence of its additional advantage for prevention of the facial nerve palsy, if applied to optical facial nerve monitoring. The authors should clarify this.

On lines 94ff the authors claim, that the location of the tumor in reference to the facial nerve, might be predicted by preoperative imaging. To my opinion, this is only applicable for large deep lobe tumors. The true localization of a standard lesion is only visible during surgery.

Methods (pages 6ff): My major concern regards to the inclusion of the malignant tumors into the analysis, which i have previously described in greater detail.

Page 7, Lines 127-128: Was 2 or 4 channel EMG Monitoring applied?

Page 7, line 138 and following: extra-capsular dissection should be changed to extracapsular dissection (ECD).

Page 8, lines 151 and following: To my opinion, the facial dysfunction after 6 months still might improve. The authors should clarify, why the facial dysfunction after 6 months was defined as a permanent palsy. Furthermore, it would be interesting in which patents with a palsy at 6 months the palsy was permanent and still present at 12 months.

Results (pages 8ff).

The data is well presented in four tables. Accordingly, the information, especially regarding the patients characteristics is redundant. I suggest to shorten the first paragraph.

Page 9, Lines 178 following: The whole paragraph is somehow illogical and confusing. In the first sentence the number of patients with a palsy after 12 months is missing. Then, the rates for primary surgery are shown. In the following, the authors describe the increased (?) incidence of the temporary palsy after revision surgery, malignant tumors and in deep lobe tumors. It would be nice to focus on few corner points / specific data and to present it more clearly. Otherwise, it looks like random enumeration.

Page 9, Line 189: I would rename into "Degree or Extent or Evaluation of facial dysfunction"

Page 10, Lines 197 and following: For the analysis of the extent of surgery, the ECD and partial parotidectomy were merged. In several recent prospective trials could be shown, that ECD is associated with a significantly lower incidence of the facial nerve palsy compared to parotidectomy approached, which require the dissection of the facial nerve (partial, superficial parotidectomy, etc.). For this reason, i strongly suggest to separate the ECD and the partial parotidectomy for this analysis.

Lines 208-209: It is only logical, that the extent of surgery is higher in treatment of malignant tumors and in cases where the tumor exceed the superficial lobe.
Discussion (Pages 11-13): The same, as in introduction: The incidence of the facial nerve palsy during parotidectomy, especially for benign tumors, decreased substantially during the last years, the incidence of the facial nerve palsy during parotidectomy, especially for benign tumors, decreased substantially because of the excellent standardization of the surgical technique, use of the microscope or a loupe and a facial nerve monitoring. For this reason, the incidences which were reported in the 80-ies and 90-ies are more than outdated. The authors refer altogether to 19 studies which were published prior to 1990 throughout the manuscript. These is excellent research in due course. However, several meta analyses has been published since then, which present the current data more precise.

Lines 256-258: If i understand this correctly, the authors stated, that the recovery of the facial nerve is delayed and/or insufficient in female patients? This is very far-fetched. I suggest, that the authors re-analyze the influencing factors for this outcome (maybe more malignant tumors? are there any independent research which would confirm this fact?)

Lines 274 and following: "The preoperative CT allows us to estimate the positional relationship between the imaginary nerve line and the tumor before surgery". I strongly disagree with this statement. The imaging of the peripheral facial nerve using the CT scan is not as far developed. There are some high-field-MRI studies regarding this matter though. For this reason, the following statement ("the anatomical continuity of the facial nerve could be preserved because of the accurate preoperative CT") is inaccurate, too.

In general, the manuscript should be linguistically edited. The sentences are often too long and confusing. For example, lines 64-66 (Abstract), lines 232-233, 240-242.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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