

Review of: "Surgical treatment of Temporal Lobe Epilepsy: comparative results of selective amygdalohippocampectomy versus anterior temporal lobectomy from a referral center in Brazil"

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Potential competing interests: The author(s) declared that no potential competing interests exist.

This is a retrospective study of seizure outcomes following ATL vs. SAH in 132 patients with unilateral hippocampal sclerosis and concordant seizure onset.

Strengths:

- The authors have kept the sample as homogeneous as possible with regard to pre-operative clinical factors, as well as consistent surgical techniques/same surgeon over the course of 11 years. Given the variability of the data on ATL vs. SAH outcomes, sample homogeneity is highly beneficial in eliminating confounding factors that have not always been appropriately controlled on previous studies,
- The Right TLE and Left TLE groups sizes were relatively even, and the groups were well matched on many important clinical and demographic variables,
- The results are compelling and clearly described.

Weaknesses:

- The authors acknowledge that the RTLE and LTLE groups differed in the surgical approach, and provided a reasonable explanation for this. However, potential pre-surgical differences between the two groups with regard to epilepsy severity (e.g., number of seizures, number of medications, age of seizure onset, duration, etc.) are not discussed. Since, as the authors acknowledge very briefly in the Limitations, surgical outcomes in general tend to be somewhat worse following a dominant (usually left) temporal lobe resection, it is reasonable to discuss pre-operative differences that may account for the difference in outcomes.
- The Discussion section does not actually provide any additional information regarding the results. In fact, it mostly reiterates the statistical findings from the Results section in an abbreviated format. This is insufficient and completely uninformative. This section should interpret the results within the context of existing literature and provide some sort of theoretical explanation for the findings. This is not done in any way, shape or form in this manuscript.
- Further to the previous point, the data in Table 2 indicates that the follow-up period was longer for patients who underwent ATL vs. SAH. This is an extremely important and interesting finding that actually supports the authors' conclusions, and that needs to be highlighted and discussed in some detail. Aside from stating that the follow-up period for the two techniques was "long enough," there is no mention of this in the Discussion.
- Since neuropsychological and functional outcomes were not monitored in this study (or at least are not reported in any

detail), the conclusion that ATL is associated with “limited morbidity” is not supported by the data. Since the focus of the study was seizure control, this is the only outcome the authors can comment on with any confidence without going well beyond the data.

Minor concerns:

The Discussion is poorly organized. The first paragraph focuses on neuropsychological and cognitive outcomes, which were not the focus of the study!

Overall Impression: This is a clearly-written, methodologically sound study. However, some important confounding factors are not addressed, the results are not discussed appropriately and some of the conclusions are not supported by the data. Most importantly, the novelty of this study is questionable as the authors do not clarify how this would actually contribute to the existing literature on this topic.