



Neurosurgical Forum

## LETTERS TO THE EDITOR

## An extent of resection threshold for recurrent glioblastoma: why wait until recurrence?

TO THE EDITOR: We read with great interest the article by Oppenlander and colleagues,<sup>2</sup> who have reported their experience with recurrent glioblastoma in a series of 170 patients treated first by reoperation (Oppenlander ME, Wolf AB, Snyder LA, et al: An extent of resection threshold for recurrent glioblastoma and its risk for neurological morbidity. *J Neurosurg* 120:846–853, April 2014). The authors have attempted to quantify the effect of the extent of resection (EOR) on survival in patients with recurrent glioblastoma, and to define a possible threshold for tumor cytoreduction. The authors should be complimented for reporting such a large and reliable group of data on resection strategy, especially when we consider that repeat resection remains an increasingly common consideration for both patients and the neuro-oncological community.

Nowadays, determining the best treatment of glioblastoma at recurrence represents a real clinical challenge. Reoperation is one of many other treatment possibilities.<sup>1</sup> Reoperation for glioblastoma, especially when a gross-total resection seems feasible, remains increasingly of interest to our group at Tirana University Hospital. The actuarial literature has provided incomplete and controversial information on this issue. However, almost all authors agree that gross-total resection has real potential clinical and survival benefits. How can we increase the number of patients undergoing a larger EOR?

Interestingly, despite the latter assumption, the second resection is always offered when new tumor growth is radiologically evident. If a gross-total resection is feasible at glioblastoma recurrence, it would surely be feasible at the time of maximal response just before tumor progression. Thus, why wait until recurrence? This was our working hypothesis. First, we performed a re-resection in 11 patients at the time of the best response after second-line bevacizumab-based chemotherapy. The median overall survival was 20.6 months. These encouraging results prompted us to propose maximal resection in 4 patients at the time of maximal response after concomitant radiochemotherapy. In all cases, a more than 90% EOR was achieved, correlating with an improved progression-free survival and a median overall survival of 24.8 months.

Despite criticism for selection bias and the retrospective design of our study, our findings compared well with those of Oppenlander and colleagues in advocating an aggressive strategy whenever possible, regardless of the

status of our patients, whether at recurrence or at greatest tumor shrinking.

To conclude, we think that a second resection should always be considered when the response after first-line radiochemotherapy for glioblastoma may help optimize the extent of surgical removal, even for tumors initially considered unresectable. Why wait until recurrence to perform that operation? In our opinion, the timing of re-resection could considerably modify the natural course of the disease as well as patient survival. Certainly, a prospective study with a long-term follow-up and larger population is required to better define the impact of reoperation at the right moment.

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### Disclosures

The authors report no conflict of interest.

### Response

We thank the authors for their interest and kind words regarding our publication. Although we agree that early intervention for recurrent glioblastoma could be of interest in select cases, it remains unclear how such cases could be appropriately selected during adjuvant therapy at the “time of maximal response.” For many patients there is minimal demonstrable enhancement on MR images in this interval. We look forward to reading the authors’ report detailing their interesting hypothesis and first-hand experience.

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## Infraorbital nerve as a surgical landmark

TO THE EDITOR: I read with interest the recent anatomical study by Ehladi et al.<sup>3</sup> emphasizing the importance of the infraorbital nerve as a guide to the pterygopalatine fossa and cavernous sinus via the foramen rotundum (Elhadi AM, Zaidi HA, Yagmurlu K, et al: Infraorbital nerve: a surgically relevant landmark for the pterygopalatine fossa, cavernous sinus, and anterolateral skull base in endoscopic transmaxillary approaches. *J Neurosurg* 125:1460–1468, December 2016). Some 20 years ago, we described the transmaxillary approach to the cavernous sinus<sup>2</sup> and later extended it to the infratemporal fossa<sup>5</sup> and combined the approach with the transsphenoidal.<sup>4</sup> A maxillotomy is performed, and the course of the infraorbital nerve is identified as a guide to the maxillary branch of the trigeminal nerve. After an osteotomy of the posterior sinus wall and pterygoid plate, the foramen rotundum is identified, which lies at a mean of 10 mm from the posterior wall of the maxilla. A superomedial enlargement of the foramen rotundum is then undertaken to ultimately expose the anterior cavernous sinus. The anterior loop of the carotid is on average 38 mm from the maxilla opening—this, in fact, is a short operative distance to the cavernous sinus, being shorter than the transnasal approach and even the more recently described lateral orbitotomy approach.<sup>1</sup> It can be performed with either endoscopic or microscopic visualization. The segments of the approach as described by Elhadi et al.<sup>3</sup> are similar to the stages defined in our paper.<sup>2</sup>

While these approaches were described agnostic to the method of visualization (endoscopic or microscopic), the anatomy is important and relevant. In rare cases, the infraorbital nerve may not be well visualized in the roof of the maxillary sinus; in such instances, image guidance is used to locate the foramen rotundum in the back wall of the pterygopalatine fossa. Alternatively, the nerve can be traced and drilled from the infraorbital foramen, although this poses some trauma risk to the nerve in its exposure.

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### Disclosures

The author reports no conflict of interest.

### Response

We value the comments of Dr. Couldwell regarding the description of the transmaxillary approach to the cavernous sinus.<sup>1</sup> As he noted, the primary advantage of this approach is that it provides a direct route to the anterior loop of the internal carotid artery. We believe that our work supplements his description as it provides additional detailed anatomical figures and photographs and surgically relevant landmarks (illustrated with a surgical case) that should prove particularly useful in the endoscopic era. We regret not citing Dr. Couldwell's foundational work in our paper.

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