




## Reply to: Letter to the Editor Regarding Anesthesia Management for Low-Grade Glioma Awake Surgery: A European Low-Grade Glioma Network Survey

Catarina Madadaki<sup>1</sup> · Sylvie Aubrun<sup>1</sup> · Lorenzo Bello<sup>2</sup> · Hugues Duffau<sup>3,4</sup> · Emmanuel Mandonnet<sup>5,6,7</sup>  · ELGGN

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To the Editor:

First of all, we would like to thank Picart et al. [6] for their interest in our work.

This work was indeed motivated by the necessity to get a better picture of the anesthetic practice for awake craniotomy, which is considered now the gold standard for DLGG resection.

We agree that one of the main limitations of our study is the delay between its completion and the publication of the results, about a 5-year period. Picart et al. specifically questioned the validity of our results regarding the limited use of hypnosis-aided awake surgery (HAS), i.e., whether the time lag could have resulted in underestimating the rate of the use of hypnosis. In order to address this limitation, we decided to resend a questionnaire by e-mail to the centers contacted during our initial survey. To achieve timely return of the survey in context of the COVID-19 pandemic, with the consequences of which many of our members are directly involved, we chose to be pragmatic

and asked only one question: “Do you include hypnosis in your awake surgery anesthetic protocol?”. We had 100% response rate (30 centers from 10 countries) and all answers remained the same: none of them but one (Lariboisière Hospital) practice hypnosis and none of them plan to introduce HAS techniques in a near future. Even internationally recognized teams with very high volume of cases reported in the survey that they do not feel the need to introduce HAS, considering their satisfactory experience without [2, 3]. As for any other technique or treatment, a convincing benefit of HAS in the context of awake neurosurgery should be demonstrated, before it could be more widely adopted. Up to now, in spite of the preliminary results demonstrated in the recent publications [4, 5, 7] mentioned by Picart et al., the true value of HAS remains undetermined. Potential benefits would include lower doses of propofol and remifentanyl, lower levels of hypercapnia, and higher hemodynamic stability during the opening period, as well as shorter time for awaking. Should such benefits be objectified, it has also to be proven that HAS does not increase the rate of adverse effect (desaturation, brain swelling, nausea or vomiting, seizures, interference with cognitive monitoring). Assuming that all these points would have been addressed, the criteria for selecting patients, in whom we would anticipate a good response to hypnosis, should be accurately established (for example, in Zemmoura et al. [7], the hypnosedation procedure failed in 6 patients out of 37, requiring a conversion to asleep-awake-asleep technique). Last but not the least, the technique requires a dedicated training, which is time-consuming, thus limiting the possibility to train all members of the anesthesia team.

Finally, on the patient’s side, it remains to be demonstrated that HAS could improve the already excellent tolerability of awake surgery in patients undergoing dedicated preparation by neurosurgeons or nursing staff without any HAS [1].

We support, are open to, and are actively involved in developing novel techniques aimed at improving peri- and post-operative management of our patients. To this end, we are looking forward to seeing more reports on prospective series

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✉ Emmanuel Mandonnet  
mandonnet@mac.com

<sup>1</sup> Département d’Anesthésie-Réanimation, Hôpital Lariboisière, Paris, France

<sup>2</sup> Dipartimento di Oncologia ed Emato-Oncologia, Università degli Studi di Milano, Milan, Italy

<sup>3</sup> Hôpital Gui de Chauliac, Montpellier, France

<sup>4</sup> Université de Montpellier, Montpellier, France

<sup>5</sup> Université de Paris, Paris, France

<sup>6</sup> Frontlab, CNRS UMR 7225, INSERM U1127, Institut du Cerveau et de la Moelle, Paris, France

<sup>7</sup> Département de Neurochirurgie, Hôpital Lariboisière, 2 rue Ambroise Paré, 75010 Paris, France

about HAS with a thorough analysis of the relevant endpoints, which are—apart from the abovementioned intraoperative factors—extent of resection, cognitive outcomes, and patients' satisfaction.

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