Neurosurgical Forum LETTERS TO THE EDITOR

Intraoperative analgesic regimens and surgical duration after spine surgery

TO THE EDITOR: We read with great interest the article of O'Connell et al.⁵ (O'Connell C, Azad TD, Mittal V, et al: Preoperative depression, lumbar fusion, and opioid use: an assessment of postoperative prescription, quality, and economic outcomes. *Neurosurg Focus* 44(1):E5, January 2018), who performed a retrospective study on 60,597 patients who had undergone lumbar fusion and concluded that a preoperative diagnosis of depression was associated with increased opioid use following lumbar fusion. The authors should be congratulated for performing a study in an important topic (opioid use) in patients undergoing spine surgery.^{1,3} The current emphasis on the need to use multimodal agents to improve postoperative pain control and reduce opioid consumption makes the topic very relevant in perioperative medicine.^{4,6}

The study of O'Connell et al. was well designed and conducted, but there are concerns that need to be clarified in order to further confirm the authors' findings. First, it is unclear whether the authors utilized a standardized intraoperative analgesic regimen, and this can significantly affect the primary outcomes. Second, we would have liked to have seen the duration of surgical operations in their analysis, as surgical duration has been independently associated with adverse outcomes after spine surgery.² Last, the authors do not describe whether the patients with the diagnosis of depression were compliant with depression medications, and this issue of compliance is likely more important than the diagnosis of depression.

We would welcome comments by the authors as this would further support the findings of this important study.

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Disclosures

The authors report no conflict of interest.

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Response

We thank Drs. Kendall and Alves for their interest in our study examining the correlation between preoperative depression and opioid use following lumbar fusion. We appreciate their thoughtful comments and agree that this is an important topic that deserves further research.

To address the questions brought up by Drs. Kendall and Alves, we would like to emphasize that this was an observational study using administrative data. While consistent application of a standardized intraoperative analgesic regimen across all 60,597 patients would have been useful for our study, this is not done in practice, and the administrative and observational nature of the database utilized in the present study made implementing a standardized operative regimen impossible. The data included only inpatient and outpatient administrative information (i.e., diagnosis codes, procedure codes, demographic information, and outpatient prescription information), and the details of all medications administered during each patient's full

operative course were not available for analysis. However, intraoperative medication choice and quantity would be more likely correlated with preoperative opioid use (which we controlled for) than with preoperative depression diagnosis, making it an unlikely confounder.

Similarly, the duration of operation was not recorded in our database. By controlling for procedure characteristics (single-level vs multilevel fusion, single-level vs multilevel laminectomy, instrumentation), as well as a variety of different comorbidities, we attempted to control for the medical complexity of each case. However, we readily acknowledge that this is an imperfect way of controlling for the severity and complexity of each surgical case, which is why we advocate for further studies utilizing more fine-grained nonadministrative data in order to characterize the proposed relationship in greater detail. While a nonretrospective trial (i.e., a randomized controlled trial) might go even further to eliminate the possibility of confounding by either intraoperative medication regimen or operation duration, randomization is clearly not possible (or ethical) when studying the effects of an exposure like depression. Similarly, it is not possible to randomize patients to surgery, and so we are left with retrospective trials and their inherent imperfections.

As for compliance with medications, we did not include compliance with depression medication regimens in the model for a variety of reasons. First, only pharmacotherapy would have been consistently recorded by MarketScan, while any therapy not covered by insurance (for example, psychotherapy) would not be consistently recorded. Sec-

ond, we believe that depression *severity* is the key issue here, not necessarily depression treatment status, and although treatment can often lead to reductions in severity, simply recording whether or not a patient is taking antidepressants is a poor metric of symptom severity. Given both its inconsistent recording in our data set and its poor correlation with our variable of interest (depression severity), we chose to omit treatment of depression from the model. However, we agree that future studies should determine whether treatment of depression, either pharmacological or nonpharmacological, can modify its effect on opioid consumption following surgery.

In summary, we thank Drs. Kendall and Alves for their interest in our study and greatly appreciate their comments. We agree that operative analgesia, duration of operation, and treatment of depression may modify the link between preoperative depression and postoperative opioid use, and we advocate for future studies examining their role.

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