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We would like to thank Schwenk and colleagues for their interest and comments related to our paper.¹ However, we are concerned that the objections raised are misguided, given the hypothesis being tested and the methods employed in our study.

In their letter, the authors state that it is only in combination with other multimodal analgesics that epidural analgesia would decrease the risk of persistent opioid use. Because we did not capture whether multimodal anesthesia was used, they argue, the entire premise of our study was flawed. While it is perhaps an interesting hypothesis that epidurals only decrease persistent opioid use when used in conjunction with other modalities, it is pure speculation. It does not make our study which tested the hypothesis that epidurals decrease persistent opioid use “flawed”. Epidurals are likely often used in conjunction with other analgesics (such as acetaminophen or non-steroidal anti-inflammatory drugs²) and if this combination were to decrease the risk of persistent opioid use then there should have been some signal of benefit for epidurals (which, unfortunately, there was not).

The authors go on to suggest that the reason we did not observe a benefit for epidurals in decreasing persistent opioid use was because we did not obtain details regarding epidural placement location, timing, duration or medication used. Indeed, we did not capture these details in our dataset and the exposure studied should be interpreted as epidural placement and management as it is routinely performed in a large, nationwide sample of hospitals. While perhaps there is a particular “location” or “medication” that would result in epidural anesthesia decreasing persistent opioid use, this is again pure speculation on the part of the letter's authors.

The last issue raised was that the outcome chosen was “unusual”. While the argument provided by the authors is difficult to follow, we would point out there is no widely agreed upon definition of persistent opioid use. Our primary definition of lack of opioid filling for 30 days is a clinically reasonable definition for discontinuing opioids after surgery that can

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be operationalized in healthcare utilization data. We also included several sensitivity analyses where the outcome definition of discontinuation was varied to ensure that our results were robust—in none of these did epidural analgesia confer a protective effect against persistent use. Further, we would argue that the use of survival techniques to longitudinally analyze prescriptions represents a significant advancement in statistical methods when compared to the prior literature.

The authors state that our study is “flawed” and does not advance the understanding of the long-term benefits of epidural analgesia, but they have not offered a single cogent argument as to why this is the case. Our study benefited from a large sample size, careful study design, control for a very extensive list of confounders using state of the art epidemiological methods based in propensity scores, and multiple sensitivity analyses to test the robustness of our findings. There are certainly many benefits associated with epidural analgesia following abdominal surgery, but our data show that prevention of persistent opioid use is not one of them.

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