In Reply: Steroid Use Associated With Increased Odds of 30-Day Mortality in Surgical Patients with Metastatic Spinal Tumors in the Setting of Disseminated Disease

To the Editor:

The authors would like to thank the editors for the opportunity to further discuss the importance of our study, “Steroid Use Associated With Increased Odds of 30-Day Mortality in Surgical Patients With Metastatic Spinal Tumors in the Setting of Disseminated Disease”.1 We appreciate the rigorous critique of our paper outlined in the letter to the editor,2 as we always champion rigorous debate and scientific discourse, and are glad to address the concerns raised therein. The critiques raised actually highlight the importance and value of our study. Most striking is our specialty’s continued reliance on studies with non-surgical cohorts, from which we externally generalize the results and make decisions regarding the utility of steroids for our surgical patients.3-8 Our study, limited to patients having received surgical intervention as a part of the treatment of their disease, provides an important insight into how steroids may affect this patient population.

The critique specifically discusses a review article by expert spinal surgeons, which cites a study by Sorenson et al9 regarding the effect of steroid use on 1-yr ambulatory status in patients with metastatic spinal cord compression. However, the review by the expert spinal surgeons,9 and thus the critique which propagates the information provided in the review, with all due respect, are inaccurate. The review, authored by Kumar et al9 quotes the Sorenson study as follows: “…Sorensen and colleagues, where the use of high-dose dexamethasone (96 mg/d) resulted in a statistically greater percentage of patients surviving with ambulatory function at 1 year”. The critique promulgates the inaccurate information further by stating, “Sorensen et al. published almost 25 years ago a single-blinded randomized trial comparing high dose dexamethasone (96 mg IV bolus followed by 24 mg every six hours tapered over 10 days) plus radiation to no steroids plus radiation in patients with MSCC, finding significantly higher rates of ambulation at 1 year and yet no increase in mortality in favor of steroids”.9 However, if you read the original manuscript, you will see that the 1-yr survival with ambulatory function was highly statistically insignificant between the 2 groups (steroid group n = 8 (30%), non-steroid group n = 6 (20%), chi-squared = 0.71, P = .4).7 The critique propagates the misunderstanding purported by the review by Kumar et al9 and thus provides no support for the letter’s argument.

Additionally, we would hesitate to use the phrase “gold standard” for a medication for which a recent Cochran review10 found there to be no evidence to suggest there is any difference between the actual usage of steroids compared to no steroids on outcomes in metastatic spinal cord compression patients. While it may be considered common practice, and indeed it is common practice at the authors’ institutions to use steroids in patients presenting with neurological symptoms, the phrase “gold standard” does not necessarily apply in this instance where there is no actual strong data of their benefit.

Regarding the concern of the steroid variable itself brought up by the letter,2 we adequately discussed the limitations of this variable in the manuscript and in the supplemental material,1 highlighting the risk of misclassification of exposure bias, and the lack of specific details regarding indication, dose, and length of administration. This hardly represents a fundamental flaw nor does it impart irrelevance to our study. There is precedent in the literature, including Neurosurgery, regarding this variable representing steroid use.11 Additionally, the claim that this variable may represent patients which have been on steroids for other diseases, is just as likely to be true as the converse, that all of these patients captured by steroid variable are receiving corticosteroids for the exact purpose we’re discussing, metastatic spinal cord compression, particularly since our cohort consists of surgical patients only.

Furthermore, we never implied nor suggested physicians should not use steroids. Steroids may have a very important role in the management of patients with spinal metastasis presenting with neurological symptoms due to cord compression. We were very cautious not to overstate the conclusions which can be drawn from this study design. We stated there was a significant association identified between steroid use and mortality in this particular setting and this particular patient type which warrants further investigation. We never stated, nor eluded to the letter’s claim that our conclusion “suggests that administration of steroids upon initial presentation of motor weakness in patients with MSCC increases the risk of mortality”. Our study was not designed, nor intended to confirm or deny this claim. It is out of place regarding our findings and representation of the data thereof. One should refrain from overstating conclusions in a negative light in order to marginalize or discredit a study’s results.

We have identified a significant association between steroid use and mortality in the data, which warrants further investigation in a controlled setting. Only then should judgment be passed. Additionally, we have identified a critical knowledge gap (steroid use in surgically managed patients) that should be addressed in our field given how frequently and how commonly this medication is prescribed to our patients.

We again thank the authors for their interest in our article and the editors of Neurosurgery for this opportunity to discuss our research.

Disclosures

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REFERENCES


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