Neurosurgical Forum
LETTERS TO THE EDITOR

The effect of methylene blue on postoperative low-back pain

TO THE EDITOR: I read with interest the article by Farrokhi et al.³ (Farrokhi MR, Lotfi M, Masoudi MS, et al: Effects of methylene blue on postoperative low-back pain and functional outcomes after lumbar open discectomy: a triple-blind, randomized placebo-controlled trial. *J Neurosurg Spine* 24:7–15, January 2016). In this study the researchers have tried to investigate the impact of methylene blue (MB) on reducing postoperative low-back pain and improving quality of life. The research was technically and methodologically well designed, the data were collected with certainty and blindly, and the investigators analyzed and discussed the results accordingly. In their paper, one might have looked for the subheading on the study's limitations; however, it was missing. This section might have included mention of the following: 1) Preoperative American Society of Anesthesiologists (ASA) grading and the analgesic/narcotic utility of the cases are not identified.^{1,2,6} 2) Functional, psychological, and quantitative sensory testing has not been assessed in the included cases, nor has medical management of pain, depression, and anxiety in the preoperative period been adjusted. 3) The main reason for using painkillers either locally during surgery or systemically is to optimize postoperative pain management. After surgery, utilizing different types of painkillers that are available to everyone at home, especially in the Iranian community, is done habitually. It can be done independent of the accuracy of the self-reported visual analog scale and Oswestry Disability Index and can violate interpretation of the data remarkably. This item is not clarified enough in the questionnaires, neither in the original nor in the Persian translated form, and can affect the power of the outcome variants notably. 4) It would have been better if the patients were checked using a kind of Analgesic Prescription Monitoring Program established and checked both pre- and postoperatively to increase the accuracy of data collection in such a randomized controlled trial (RCT) on pain control.² 5) It would have been better if the statistical analysis was done using Poisson regression with robust error variance, both without adjustments (bivariate analysis) and then with adjustment (univariate analysis) for each characteristic. This could have elucidated whether the impact of the intervention was due

to a confounder. 6) Local administration of methylprednisolone to the dural sac and surrounding tissues was and still is part of the disc surgery procedure performed by many surgeons around the world, especially when the surgeon has decompressed a tight segmental narrowing. The concept behind it is to reduce local inflammation caused either by the disc disease or the manipulations during surgery. This can decrease postoperative pain and scar formation according to Pospiech et al., who produced epidural scars via laminectomies at 3 different levels in 30 dogs, thus yielding 90 operative segments for study of various substances that might reduce cicatrix.⁵ They applied 10 mg of liquid triamcinolone to 18 of these segments, which were examined histologically. Significant scarring was demonstrated in 7 of 12 segments examined between 1 week and 3 months compared with 12 of 13 segments in the control group. Heavy cicatrix was found in only 1 of 6 steroid-treated segments examined at 6 months and in 4 of 5 controls.⁴

Using MB during disc surgery is a novel intervention, and the RCT performed by Farrokhi et al. could be a milestone in elucidating its safety. To prove the efficacy and efficiency of MB to optimize postoperative pain control and to decrease the amount of usual analgesics used in different centers, a multicentric well-adjusted case-control study including enough cases may answer the question correctly.

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Disclosures

The author reports no conflict of interest.

Response

We thank Prof. Amirjamshidi for his careful review of our article and for his thoughtful comments. Although the references cited in this response are rather old, in the following text we clarify and explain the raised ambiguities.

The limitations of using MB have been stated in our *Methods*, including unintended durotomies, that is, unintentional opening of the dura during the operation, which should not be used. In addition, the exclusion criteria have been mentioned in our study.

It is not necessary to use preoperative ASA grading in conducting such studies because it does not have a direct role in the measurement of outcomes. In our study, demographic characteristics such as age, sex, smoking, taking medications, and other variables have been shown in Table 2. In addition, the study conducted by Sadrolsadat et al.,⁴ which is mentioned in the author's letter, is not relevant to our study or to the evaluation of postoperative pain. Sadrolsadat et al.⁴ evaluated the ASA system and compared 2 techniques of anesthesia, but it is clearly evident that the ASA scale is used in all anesthetic techniques as a standard, scientific, and legal grading system to determine anesthetic outcomes in patients, but it has no relationship to the measurement of postoperative pain.

Because pain is an ordinal variable and not a count variable, Poisson regression was not used in our study. In addition, since our sample size was large, instead of a nonparametric method for our ordinal variable, the use of a parametric method is recommended. Moreover, as random allocation was done and the 2 groups were similar, adjustment for each characteristic was not necessary.

The use of painkillers after surgery by the patients at home can be common in every society or country and is not specific to Iran. To our knowledge, there is no comparative study in the literature that has evaluated the habitual

use of painkillers after surgery by the patients at home in Iran and has compared and contrasted it with the other countries, but it is a fact that the painkillers may be available to the patients of all countries. Moreover, the 2 groups had the possibility to use painkillers; therefore, bias may not have affected the results of our study.

The cited study by Nelson and Landau² is a review article on the use of intraspinal steroid injections but not during the surgery. However, they concluded that 1) intraspinal steroid therapy is not effective for back pain or radicular syndromes, 2) epidural steroid infusion may result in increased pain, and 3) patients should be informed that there is no evidence that epidural steroid injections provide permanent relief of pain. There is no study or evidence in the literature that can prove the efficacy of steroids on the reduction of pain during laminectomy.

Our study shows that MB (1 ml at a concentration of 0.5%) reduces postoperative pain, which has also been documented by other clinical and experimental studies.^{1,3} Anyway, we suggest that future studies may be warranted to further compare the efficacy of MB and other agents in the reduction of postoperative pain after laminectomy.

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