Nerve Blocks and Length of Stay?

To the Editor:

McIsaac *et al.*[1] recently published their population-based cohort study on outcomes after total knee arthroplasty in relation to the use of peripheral nerve blocks. The primary outcome was length of stay (LOS), and they concluded that nerve blocks reduced LOS (risk ratio = 0.98!).

Although such large cohort studies may be valuable, we find the discussion insufficient in relation to the primary outcome, where we get no information on why the patients were hospitalized or whether a type of fast-track care was implemented.[2] Furthermore, there is no information about discharge destination, which we know from several studies may hinder sufficient interpretation of LOS, because transfer of patients to rehabilitation or other institutions may depend on potential economic benefit[3] or on local traditions[4] and may misleadingly reduce the registered LOS after surgery.[3] Finally, their mean LOS was approximately 4.7 days, which is beyond what has been published before (but not referred to) from prospective multicenter studies with a mean LOS of 3.0 days[5] from well-defined fast-track programs without the use of peripheral blockades. Also, median values of LOS of approximately two days in subsequent large cohorts are available.[6]

In summary, when discussing LOS as a primary outcome, interventional studies in perioperative medicine need to include data on why the patient was hospitalized, as well as discharge destination.[4]

Competing Interests

The authors declare no competing interests.

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Effect of Peripheral Nerve Block on Length of Stay after Total Knee Arthroplasty

To the Editor:

We read the article by McIsaac *et al.*[1] with great interest. The authors should be commended for attempting to estimate the effects of peripheral nerve blocks (PNBs) on healthcare resource use. These efforts could decrease the cost of health care without compromising patient health. However, we have a few points that we wish to pose to the authors, which may confound interpretation of the results.

First, PNBs are widely used to reduce pain after total knee arthroplasty (TKA). However, these techniques have shortcomings, such as inadequate pain control due to technical difficulty and inexperience. Multimodal analgesia has been introduced to overcome these shortcomings.[2] The pain score is important to determine whether a nerve block is successful, but this retrospective design made it impossible to include pain scores.

Second, factors contributing to length of stay after TKA include preoperative, intraoperative, and postoperative variables. Elderly patients are more prone to postoperative complications. It is well documented that length of stay is associated with postoperative complications, such as cardiovascular complications, mechanical wounds, and infections.[3] These variables may affect the results. However, these variables are not included in the analysis.

Third, the use of propensity score methods has increased significantly in recent years to evaluate treatment effects using observational data. These methods allow observational studies to be designed similar to randomized experiments. Four methods of using the propensity score have been described in the statistical literature, including matching, stratification, covariate adjustment, and weighting (inverse probability of treatment weighting; IPTW). It has been suggested that the last two methods directly estimate the effect of treatment, whereas the first two methods only group subjects rather than estimate the effect of treatment. Therefore, the latter two methods may be more sensitive to misspecification of the propensity score model than

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the first two methods.[4] It would better to use the IPTW method to estimate treatment effects of PNB. Moreover, selecting similar propensity scores during matching allows the high and low propensity scores to be discarded. We are concerned that this portion will not represent all patients who have undergone TKA. The IPTW method would solve this problem.

Competing Interests

The authors declare no competing interests.

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In Reply:

We thank Drs. Hwang and Jeon and Drs. Kehlet and Jørgensen for their letters and welcome the opportunity to discuss the strengths and limitations of our study.[1]

As stated in the letter from Drs. Hwang and Jeon and acknowledged in our article,[1] we were unable to identify whether each nerve block studied was actually clinically effective. When considered from the perspective of an explanatory research question, this is clearly a limitation. However, because the aim of our study was comparative effectiveness, our specific objective was in the realm of pragmatic research, that is, how effective and generalizable might the intervention be in real-world practice.[2] From this perspective, we hope that our measures of association provide useful insights into the impact that the peripheral nerve blocks have on system outcomes across a generalizable large sample of patients across an entire healthcare system.

With respect to the assertion by Drs. Hwang and Jeon that our lack of control for intraoperative and postoperative variables and complications is a limitation, we would argue the contrary. In observational comparative effectiveness research, efforts must be made to adjust for indication bias and confounding bias (among other sources). When selecting variables that may be confounders, one must ensure that they meet the definition of a confounder, specifically that they differentially impact exposure (*i.e.*, receipt of a block), differentially impact outcome, and are not on the causal pathway.[3] Therefore, although complications may contribute to differences in length of stay (LOS), they are not true confounders because they occur after exposure and are likely on the causal pathway to prolonged LOS. Furthermore, it has been shown that control for variables such as these that are not true confounders can lead to spurious associations.[4]

Finally, we agree with Drs. Hwang and Jeon that the choice of analytic approach when performing propensity score–based analyses impacts interpretation of study results.[5] Specifically, matched analyses such as ours estimate the average treatment effect in the treated (ATT), because some individuals are excluded if they received treatment but no adequate match was available or if they were untreated and again went unmatched to a treated subject. Although this may decrease overall generalizability, it may also decrease bias. In contrast, methods such as inverse probability of treatment weighting (IPTW) or regression analysis provide an average treatment effect (ATE), that is, what might happen if the entire population was shifted from untreated to treated.[6] Although the ATT and ATE are typically similar in direction and magnitude, this is not always the case. In fact, in the case of IPTW, including individuals who were treated despite a very low propensity for treatment can greatly over-weight their contribution to the analysis, especially if extreme tails of the distribution are not trimmed.[5] Furthermore, matched analyses can provide an estimate of the absolute risk difference, as opposed to IPTW and regression-based approaches that are typically limited to estimating relative outcome differences. Lastly, in our sensitivity analysis we used a regression-based multilevel multivariable regression analysis, which estimated an ATE for single shot blocks that was identical in direction and magnitude to the ATT estimated from the propensity score–matched analysis.

We would also like to thank Drs. Kehlet and Jørgensen for their commentary regarding our publication[1] and in particular their interest in promoting improvements in reporting, analysis, and overall research efforts related to LOS. First, we agree that different patterns of care between jurisdictions or individual hospitals can skew LOS findings. As Hart *et al.*[7] outlined in an analysis of Canadian *versus* American total joint arthroplasty outcomes, LOS in Canadian hospitals tends to be approximately 1.3 to 1.4 days longer, a finding that may be attributable to a 21 to 27% increase in rates of discharge to short-term rehabilitation from American hospitals. Data from Hart *et al.*[7] also suggest a mean LOS after joint replacement in Canadian hospitals of slightly more than four days, a figure consistent with mean LOS reported in our study, which included a larger cross-section of hospitals than would have been included in the National Surgical Quality Improvement Program data file.