join the call for establishing consistent cutoff criteria when a classification is deemed necessary, especially for the same measurement tool.

Ong *et al.*[1] provide examples of discrepancies in the estimates of burnout prevalence, specifically with the use of the abbreviated Maslach Burnout Inventory. They note that de Oliveira *et al.*[8] estimated 41% of anesthesiology residents to be at high risk for burnout in 2013, and our study reported an estimate of 51% among anesthesiology residents and first-year residency graduates from 2013 to 2016.[2] We suspect that different compositions of subgroups and the timing of the studies contributed to the difference in the estimates, although both demonstrate alarmingly high rates. Lim *et al.*[9] reported strikingly different estimates of burnout prevalence among the same group of anesthesiology residents in Singapore when different cutoff criteria were applied—22.4% based on Maslach Burnout Inventory–Human Services Survey and 62.1% based on its abbreviated version. Had the same Maslach-recommended criteria been applied, however, the prevalence of burnout in Lim *et al.*'s study would be estimated at 20.7% based on the abbreviated Maslach Burnout Inventory, which would be close to the 22.4% identified based on the full scale.[9] In addition, the correlation coefficients for the three subscales ranged from 0.92 to 0.96 between the two versions. We argue that Lim *et al.*'s study actually provides some assurance that the abbreviated version offers a reasonable alternative for brevity. Regarding the prevalence of 51% of burnout among U.S. anesthesiology residents[2] *versus* 22% among Singapore anesthesiology residents,[9] the limited generalizability of conclusions in the latter study due to small sample size (N = 58) and imbalance of males (N = 17) and females (N = 41) must be recognized. We also suggest that there are a multitude of sociocultural factors that might impact burnout beyond language and training system.

In summary, we concur with Ong *et al.* that burnout could be better defined, more precisely characterized and measured, and compared with more consistency. Nonetheless, we also want to acknowledge that progress in burnout characterization and assessment has been made since it was first described in the 1970s, and we welcome a continuation of the discussion about its relevance to anesthesiologists.

Competing Interests

Drs. Sun and Zhou are staff members of the American Board of Anesthesiology (ABA); Drs. Culley, Keegan, Macario, and Warner are ABA Directors and receive a stipend for their participation in ABA activities.

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Pectoralis-II Myofascial Block and Analgesia: Comment

To the Editor:

We read with great interest the article by Hussain *et al.*, "Pectoralis-II Myofascial Block and Analgesia in Breast Cancer Surgery: A Systematic Review and Meta-analysis."[1]

The authors sought to evaluate the Pectoralis-II block regarding its analgesic properties for breast cancer surgery when compared with control or with paravertebral blockade. To this purpose, pain severity scores in the first 24 h and cumulative 24-h analgesic consumption were chosen as coprimary outcomes. The authors identified a total of 14 clinical trials as eligible to be included in their meta-analysis.

For the validity of any meta-analysis, the quality and integrity of the data provided in the selected clinical trials is of crucial importance. Unfortunately, due to multiple most likely clerical errors, it is extremely difficult for the critical reader to verify quality and integrity of the data that were included in this meta-analysis.

Whereas the authors report in the Results section of their article that a trial by Naja *et al.*[2] was selected for inclusion in the meta-analysis, this article is not listed in table 1 (Characteristics of Included Studies) and is also not listed among the 14 studies that report pain severity scores in the first 24 h. Instead, a study by Versyck *et al.*[3] appears in the table and also among the 14 studies listed to report pain severity scores but is not mentioned among the studies included.

Among the studies listed to report on cumulative 24-h opioid consumption, another study (Lykoudi *et al.*,[4] European Society of Regional Anesthesia Abstract 2016 – coauthor Stavropoulou E is omitted in the References section) appears without being mentioned previously as being included in the meta-analysis and without being listed in table 1. On the other hand, an abstract by Kanitkar *et al.*,[5] which is mentioned as being selected for this meta-analysis and does in fact report on cumulative opioid consumption, is not among the studies listed to report on 24-h opioid consumption.

Furthermore, a study by Syal and Chandel,[6] which is mentioned as being selected for the meta-analysis, listed in table 1, and listed among the studies reporting on cumulative 24-h opioid consumption, does not actually report cumulative 24-h opioid consumption.

Besides these methodologic issues that may or may not have impacted the results of this meta-analysis, we raise another concern:

The Editor's Perspective which accompanies this article gives the impression that this meta-analysis includes 14 randomized, controlled trials that compare Pectoralis-II blocks with paravertebral blocks and finds Pectoralis-II blocks to be noninferior. In reality, only five of the 14 trials investigate these two techniques head to head. A review of these five trials demonstrates that paravertebral blocks were performed only as single level injections in all patients who received this technique. Since previous research clearly indicates that single-level paravertebral blocks only provide unpredictable and unreliable analgesia,[7,8] many practitioners perform paravertebral blocks as multiple-level injection techniques. Furthermore, if one excludes nonindexed articles and abstracts from the comparison between Pectoralis-II blocks and paravertebral blocks, the meta-analysis is reduced to 41 *versus* 42 patients in total for each technique, respectively.

Consequently, we feel strongly that no conclusions can be drawn regarding the noninferiority of Pectoralis-II blocks when compared with paravertebral blocks from the data presented in this meta-analysis.

Competing Interests

Dr. Gebhard has received research grants from Hikma Pharmaceuticals (formerly Westward; Eatontown, New Jersey) Purdue (Stamford, Connecticut), and Avanos (Alpharetta, Georgia), as well as honoraria for consulting from Pacira (Parsippany, New Jersey), Milestone Scientific (Roseland, New Jersey) and Recro Pharma (Malvern, Pennsylvania), and Acacia (Indianapolis, Indiana). The other authors declare no competing interests.

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Pectoralis-II Myofascial Block and Analgesia: Reply

In Reply:

We received a commentary by Gebhard *et al.*[1] on our systematic review and meta-analysis of the benefits of Pectoralis-II fascial block for breast cancer surgery.[2] In their comments, the authors claim to have found it "extremely difficult to verify the quality and integrity of the data."

This difficulty is partly based on the assumption that every outcome reported to be assessed in table 1 (Characteristics of Included Studies) should also appear in the Results section. Unfortunately, this assumption is not true, and the involved scenarios are numerous. For example, included trials (1) seldom assess an outcome without explicitly reporting its numerical results; or (2) do not report results of an outcome in a format that permits data extraction; or (3) simply state that the two groups were not different for an outcome, without providing further explanation. While we routinely contact authors of such trials seeking additional details, and often obtain valuable input, having studies with outcomes that were assessed but that were not included in the quantitative analysis is sometimes inevitable. In contrast, some outcomes (*e.g.*, opioid consumption) may not be explicitly reported as an outcome, but it can still be deduced from the description of the analgesic regimen and hence included in the quantitative analysis. Authors of meta-analyses know that the quality and integrity of data are not affected by such scenarios.

Furthermore, Gebhard *et al.* claim that the noninferiority conclusion is based on a comparison involving 83 patients only. Again, this claim is not justified because it is based on selective exclusion of four clinical trials that had met the criteria for inclusion in this review. Excluding four studies should be based on strong justifications, which Gebhard *et al.* do not provide.

The Editor's Perspective accompanying our review also receives criticism. We find this unnecessary; a careful reader of our article will find our conclusion to be meticulously stated: "[I]t is important to confine the conclusion to the specific settings where the comparisons were conducted. Pectoralis-II is not clinically worse (noninferior) for analgesic outcomes to single-injection paravertebral block in patients having breast surgery procedures involving the axilla." This finite and very specific statement renders the suggestion of overstating results unreasonable.

Finally, the "strong feeling" of Gebhard *et al.* that no conclusions can be drawn from the data may have been biased by their stated belief that multilevel paravertebral block is superior to single-level paravertebral block, which is debatable. There is evidence from a recent clinical trial indicating that single-level paravertebral block provides analgesia (and dermatomal spread) that is equivalent to multilevel paravertebral block.[3] Moreover, many practitioners continue to use single-level paravertebral block to provide postoperative analgesia for breast surgery. Consequently, the comparison of a single-level paravertebral block with single-injection Pectoralis-II block is a valid clinical and research question.

Competing Interests

The authors declare no competing interests.

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