



Letter to the Editors

Abdominal wall blocks to reduce pain in pediatric surgery



To the Editors:

We read with great interest the recent report by Landmann et al.¹ The authors performed a retrospective chart review in pediatric patients treated at a single institution during a 2-year period and concluded that laparoscopic-guided abdominal wall nerve blocks show similar efficacy to ultrasound-guided nerve blocks performed by pain management physicians without increasing time in the operating room. The authors should be congratulated for performing a well-designed study to improve an important topic (eg, acute pain) in pediatric patients undergoing surgery.^{2,3} In addition, the current interest in the use of regional blocks to improve postoperative analgesia across many pediatric procedures makes the topic timely in perioperative medicine.^{4,5}

Although the study of Landmann et al¹ was well conducted, there are some questions regarding the study that need to be clarified to determine the validity of the results. First, it is not clear whether the intraoperative and postoperative analgesic administration was standardized for the groups, as this can significantly alter the main outcomes. Second, there is currently no evidence that the transversus abdominis block and the rectus sheath block are equivalent and can be combined in a single study group. Last, the authors failed to prove superiority; however, they did not demonstrate noninferiority between the techniques.

We would welcome some comments to address these issues. This would help to further support the findings of this important study.¹

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Re: Abdominal wall blocks to decrease pain in pediatric surgery



This letter is a response to a letter to the editor titled “Abdominal Wall blocks to Reduce Pain in Pediatric Surgery,” which was a correspondence regarding our manuscript titled “Laparoscopic-Guided Abdominal Wall Nerve Blocks in the Pediatric Population: A Novel Technique With Comparison to Ultrasound-Guided Blocks and Local Wound Infiltration Alone,” which was published in *Surgery*. We would like to thank the authors for highlighting some important points. They bring up 3 major points, which we will address in order:

1. Intraoperative and postoperative analgesic administration was not standardized across groups in this study. We recognize that is a weakness of this study and is, unfortunately, a common flaw when comparing groups retrospectively. We highlighted this limitation in our discussion, and our group intends to perform a prospective trial with a standardized perioperative analgesia strategy across all groups.
2. It is true that there is no current evidence stating that rectus sheath blocks and transversus abdominis blocks are equivalent. In fact, their utilization is more dependent on the desired distribution of the block. The “mixing” of these approaches is a result of the retrospective nature of this study but also demonstrates the typical utilization of these blocks across several different operations. As stated earlier, we plan to compare only rectus sheath blocks and only for 1 operative indication (appendicitis).
3. The goal of our study was to describe our experience with laparoscopic abdominal wall nerve blocks and compare it with 2 more established techniques, the ultrasound-guided abdominal wall nerve block and local port site injection. Although we did not show superiority, our study demonstrated that pain scores and narcotic usage were no different than when an ultrasound-guided abdominal wall nerve block was performed. We believe that this is important to point out because a clear benefit of a laparoscopic-guided abdominal wall nerve block is the increased access for many patients. The operating surgeon can perform the block with no additional equipment or staff. This point is essential to understand the benefit of our work because this block, which leads to similar outcomes, can be done at an institution where trained pain-management physicians may not

be available or at a time of day when there may be limited access to a pain management team. It is important to note that the most common operation represented in the entire cohort was laparoscopic appendectomy, which is rarely a scheduled operation and often takes place in the evening or on weekends.

We again thank the authors of this letter to the editor, and would welcome any additional comments.

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Concerns about the study of septic predictor index as a novel tool in detecting thermally injured patients susceptible to sepsis



To the Editors:

We read with great interest the recently published article by Chen et al.,¹ who found that the Septic Predictor Index (SPI) can help determine sepsis onset in thermally injured patients. However, we wish to raise some questions about the study.

First, Chen et al.¹ claimed that they defined sepsis prospectively and partially based on the newest international definition of sepsis (Sepsis-3).² However, Chen et al.'s research¹ was conducted between 2013 and 2015, while the Sepsis-3 study² was not published until 2016. What is the explanation for this time discrepancy?

Second, some data in Chen et al.'s report¹ should be reassessed. In the demographics table, the proportion of subjects with inhalation injury in the septic group should be 80% (16/20) rather than 67%. Furthermore, Fig 1, A displays 18 circles in the nonseptic group to represent 18 patients, while only 17 patients were included in total. Similarly, Fig 3, B shows only 15 circles representing 15 patients, while there were 20 septic patients in total.

Third, underlying diseases such as pneumonia,³ renal failure,⁴ diabetes mellitus,⁵ acquired immune deficiency syndrome, and some immunological therapies (thymosin, immunoglobulins, and glucocorticoids) can change the body's immune status, resulting in altered SPI. As the table in the report¹ indicates, the proportion of complications of pneumonia and renal failure differed significantly between the septic and nonseptic groups, which can lead to biases in interpreting the final results. However, the authors do not mention this concern.

Fourth, when comparing the SPI values, Chen et al.¹ found no significant differences between the adult and elderly groups and the survival and death groups, respectively. They then concluded that different immune status measured by SPI values were exclusive to septicemia prediction and not confounded by age or mortality. But did a young man actually have a similar immune status

to an elderly man? Did a living person actually have a similar immune status to the dead? Perhaps not, in fact, as Figs 4, A and 4, B demonstrate: In the septic group, the adult patients had almost twice the SPI values compared with elderly patients. Similarly, the surviving patients also had almost twice the SPI values compared with their dead counterparts. What is the explanation for these results? We believe the insignificant difference was mainly because of the small sample size of the septic group, which consisted of only 20 subjects.

Finally, we appreciate Chen et al.¹ for their innovative work, but further rigorous, validating studies are still needed.

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Re: Concerns about the study of Septic Predictor Index as a novel tool in detecting thermally injured patients susceptible to sepsis[☆]



To the Editors:

We thank Dr. Zhou and colleagues for their interest in our study¹ and their comments² on our recent article.

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