## Efficacy and pharmacokinetics of erector spinae plane block in children

To the Editor

I read with great interest the recently published study by Macaire *et al*[1] on bilateral erector spinae plane block (ESPB). It is apparent that the authors have put substantial effort into their study. However, in line with the Editorial in the same issue,[2] partly dealing with issues related to the peer-review process, I would like to raise two issues that need further comment:

First, in the first paragraph of the Discussion, it is suggested that bilateral ESPB with intermittent bolus injections is a useful technique in the context of pediatric cardiac surgery. However, the design of the study, where both groups initially were treated with bilateral ESPB, together with the lack of the two all-important control groups outlined in our recent Daring Discourse,[3] make the interpretation of the results quite complex and non-conclusive. What can be concluded is that repeated administration of parenteral local anesthetics does reduce opioid requirements and vomiting, something that is already described in the literature.[4]

Second, the pharmacokinetic part of the study is intriguing. There is no explanation why the plasma levels were only studied in 10 out of the 27 patients receiving repeated boluses of ropivacaine. Were these patients the first 10 consecutive cases or were there some sort of randomization? Or were they the last 10, realizing that analyzing plasma levels were a necessary part of the study? Furthermore, the authors report the 48 hours results as mean±SD (0.46±0.49 µg mL-1). This represents a textbook example of a non-Gaussian distribution since the SD is larger than the mean value. Since the plasma level for obvious reasons cannot be a negative value, this implies that some of the 10 48 hours samples may have been deviating considerably from the mean value, being quite high. I look forward to the presentation of the individual values in the authors' response to this letter. Additionally, the analysis methodology for ropivacaine is just described as gas chromatography with an upper detection limit of 5 microgram/mL-1, accompanied by a reference for more detail. When you look up this citation, that publication (by some of the same authors) in fact analyzes levo-bupivacaine. However, there is in turn an additional reference regarding ropivacaine, but this reference is describing liquid chromatography–electrospray mass spectrometry determination of ropivacaine,[5] something that is very different from gas chromatography. Not only is the most adequate analysis method not gas chromatography, but it must also be considered that you need to do your calculations in relation to the fact that ropivacaine in this setting is the ropivacaine base with a relative molar mass of 274[6] and not on the ropivacaine molecule itself. In summary, to analyze ropivacaine properly is a demanding endeavor. Thus, against the above I unfortunately find the pharmacokinetic data reported by the authors highly questionable.

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