In our editorial, we focused on discussing Dr. Duncan's paper and fast-tracking techniques. We did not compare his work to other fast-tracking studies. References (the number of which is limited to ten) were selected to underline specific points in the discussion. Notwithstanding Dr. White's considerable contributions to the field of fast-tracking in ambulatory anesthesia, because of the limited space and a different focus, these could not be included in our editorial comment on Dr. Duncan's study. Dr White can rest assured that it was never our intent to "knowingly ignore the peer-reviewed literature on a topic when preparing an editorial".

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Epidural analgesia and maternal fever

To the Editor:
We read with interest the article by Vallejo *et al.*[1] and wish to comment on the methodology used and the authors' conclusions.

In this case-control study patients were selected from database in which the presence of maternal fever > 38.0°C was a diagnostic sign for chorioamnionitis. Thus, it is hardly surprising that 100% of the selected patients had fever (regardless of whether they had an epidural or not), while only 1% of the patients enrolled in the no-chorioamnionitis group developed fever. Accordingly, the chorioamnionitis patients had also a higher incidence of histologic chorioamnionitis. In addition, since the indication for neonatal sepsis evaluation rate was maternal fever or clinical amnionitis, the differences in evaluation rates precisely followed patients' selection rather than – as implied from the discussion section – a new finding.

Based on the above, we question the authors' conclusion that chorioamnionitis and not epidural anesthesia was the cause for maternal fever, a finding that could not have been derived from the methodology that was used.

The incidence of maternal fever increases with longer epidural use,[2,3] ranging between 7% with epidural use less than six hours to 36% after >18 hr. The authors did not report labour length, thus, the low incidence of maternal fever in the epidural without amnionitis group could relate to a short epidural use.

We believe that a large-scale prospective study that examines maternal and neonatal outcome, together with histological and microbiological evaluations of the placenta and neonate, would better elucidate the true nature of maternal fever after epidural analgesia for labour.

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Reply:

The selected variable in our obstetrical database was clinical chorioamnionitis (amnionits), and not fever (> 38°C). Our results illustrate the relationship between fever and amnionitis in that parturients with the diagnosis of amnionits with or without concomitant labour epidural analgesia (LEA; Groups I and II) have a significantly higher percentage of fever (100%) compared to the LEA group without concomitant amnionitis (1% - Group III).[1] Indeed, these results were due to patient selection and the diagnosis of histological chorioamnionitis is biased due to the selection process. However, the author of the letter has overlooked the main point of patient selection (methodology). Parturients were selected to control for the confounding effect clinical chorioamnionitis (amnionitis) has on maternal fever and LEA. Our methodology purposefully subdivided nulliparous parturients into three groups to control for the presence of clinical chorioamnionitis (amnionitis). Clearly, when parturients are subdivided by whether they presented with amnionitis or not, the numbers of non-amnionitis parturients with maternal fever (> 38°C) drops to almost zero (P = 0.000).[1]

The incidence of maternal fever increases with duration of epidural use, about 0.07°C per hour.[2] However, epidural analgesia does not elevate maternal temperature enough to cause maternal fever (> 38°C) regardless of labour duration.[2,3] Additionally, our results concur with the results of Dashe et al. in that epidural analgesia is associated with intrapartum fever only in the presence of histologic chorioamnionitis.[4]

We too believe a large-scale prospective study is necessary to better elucidate the true nature of maternal fever, however, the present study suggests that one must control for and cannot ignore the confounding factor of clinical chorioamnionitis (amnionitis).

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Guilt by association?

Letter to the Editor:

We read with interest the article by Han *et al.*[1] on the use of laryngeal mask airway in Cesarean delivery. We agree with the editorial by Roanne Preston[2] that regional anesthesia (RA) is the preferred choice of anesthesia for Cesarean delivery. However, we disagree with Dr. Preston's assertion that Hawkins *et al.*[3] data showed general anesthesia (GA) to be 16 times more lethal than RA. David Chestnut[4] pointed out the serious limitations of the statistics and their interpretation not the least of which was that at risk patients may have received GA instead of RA.

The British have been rigorously collecting data on maternal mortality. The data is much more complete and in their most recently published triennium of 1994–96,[5] there was only one death solely attributed to anesthesia. It was a regional anesthetic.

Josten *et al.*[6] reported their experience with maternal mortality from 1988 to 1996. Of 890,422 births, there were no fatalities attributable to anesthesia. The distribution of anesthesia for Cesarean section was 60.8% GA and 39.2% RA during this time period. There is no suggestion from the German data that one technique is better than another, but that they are both safe.

We believe RA to be the preferred technique to GA but think we are doing ourselves as a group a disservice by stating there is a 16-fold lethality associated with GA over RA. This may be guilt by association, not by causality. By branding GA as intrinsically much more dangerous we encourage other health care providers, regulators, and the public to consider it reckless disregard any time we elect to, or have to, administer a GA. More hard data is needed before we can come to meaningful conclusions and statements.

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Bedside indices to predict weaning from mechanical ventilation

To the Editor:

An experienced intensivist may be able to predict whether a patient can be weaned successfully from mechanical ventilatory support or not. However, it is always helpful to have criteria on the basis of which the outcome may be predicted. After the introduction of the rapid shallow breathing index - the frequency to tidal volume ratio ($breaths \cdot min^{-1} \cdot L^{-1}$) by Yang and Tobin,[1] many studies have found it to be a very effective and simple bedside index.[2–4] In an attempt to further improve the accuracy of this index, we modified it by incorporating the weight of the patient as the ratio of frequency to the tidal volume corrected for patient's weight ($breaths \cdot min^{-1} \cdot mL^{-1} \cdot kg^{-1}$). We hypothesized that the tidal volume corrected for the