ed from SGB which blocks sympathetic efferents originating from the thoracic spinal cord. SGB is known to increase cerebral blood flow on the injected side.[2] Modified blood flow to the cerebrum may have affected schizophrenia-related symptoms.[3] The relaxing effect of SGB may have been additive. We were impressed with this unexpected, beneficial effect of SGB on psychiatric symptoms and suggest that more research in this direction may be warranted.

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Fast-tracking in ambulatory anesthesia: a new concept? Not!

To the Editor:

A recent editorial in the Canadian Journal of Anesthesia by Song and Chung[1] entitled "Fast-tracking in ambulatory anesthesia" was of interest because my research group has been actively involved in this area of clinical research for many years. Although Duncan and his colleagues[2] are to be congratulated for achieving successful postanesthesia care unit (PACU) bypass in 83% of their outpatient population undergoing knee arthroscopy procedures, the editorialists erroneously suggested that this was "the first report of a successful (fast-tracking) practice in a community setting." As a former research fellow at the University of Texas Southwestern Medical Center in Dallas, Dr. Song should have been aware of the numerous papers which our group has published on fast-tracking techniques for ambulatory surgery in both the university and community-based setting. In fact, Dr. Song participated in some of the early studies and co-authored the manuscript[3] which described the criteria used by Duncan *et al.*[2] to determine fast-tracking eligibility in their study. Of interest, in our community hospital-based practice in Los Angeles, 100% of the outpatients undergoing hernia repair and breast surgery are fast-tracked, with average times to *discharge home* of less than 60 min.[4,5]

In my opinion, it is unprofessional to knowingly ignore the peer-reviewed literature on a topic when preparing an editorial. While there is clearly a need for further studies on fast-tracking after ambulatory surgery, I would suggest that there are already a large number of published studies demonstrating the safety of fast-tracking programs in this surgical setting.

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Reply:

Failure to acknowledge Dr. White's articles in our editorial was unfortunate but can be explained easily.

In his letter, Dr. White points out that his research group has published numerous articles on fast-tracking techniques for ambulatory surgery in both university and community-based setting. At the time we wrote our editorial, we considered these earlier studies were teaching hospital related researches (including office space anesthesia) and did not represent common practice in the community hospital. Both Texas Southwestern Medical Center at Dallas and Cedar Sinai Medical Center in Los Angeles are affiliated with universities. Therefore we suggested that Dr. Duncan's study was the first report of successful fast-tracking in ambulatory anesthesia in a community setting, which is more generalizable and applicable to community practitioners. Victoria General Hospital is not affiliated with any university. If our assumptions are incorrect, we apologize for this erroneous statement.

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).