Letters to the Editor

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The effect of the dynamic air bubble trap on cerebral microemboli and S100β

To the Editor:

We read with interest the article by Schoenburg and colleagues[1] regarding the use of an air bubble trap during coronary artery bypass grafting. The authors used measurement of S100β as an indicator of cerebral injury. It is not clear, however, whether they took steps to minimize autotransfusion of S100β from extracerebral sources. There is good evidence to show that blood aspirated from the surgical field and returned to the patient through cardiotomy suckers will result in significantly higher serum levels of S100β.[2,3]

The study showed no significant difference in serum S100β between the two groups at the end of the operation and 6 hours afterward. If the hypothesis that microemboli are the main cause of cerebral injury with a resultant rise in S100β is correct, then a significant reduction in cerebral microemboli should be accompanied by a significant reduction in S100β at these time points. In fact the higher, albeit not statistically significantly, mean S100β levels in the placebo group could be accounted for by the longer mean cardiopulmonary bypass time in this group. A longer bypass time implies that more blood is suctioned from the mediastinum, with increased return of mediastinal blood rich in extracerebral sources of S100β to the circulation, thus resulting in higher serum levels of S100β.[4]

Furthermore, the statement that there was a significant difference in S100β values at 48 hours is misleading. The Sangtec 100 immunoradiometric assay (Sangtec Medical AB, Stockholm, Sweden) that was used in the study has a lower detection limit of 0.2 μg/L. The mean S100β value for both the bubble trap and placebo groups was below this level, and thus normal. We therefore do not agree with the conclusion that the dynamic air bubble trap reduces S100β early after coronary artery bypass grafting. A more sensitive analysis of S100β could have been carried out with the immunoluminometric method (Sangtec LIA 100; Sangtec Medical AB), which has a functional detection limit of 0.02 μg/L.

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Improved pain control after cardiac surgery

To the Editor:

We read with interest the article by Dowling and associates[1] about improved pain control by means of intraoperative intercostal nerve block followed by postoperative continuous infusion of ropivacaine. We performed a similar study that will be published soon.[2] We randomly assigned 47 patients to two groups. The study group (n = 23) was treated by soft-tissue infiltration with bupivacaine (10 mL 0.5% bupivacaine) followed by 36 hours of continuous infusion (bupivacaine 0.5% at 120 mL/24 h) through a small-diameter catheter positioned anteriorly to the sternum. In the control group (n = 24) the catheter was posi-

tioned as well but there was neither infiltration nor postoperative infusion. After the operation we assessed pain control by a visual analog scale at specific time points (extubation and 12, 24, and 36 hours). We also recorded the assisted ventilation time as well as the intravenous administrations of analgesic drugs. Blood gas analysis was performed at extubation and at 6, 12, and 24 postoperative hours. Unlike Dowling and associates' findings,[1] we were unable to demonstrate any effectiveness of this treatment, even though the drug and its dosage were similar to those used in their study. However, the protocol of these authors consisted in a T1-T12 intercostal nerve block, whereas we performed a local soft-tissue infiltration. We therefore may speculate that the efficacy of Dowling and associates' treatment was related more to the intercostal nerve block than to the postoperative continuous infusion, which was common to both studies. An interesting second step of their study would be the comparison between the sole intercostal nerve block and this procedure followed by continuous anesthetic infusion. Regardless, we may conclude that these attempts to reduce postoperative pain are promising and deserve future developments.

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2. Magnano D, Montalbano R, Lamarra M, Ferri F, Lorini L, Clarizia S, et al. Uneffectiveness of local wound anesthesia to reduce postoperative pain after median sternotomy. *J Card Surg*. In press.
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Reply to the Editor:

We are responding to the letter of Racigno and Carnelos. As they described, we performed a double-blind randomized controlled trial in which patients undergoing elective bypass surgery received either bilateral intercostal nerve block with ropivacaine or a saline placebo. At wound closure, two catheters with multiple side openings were inserted percutaneously and placed directly over the sternum. The same agent (ropivacaine or saline solution) was then administered as a continuous regional infusion for 48 hours through an elastomeric pump. The main findings of our study were that patients in the ropivacaine group had improved postoperative pain control and required less narcotic analgesia. Additionally, patients in the ropivacaine group had significantly lower pain scores than did patients who received placebo, and length of stay was also decreased in the ropivacaine group.

Racigno and Carnelos report doing a similar randomized trial in which they were unable to demonstrate any effectiveness of this treatment. In their letter, however, it is not clear how similar the therapeutic approaches truly were. We believe that a key component in our study was the development and clinical use of catheters with multiple side openings, which allow even distributions of the anesthetic agent throughout the operative field. Racigno and Carnelos reported using a small-diameter catheter, but it is not clear whether this catheter was effective in providing a true regional anesthetic. They further suggested that the difference in outcome may have been related to the use in our study of bilateral intercostal nerve block before performing wound closure.

We believe this is unlikely to explain the major differences seen between our two groups for a number of reasons. First, it is widely documented that intercostal nerve blocks, even with long-acting anesthetic agents, do not provide pain control beyond 6 to 8 hours. Second, we assessed pain scores on the second postoperative day to eliminate the effect of this and other confounding variables, such as residual analgesia given at the time of operative therapy. We were able to demonstrate marked decrease in narcotic use on the second postoperative day, when clearly the patient would not be receiving any benefit from the intercostal nerve block.

In addition, since the submission of our article a similar study has been performed that has demonstrated the advantages of a continuous local infusion for pain management after median sternotomy.[1] White and colleagues[1] performed a prospective, randomized, placebo-controlled trial with the same catheter system that was used in our study. These investigators also showed a significant reduction in pain scores, morphine use, patient dissatisfaction, time to ambulation, and duration of hospital stay.

In our opinion, the results of our study and that of White and colleagues[1] strongly suggest that this approach will result in improved patient comfort and satisfaction, as well as decreased narcotic use after median sternotomy. As noted in both studies, the use of the catheter does not require any physician or nursing intervention after its placement, and there has been no morbidity associated with this approach. We consider this approach an excellent first step to improving the overall approach to pain management for our patients.

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Reference

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Suggestion for functional model to test effects of decellularization of rat aortic valve allografts on leaflet destruction and extracellular matrix remodeling

To the Editor:

In their article "Decellularization of Rat Aortic Valve Allografts Reduces Leaflet Destruction and Extracellular Matrix Remodeling," Grauss and colleagues[1] elegantly evaluated various methods of decellularization of aortic valve conduits and tested their structural integrity after implantation in a rat model. Their findings suggest that decellularization is possible and may reduce the recipient's immune response to allogeneic tissue, as demonstrated by preservation of the leaflets' structural integrity in decellularized allografts relative to cellular allografts. Despite the lack of immunologic data and the small number of animals, they showed convincing histologic evidence reflecting these findings.

However, one of the major limitations affecting the generalizability of their find-