[who] were misclassified with respect to the presence or absence of PPM." Misclassification is definitively more frequent when the classification is based on fixed data, either from in vitro study or previously published studies, and not when the classification is based on real in vivo data that reflect the hemodynamic condition of a specific patient. The question of why some patients with the same prosthesis size had such different postoperative EOAs is surely matter for further discussion and evaluation, but postoperative in vivo EOA must anyway be considered the criterion standard for PPM identification and cannot be considered simply misleading just because different patients have different EOAs.

The final two comments of Pibarot and coauthors are all related to the clinical effect of PPM. With respect to the effect of PPM on pulmonary arterial pressure, Pibarot and coauthors simply underlined what we had already stated in our Discussion section. Furthermore, among three recent studies quoted by Pibarot and coauthors, the one from Li and colleagues⁶ was also a key reference in our study, and the two from Magne and coauthors⁷ and Lam and coworkers⁸ were published well after we sent our article for consideration for publication in the *Journal* (February 2007, as clearly reported on the first page¹).

With respect to clinical impact of PPM in terms of operative and late mortalities, we also clearly stated in the Discussion section that our study could not be elucidative because only 8 patients had PPM. Clinical relevance of PPM has to be better elucidated and surely represents an important matter. We would like to stress, however, that once more the evidence must come from a complete evaluation and analysis of different experiences published in the literature. Recent experiences quoted by Pibarot and coauthors^{7,8} clearly identify PPM as risk factor for poor outcome. Previous experience from Lorusso and associates⁹ and Ruel and coworkers,¹⁰ however, did not confirm different outcome in patients receiving small prostheses. Because Lam and coworkers' definition of patients with PPM was not based on postoperative in vivo data,⁸ patients classified as having PPM should mainly be patients receiving small prostheses. These data are therefore in contrast and deserve further evaluation and further studies. In conclusion, in our opinion only a multicenter, prospective

study that is based on in vivo postoperative evaluation of PPM could reach a better level of evidence regarding the exact definition of PPM at the mitral level, the real incidence of postoperative mitral PPM, and its clinical impact. Our study was not meant to contradict results from other researchers but simply to warn about the real potential for misclassification of patients has having PPM solely on the basis of fixed data rather than dynamic data specific to that patient. For the patient receiving a mitral prosthesis, specific postoperative in vivo evaluation should be mandatory to evaluate the real incidence of mitral PPM.

Finally, we would respectfully underline that the appropriate scientific controversy should call for a different attitude and approach to other investigators simply driven by the same enthusiasm for proven data about a scientific aspect whose explanation, unless proven, is usually not in the hands of a unique group of investigators.

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doi:10.1016/j.jtcvs.2007.10.028

Thoracic epidural anesthesia and atrial fibrillation after coronary bypass grafting To the Editor:

I read with great interest the recent article by Dr Bakhtiary and colleagues¹ detailing the protective effect of high thoracic epidural anesthesia against atrial fibrillation after off-pump coronary bypass grafting. The following clinical questions arise as a result of this fascinating study:

1. Why was the placement of the thoracic epidural catheters the day before the operation?
2. Were there any thoracic epidural placements complicated by significant puncture-site bleeding? What was the protocol for managing this uncommon but important event? Was another epidural space selected?
3. How was the epidural catheter tested to ensure effective bilateral thoracic anesthesia?
4. What was the management of postoperative β -blockade? Was β -blocker withdrawal a possible confounder in this study?
5. Is there any explanation for the observation that women were more likely to experience nausea and vomiting in the intervention group?
6. What was the power calculation for this study based on the existing literature? What is the probability of a type I error, a type II error, or both?

7. Is aprotinin exposure a consideration, given its trend to protect against atrial fibrillation after coronary surgery?²
8. Is hyperglycemia a possible confounder, given its association with higher rates of atrial fibrillation after cardiac surgery?³ Was there a standardized approach to perioperative glucose management? Insulin therapy has been shown to be protective against postoperative atrial fibrillation.⁴
9. Was perioperative magnesium therapy standardized, given its link with the incidence of atrial fibrillation after cardiac surgery?⁵

I congratulate the authors again on a most excellent study. I look forward to their feedback about these considerations.

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doi:10.1016/j.jtcvs.2007.09.063

Reply to the Editor:

We appreciate the comments of Dr Augoustides regarding our recent article¹ and would

like to point out the following issues in response to his considerations.

The placement of the thoracic epidural anesthesia (TEA) catheter at the day before surgical intervention was recommended based on study protocol to control any acute catheter-related complications. Fortunately, we did not have any neurological or respiratory complications in the TEA group during our study.¹ In case of any important complication, the patient would be excluded from the study. In case of any bleeding complication, we would not recommend to reselect another epidural space.²

Sensory block levels were determined bilaterally by using loss of warm/cold sensation, as well as pinprick discrimination. The levels of motor block were estimated in the outplaced left arm by using an epidural anesthesia-scoring scale for arm movements. Both sensory and motor block were checked in 5-minute intervals until the desired anesthetic level was established.

The study protocol did not contain any medication prophylaxis, such as a β -blocker, against atrial fibrillation (AF). Thirty-two patients in the general anesthesia group and 35 patients in the general anesthesia plus TEA group had a medication with a β -blocker. It is well known that female sex is one of the most predictive risk factors for postoperative nausea and vomiting, and the results of our study confirm the value of this specific risk factor.³ The power calculation for this study was 0.90, with an error probability of 0.50. We share the opinion with Dr Augoustides that, based on recent literature, the use of aprotinin in some schema, such as the Hammersmith strict perioperative management of hyperglycemia, might have some protective effect on prophylaxis of AF in patients undergoing coronary bypass surgery.

There was a standard approach to manage perioperative hyperglycemia by using insulin therapy to achieve blood glucose values of less than 120 mg/dL. The study protocol did not contain any medication prophylaxis of magnesium against AF.

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Effect of eliminating daily routine chest radiographs on on-demand radiograph practice in post-cardiothoracic surgery patients

To the Editor:

We read with great interest the recent article by Mets and colleagues¹ describing the results of a study comparing the effects of a routine versus on-demand chest x-ray approach in postoperative cardiothoracic surgery patients. The authors conclude that the on-demand approach led to a reduction in the number of chest radiographs performed without changing x-ray practice on their post-intensive care unit.

The practice of routine daily chest radiographs has been frequently questioned in the past.² The authors themselves have documented a low number of unexpected findings in routine chest radiographs and a low impact of these findings on further therapy.^{3,4} As mentioned in the article, the authors have therefore abandoned daily routine chest radiographs for all patients in the intensive care unit. All the more surprising is the fact that this study was performed in a prospective comparative fashion and that the ethics committee deemed it unnecessary to obtain informed consent.

The quoted recommendations by the American College of Radiology as a scientific basis for daily routine chest radiographs