

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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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

require special scrutiny: daily chest x-ray films are recommended only for patients with respiratory failure supported by mechanical ventilation or for acute cardiopulmonary problems. This does not compromise all postoperative cardiothoracic patients (http://www.acr.org/SecondaryMainMenuCategories/quality_safety/app_criteria/pdf/ExpertPanelonThoracicImaging/RoutineChestRadiographDoc7.aspx [last accessed August 2007]).

In Germany, government regulations require that each radiograph be ordered by a physician with a documented clinical indication (<http://www.bmu.de> [homepage of the German Federal Ministry for the Environment, Nature Conservation and Nuclear Safety, last accessed August 2007]). Therefore, radiologic assessment of a patient solely on a routine daily basis is actually illegal. A change in the clinical status of a patient (eg, laboratory findings, new chest tube) and a lack of alternative diagnostic tools (eg, ultrasound) have to be present before a chest radiograph is performed. This reflects the fact that any unnecessary radiation exposure should be avoided.

We hope that this study further helps to abandon the seemingly still common practice of daily routine chest radiographs.

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medical-surgical intensive care unit. *Intensive Care Med.* 2007;33:639-44.
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Reply to the Editor:

We appreciate the attention paid by Kappert, Stehr, and Matschke to our article concerning the elimination of daily routine chest radiographs in post-cardiothoracic surgery patients.¹ They raised several issues, which we would like to address point by point.

First, it may seem surprising that the ethics committee deemed it unnecessary to obtain informed consent when abolishing a daily routine strategy. However, it was in fact a medical decision to change the policy of obtaining daily routine chest radiographs in view of all the data indicating its limited value.² This study was used to evaluate the effects of that medical decision. In addition, in a separate prospective study, daily routine chest radiographs were blinded to attending physicians but evaluated by trained radiologists, that is, unsuspected predefined major abnormalities were disclosed and communicated with attending intensive care unit physicians.³ In that study, we demonstrated that the number of unsuspected predefined major abnormalities on daily routine chest radiographs was low (4.4%) and the number of radiographs resulting in a change in therapy was even lower (1.9%).

Second, we agree with Kappert, Stehr, and Matschke that the current American College of Radiology guidelines only recommend chest radiographs for patients with respiratory failure supported by mechanical ventilation or with acute cardiopulmonary problems. Indeed, this is not the case in all cardiothoracic patients.

Third, it is interesting to read that a daily routine strategy for obtaining chest radiographs is in fact prohibited by law in Germany. At least in the Netherlands, a daily routine strategy for obtaining chest radiographs in every patient in the intensive care unit is still frequently practiced,⁴ although in our experience this extends to many other countries in the world. We fully agree with Kappert, Stehr, and Matschke in hoping that this approach will change in view of all the available evidence.

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Surgery for esophageal cancer: Do age or incorrect data analysis have any impact on clinical outcome?

To the Editor:

Within the frame of a general concern about the quality of scientific articles published in peer-review international journals, I would like to make some considerations regarding the manuscript by Ruol and colleagues¹ recently published in the *Journal of Thoracic and Cardiovascular Surgery*, titled “Results of Esophagectomy for Esophageal Cancer in Elderly Patients: Age Has Little Influence on Outcome and Survival.”

The authors conclude that age should not be considered a contraindication for elderly patients (>70 years) because in their retrospective study no significant survival difference was found between elderly and younger patients.

In the light of the large series described, the authors had the opportunity of providing the scientific community with a clinically relevant piece of information: unfortunately, owing to some evident limits in their data analysis, Ruol and colleagues have missed this opportunity by significantly undermining the reliability of their work.

My perplexities on this article mainly arise from the following considerations, which are mainly statistical in nature:

1. The authors state they used the Kaplan–Meier method to compute survival estimates, which is virtually the only method used worldwide to this aim. However, the curves