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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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

illustrated in Figures 1 and 2 are *not* generated with the Kaplan–Meier method, as anybody familiar with this statistical approach can readily confirm (eg, no staircase appearance, both curves end at 5 years in both figures, symbols are used to differentiate study groups instead of censored/uncensored data that are not reported anywhere in the article). The authors should explain how they built the curves: in other words, if they did not use a statistical software—as it appears—they must explain according to what rules they drew the curves that do not appear to take into account the difference between censored and uncensored data (ie, the foundation of survival analysis). The reason for the discrepancy between the method cited in the “Methods” section and the figures shown in the “Results” section should be explained and the error corrected.

2. In the text (“Results” section), the median survival appears to be 17.9 and 23.1 months for the elderly and the younger group of patients, respectively. However, likely because of the aforementioned issue, the median survivals extrapolated from the curves depicted in Figure 1 are strikingly different (roughly 23 and 30 months, respectively). This is further proof that something is wrong with the survival analysis reported by Ruol and colleagues.
3. In the “Discussion” section the authors keep repeating that the study was specifically designed to investigate the impact of age on the clinical outcome. Nevertheless, they did not provide any study design in the “Methods” section. In particular, they did not specify the power of the study, which is of paramount importance to correctly interpret their findings. Since their conclusion is that no survival difference was detected between the two study groups, what is the error beta of this analysis? In other words, what is the likelihood that the null hypothesis (no difference between survivals) was incorrectly accepted? Or, the other way around, what is the median survival

difference detectable with the sample size considered in the study?

4. In the Cox model, the authors chose to include seven covariates: besides age, which was the variable specifically targeted by the study, the choice of the other variables appears quite “random.” In fact, if one can agree on the choice of the variables resulted significant at the univariate analysis reported in Table 7 (ie, tumor stage and grading); however, it is more difficult to understand why the authors included other non-significant variables (ie, sex, tumor location, neoadjuvant therapy, histologic type) while they completely omitted significant variables, such as those reported in Table 2 (ie, history of respiratory disease, history of vascular disease, and American Society of Anesthesiologists classification). Since the number and the type (informative vs confounding) of variables included in the model can profoundly affect the final result, the authors should explain the reasons for their choice and provide data on models built with all the variables yielding significant results at univariate analysis.
5. In Table 8 (results of the Cox model), the authors reported the χ^2 values for each covariate: clearly this information is meaningless to most readers, who are certainly much more interested to know the hazard ratio associated with each covariate (ie, the risk of death of the covariate categories as compared with the baseline category). Again, the choice of the authors appears unfounded and leaves some doubts on the statistical competence of the person(s) who took care of the data analysis.
6. Although there is a significant difference in preoperative risk (as assessed by the American Society of Anesthesiologists score system) between the two study groups (Table 2), the post-operative death rates are almost identical (Table 6). If confirmed, this is a potentially useful piece of information that has been completely overlooked in the “Discussion” section. If the authors are confident in their data, they should

probably let the American Society of Anesthesiologists know that the most commonly used preoperative risk assessment score likely does not work with patients affected with esophageal cancer.

Overall, I am aware that the above list resembles a second revision rather than a commentary on an article already published. In this regard, I believe that the review process in this specific case has failed to detect some significant limits of the manuscript and thus the responsibility of such “limping” publication is not to be attributable only to the authors. On the other hand, I hope everybody will agree that clinical research cannot be thought of as a list of therapeutic interventions with approximate data analysis, inasmuch as no scientifically and/or clinically meaningful information can derive from such an approach.

If we intend to make advances in medicine (as well as in any other field) by publishing the experiences of research groups in scientific journals so that other researchers can take advantage of the published data, we must guarantee that the information is at least formally correct.

In this specific case, I would recommend that the authors have the data analysis revised by a biostatistician and then publish a letter with the explanations and corrections necessary to recover the reliability of their work.

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Reply to the Editor:

We appreciate Dr Yung’s concerns about the quality of scientific articles published in peer-reviewed journals such as the *Journal of Thoracic and Cardiovascular Surgery*, which are read by thousands of specialists in both thoracic and general surgery. It has been a privilege for us to have our data published in the *Journal*