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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Reference

1. Ruol A, Portale G, Zaninotto G, Cagol M, Cavallin F, Castoro C, et al. Results of esophagectomy for esophageal cancer in elderly patients: age has little influence on outcome and survival. *J Thorac Cardiovasc Surg*. 2007; 133:1186-92.

doi:10.1016/j.jtcvs.2007.07.070

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*

and we would like to reassure Dr Yung that the review process of the *Journal* has not failed to detect possible significant limits in our manuscript, as he suggested.

We will supply the following details.

1. The survival curves shown in the figures were generated with the Kaplan–Meier method, but the “reproduction” process failed to show the typical staircase appearance. We are submitting 2 new figures with the censored numbers (Figures 1 and 2). The *P* values are the same because the survival estimate had already been correctly calculated with the Kaplan–Meier method.
2. The median survival for all patients was 29.8 for patients younger than 70 years old and 20.8 for those older. Once again, nothing was wrong with the survival analysis we had provided the readers; it was just a typing mistake, as anyone *truly* familiar with statistics surely has already realized.
3. We obviously included in the multivariate analysis variables significant on univariate analysis, plus other variables that, although not significant on univariate analysis, the reader might have been interested to see in the multivariate analysis (eg, use of neoadjuvant therapy, histologic type). We did not include variables of no clinical interest (eg, region where the patients came from) but only variables of clinical interest. We also are providing a new multivariate analysis that includes the variables suggested by Dr Yung. Clearly, pStage, as anyone could expect, remains the most significant factor predicting long-term survival (Tables 1 and 2).
4. χ^2 Is not meaningless and there are hundreds of papers with χ^2 rather than hazard ratio. In any case, we are supplying the data Dr Yung was interested in (Table 3). Obviously, and once again, this was clearly not an issue of statistical “competence” or “ignorance,” but rather preference.
5. We want to reassure Dr Yung (not the American Society of Anesthesiologists [ASA], which is already aware of this) that the ASA score *does* work for esophageal cancer.

Figure 1. All patients.

The fact that mortality rates in younger and elderly patients are similar does not necessarily imply that there cannot be differences in the ASA class distributions among the groups. When we analyzed patients from both groups and compared mortality and morbidity of patients in ASA classes I–II versus those in

ASA classes III–IV, this is what we learned (Table 4).

The ASA classes *do* make a difference in the risk of mortality.

We hope Dr Yung is now pleased with the completeness of the data provided. There is definitely no need to “recover” this article. We are sure the reviewers of the *Journal* who took care of our—not that

Figure 2. R0 patients.

TABLE 1. Univariate analysis

	Five-year survival (%)	P value
Cardiologic history		.066
Yes	29.6	
No	34.8	
Respiratory history		.02
Yes	28	
No	35	
Vascular history		.294
Yes	30.8	
No	34.2	
ASA class		.0015
I–II	36.1	
III–IV	26.8	

ASA, American Society of Anesthesiologists.

TABLE 2. Multivariate analysis

	HR (95% CI)	P value
Age (≥ 70 y)	—	.26
pStage (III–IV)	2.73 (2.23–3.34)	<.0001
Histologic type (SCC)	1.33 (1.08–1.64)	.008
Degree of tumor differentiation (poor)	1.26 (1.01–1.58)	.04
ASA class (III–IV)	—	.19
Respiratory history (yes)	1.39 (1.09–1.77)	.008

HR, Hazard ratio; CI, confidence interval; SCC, squamous cell carcinoma; ASA, American Society of Anesthesiologists.

TABLE 3. Multivariate analysis

	HR (95% CI)	P value
pStage (III–IV)	2.68 (2.19–3.28)	<.0001
Histologic type (SCC)	1.37 (1.11–1.69)	.005
Degree of tumor differentiation (poor)	1.25 (1–1.56)	.048

HR, Hazard ratio; CI, confidence interval; SCC, squamous cell carcinoma.

TABLE 4. Mortality data

	In-hospital mortality	P value	30-day mortality	P value
ASA class		.006		.002
I–II	8/527 (1.5%)		5/527 (0.9%)	
III–IV	11/194 (5.7%)		9/194 (4.6%)	

ASA, American Society of Anesthesiologists.

“limping,” really—manuscript were at least as precise and meticulous in their work as Dr Yung would have been. Hypercriticism for the sake of being critical does not help any progress in surgical research.

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doi:10.1016/j.jtcvs.2007.10.018