5. Bodnar E, Horstkotte D. Potential flaws in the assessment of minor cerebrovascular events after heart valve replacement. *J Heart Valve Dis.* 1993;2:287-90.
6. Horstkotte D, Trampisch JH. Long-term follow-up after heart valve replacement. *Z Kardiol.* 1986;75:641-5.
7. Baudet EM, Puel V, McBride JT, Grimaud JP, Roques F, Clerc F, et al. Long-term results of valve replacement with the St Jude Medical prosthesis. *J Thorac Cardiovasc Surg.* 1995;109:858-70.
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Reply to the Editor:

We thank Dr Horstkotte for his comments on our article comparing two heart valves by means of reports from the literature. In describing the limitations of such comparisons, he gives us another opportunity to stress the main purpose of our report, which was to point out precisely these limitations.

Dr Horstkotte found four "shortcomings" and agreed with three "messages" in our article. The shortcomings were (1) inconsistent use of linearized rates, (2) inclusion violation, (3) inadequate reflection of follow-up techniques, and (4) publication bias.

We attempted to include only late events in the linearized event rates. Most reports used in our study considered late events only, but some used both early and late events and some were unclear on this point. We tried to separate those that used late events from those that used both and attempted classify those that were unclear from other clues in the published reports, but we found this extremely difficult. We finally decided to use the results as published, and we specified this in the Clinical Material section.

We selected only articles that claimed to adhere to the American Association for Thoracic Surgery and Society of Thoracic Surgeons guidelines for reporting,^{1,2} as did Horstkotte and colleagues' 1993 article,³ which said, "Prosthetic valve related and anticoagulant-related complications were reported according to the current guidelines for reporting morbidity and mortality after valve replacement surgery."¹ In addition they used another set of definitions that included minor bleeding events, but we used only the major bleeding events, as recommended by the American Association

for Thoracic Surgery and Society of Thoracic Surgeons.² We did not include their 1994 article⁴ in our analysis.

We agree that the completeness of reporting complications is directly related to the follow-up strategy. We only selected articles with at least 95% follow-up completeness, evaluated the results as a function of follow-up completeness, and emphasized this issue in the Discussion section. However, there is no doubt a wide variation in the use of the word *complete*. A thorough evaluation of this variable should include not just the quantitative (percentage) aspect but also a qualitative description of what is meant. Such meanings can range, for example, from regular office visits with the patient to a phone contact with a widowed spouse long after the patient has died.

We attempted to eliminate articles that did not use universal anticoagulation. We apparently missed the fact that in Baudet and associates' report⁵ 65 patients with aortic replacements and 10 with mitral replacements received aspirin and dipyridamole only for 1 year, and had a higher incidence of thrombosis and embolism, because the article itself presented combined linearized rates. If this article had been eliminated from our study, however, it would have not changed the conclusions. The term *publication bias* refers to the fact that the publication of research studies is nonrandom but depends on the nature and direction of their findings, as described in our Discussion section.

We agree completely with the three "messages" that Dr Horstkotte took from our article—the needs for (1) refined reporting of complications, including grading of severity, (2) consecutive patients, and (3) thorough follow-up techniques—and we again thank him for emphasizing these points. The purpose of our study was to stress the limitations of data collection and definitions, as well as to present refined statistical methods of valve comparison used to deal with variability. The shortcomings that Dr Horstkotte mentioned were exactly what we studied and discussed in the article.

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References

1. Edmunds LH Jr, Cohn LH, Weisel RD. Guidelines for reporting morbidity and mortality after cardiac valvular operations. *J Thorac Cardiovasc Surg.* 1988;96:351-3.
2. Edmunds LH Jr, Clark RE, Cohn LH, Grunkemeier GL, Miller DC, Weisel RD. Guidelines for reporting morbidity and mortality after cardiac valvular operations. *J Thorac Cardiovasc Surg.* 1996;112:708-11.
3. Horstkotte D, Schulte H, Bircks W, Stauer B. Unexpected findings concerning thromboembolic complications and anticoagulation after complete 10 year follow-up of patients with St Jude Medical prostheses. *J Heart Valve Dis.* 1993;2:291-301.
4. Horstkotte D, Schulte HD, Bircks W, Strauer BE. Lower intensity anticoagulation therapy results in low complication rates with the St Jude Medical prosthesis. *J Thorac Cardiovasc Surg.* 1994;107:1136-45.
5. Baudet EM, Puel V, McBride JT, Grimaud JP, Roques F, Clerc F, et al. Long-term results of valve replacement with the St Jude Medical prosthesis. *J Thorac Cardiovasc Surg.* 1995;109:858-70.
doi:10.1067/S0022-5223(03)01222-4

Safety of bronchoplastic resection after induction therapy for lung cancer

To the Editor:

We read with interest the article by Ohta and colleagues¹ concerning the impact of induction treatment on patients who underwent full sleeve resection. Ohta and colleagues¹ concluded that preoperative therapy did not significantly affect postoperative outcome. The issue of the impact of the neoadjuvant chemotherapy in patients with lung cancer is today an object of debate. It has been demonstrated that right pneumonectomy after induction treatment is associated with a high risk of postoperative complications,² and Rendina and colleagues³ showed that lung-sparing resection could reduce the rate of pneumonectomy and could provide an alternative to a pneumonectomy after induction treatment.

We published in 2002 our experience with 27 patients who underwent induction treatment before lung-sparing resection.⁴ We had similar results, demonstrating that chemotherapy or chemoradiotherapy before surgery did not influence postoperative outcome. In addition, in our experience postoperative radiotherapy done for patients with N2 disease did not influence

TABLE 1. Comparison of postoperative morbidity and mortality in patients undergoing bronchial sleeve resection (induction treatment versus no treatment)

	Overall (n = 106)		Induction (n = 56)		No induction (n = 50)		P value
	No.	%	No.	%	No.	%	
Perioperative morbidity							
Minor complications	29	27.5	12	21.4	17	34	.15
Major complications	7	6.6	5	8.9	2	4	.44
Late morbidity	6	5.6	3	5.3	3	6	.99
Perioperative mortality	5	4.7	2	3.6	3	6	.66

bronchial anastomosis in terms of late complications. Finally, the completeness of resection after lung-sparing resection in term of extension of mediastinal and hilar lymph node dissection was comparable to that obtained after pneumonectomy. At present, our experience with lung-sparing technique for lung cancer after induction treatment consists of 58 patients operated on through a period of 5 years, and it represent 54% of all sleeve resections performed during the same period (n = 106). A vascular resection was associated in 35% of cases. The overall postoperative morbidity rate and mortality of patients who underwent sleeve resection were 34% and 4.7%, respectively. Table 1 reports the comparison between the two groups (induction treatment vs no treatment).

This recent updated experience confirms our previous results and strongly supports the result of the article of Ohta and colleagues.¹ Bronchoplastic resection after induction chemotherapy is a safe treatment for locally advanced lung cancer, and it should be done whenever it is possible instead of pneumonectomy.

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References

1. Ohta M, Sawabata N, Maeda H, Matsuda H. Efficacy and safety of tracheobronchoplasty after induction therapy for locally advanced lung cancer. *J Thorac Cardiovasc Surg.* 2003;125:96-100.
2. Martin J, Ginsberg RJ, Abolhoda A, Bains M, Downey RJ, Korst RJ, et al. Morbidity and mortality after neoadjuvant therapy for lung

cancer: the risks of right pneumonectomy. *Ann Thorac Surg.* 2001;72:1149-54.

3. Rendina EA, Venuta F, De Giacomo T, Flaishman I, Fazi P, Ricci C. Safety and efficacy of bronchovascular reconstruction after induction chemotherapy for lung cancer. *J Thorac Cardiovasc Surg.* 1997;114:830-7.
4. Veronesi G, Solli PG, Leo F, D'Aiuto M, Pelosi G, Leon ME, et al. Low morbidity of bronchoplastic procedures after chemotherapy for lung cancer. *Lung Cancer.* 2002;36:91-7.

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Reply to the Editor:

My coauthors and I appreciate the comments by Veronesi and colleagues from the European Institute of Oncology. We find their results¹ to be consistent with ours,² with their latest study reconfirming our conclusion. They had excellent outcomes with a sleeve resection after induction therapy; however, the overall morbidity and mortality resulting from a primary operation were 44% and 6%, respectively, which were higher than those of Kutlu and Goldstraw³ (12% and 2%, respectively), who used the same continuous suture technique for the bronchial anastomosis as did Veronesi and colleagues.¹ The somewhat high morbidity rate in their control group may have had an influence on the statistical comparison analysis. Because surgical complications had a correlation with the extent of surgical resections,⁴ patients in their study who underwent a primary operation have probably received a more radical dissection of the tumor and invaded organs.

In our experience to date, the overall surgical morbidity of 22 patients who received preoperative induction therapy is 32% and that of 38 patients who underwent

a primary operation is 8%, and no postoperative deaths have occurred. Thus we believe that a sleeve resection is useful for any patient with centrally located advanced lung cancer in whom the anatomy is suitable for such a procedure. Further, we can insist, and are supported by the results of Veronesi and colleagues,² that a sleeve resection is a safe alternative to pneumonectomy, irrespective of preoperative treatment.

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References

1. Veronesi G, Solli PG, Leo F, D'Aiuto M, Pelosi G, Leon ME, et al. Low morbidity of bronchoplastic procedures after chemotherapy for lung cancer. *Lung Cancer.* 2002;36:91-7.
 2. Ohta M, Sawabata N, Maeda H, Matsuda H. Efficacy and safety of tracheobronchoplasty after induction therapy for locally advanced lung cancer. *J Thorac Cardiovasc Surg.* 2003;125:96-100.
 3. Kutlu CA, Goldstraw P. Tracheobronchial sleeve resection with the use of a continuous anastomosis: Results of one hundred consecutive cases. *J Thorac Cardiovasc Surg.* 1999;117:1112-7.
 4. Hayes JP, Williams EA, Goldstraw P, Evans TW. Lung injury in patients following thoracotomy. *Thorax.* 1995;50:990-1.
- doi:10.1016/S0022-5223(03)01224-8

Lack of evidence for vitamin C as acute vasodilator

To the Editor:

We read with interest the article by Drossos and colleagues.¹ They measured radial artery diameter before and after acute oral administration of vitamin C (2 g) in two separate studies. Study one was a comparison in healthy subjects comparing nonsmokers with smokers; study two was double-blind comparison with diltiazem.

The article raises several issues that we believe require clarification by the authors. First, the article does not contain any data on the diameter measurements. This is important, because previous studies^{2,3} that