Although this study summarizes an initial experience, we thank the authors for sharing with us their results and for their contribution to this important new field of interventional therapy.

Rachid Zegdi, MD, PhD^{a,b}

Paul Achouh, MD, PhD^a

Jean-Noël Fabiani, MD^{a,b}

Assistance Publique-Hôpitaux de Paris AP-HP

Service de Chirurgie Cardiovasculaire

Hôpital Européen Georges Pompidou

Paris, France^a

Université René Descartes

Paris, France^b

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Reply to the Editor:

We appreciate the authors' concerns about the quality of our work published in the *Journal of Thoracic and Cardiovascular Surgery*. We totally agree with the two underlined points and we would like to simply answer the two questions.

It is true that prosthesis sizing is an important issue. All the reported patients had an aortic annulus diameter ranging from 20 to 23 mm. Smaller and larger annuli were considered as contraindicated so as to

allow correct anchorage of the prosthesis in the annulus.

All the patients had the echographic aspect of a tricuspid aortic valve. We cannot answer Zegdi and colleagues because we do not have any experience with percutaneous aortic valve implantation in bicuspid valves. Bicuspid valves are generally more calcified than tricuspid valves and frequently come with a moderate dilatation of the aorta. They are less favourable for currently used percutaneous aortic valves.

Bertrand Marcheix, MD

Raymond Cartier, MD

Montreal, Quebec, Canada

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Does the method of lung preservation influence outcome after transplantation? An analysis of 681 consecutive procedures

To the Editor:

We read with great interest the study by Ganesh and colleagues¹ that compared various graft preservation methods in lung transplantation. The authors rigorously assessed the association between the lung preservation method and the posttransplant survival up to 3 years in a large cohort of patients; they failed to detect any association. However, we think that the way the graft ischemic time was calculated and included in the analysis may have biased the results. Graft ischemic time has been shown to be associated with survival after lung transplantation in several studies.^{2,3} In these studies, in case of double-lung transplantation, graft ischemic time was defined as the time from donor crossclamping to reperfusion of the second lung. In their study, Ganesh and colleagues considered ischemic time as the time from donor crossclamping to reperfusion of the first lung only. This could lead to incorrect adjustment. Second, we have found that the relationship between graft ischemic time and survival could be of exponential form, the graft ischemic time altering the survival of recipients only when it exceeds 4 to 6 hours.³ Unfortunately, the authors did not report on the distribution of graft ischemic time by the preservation method. Given the incidence of local donors in their cohort, we suspect that graft ischemic time was rather short in most patients.

This point is essential, because the efficacy of different graft preservation solutions may prove similar for short graft ischemic times and different for longer ischemic times. We think that the analysis of adjusted survival by preservation solution for patients with a long ischemic time would have yielded more insight in the efficacy of preservation solutions. However, the number of patients in each group would have probably been low, precluding powerful statistical analysis. The authors report no interaction between the graft ischemic time and the preservation solution used; however, interaction testing is not powerful enough. We agree that the way to definitely answer this question is to conduct a randomized controlled trial, and we think that such a trial should stratify patients by the expected graft ischemic time or the surgical procedure (single vs bilateral lung transplantation).

Gabriel Thabut, MD^a

Yves Castier, MD^b

Hervé Mal, MD^a

Service de pneumologie B et transplantation pulmonaire^a

Service de chirurgie thoracique et vasculaire^b
Hôpital Bichat et Université Paris 7,
Paris, France

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doi:10.1016/j.jtcvs.2007.11.059

Reply to the Editor:

We appreciate the comments made by Thabut and colleagues on our article on lung preservation.¹ We agree that graft ischemia time may be an important predictor to survival after lung transplantation, particularly if this is particularly prolonged. The study by Novick and colleagues² did not find any influence of graft ischemia time alone on survival, but there was a significant