Reply to Baisi et al.

Gail E. Darling*, Waël C. Hanna, Marcelo Cypel and Thomas K. Waddell

Department of Thoracic Surgery, University of Toronto, Toronto, ON, Canada

* Corresponding author. Department of Thoracic Surgery, University of Toronto, 200 Elizabeth Street 9N-955, Toronto, ON MSG2C4, Canada.
Tel: +1-416-3403121; fax: +1-416-3403660; e-mail: gail.darling@uhn.ca (G.E. Darling).

Keywords: Non-small-cell lung cancer • Video-assisted thoracic surgery • Lobectomy • Propensity matching

We would like to thank Dr Baisi et al. [1] for their insightful comments on our paper, specifically regarding the discrepancy in cancer-specific survival between matched patients who underwent lung cancer resection via VATS lobectomy vs open lobectomy [2]. Although no statistical difference could be detected in cancer-specific survival, the survival curves appear to show a slight divergence in favour of open lobectomy. This apparent divergence can be explained by two potential theories. The first is that it is due to chance. One will always wonder whether a significant difference may have been detected if a larger population sample was available, and whether a study with enough power can be actually achieved. The second theory is that better cancer-specific survival can be attributed to better lymph node dissection in the open group. Although we did not find a difference in the number of lymph nodes harvested between the two groups, larger database studies have clearly demonstrated that the rates of N1 lymph node harvest in VATS lobectomy is inferior to open lobectomy [3]. Speculation remains around whether better lymph node harvesting translates into better survival. We agree with Dr Baisi et al. [1] in restricting our conclusions to early-stage lung cancer. Although other groups have demonstrated the feasibility of VATS resection for large or locally advanced tumours [4], our data include only Stage I and II patients, with a predominance of Stage I disease. We are grateful to Dr Baisi et al. [1] for their kind and insightful comments, and we thank them for taking the time to remark on our work.

REFERENCES

I read with great interest the excellent paper by Dr Sponga et al. [1]. This is the first study to show that periprosthetic aortic regurgitation (pAR) after surgical aortic valve replacement (AVR) was independently associated with higher long-term mortality and reintervention. It echoes several recent studies of transcatheter aortic valve implantation (TAVI) that also found pAR to predict worse outcomes [2–4].

Cerebrovascular event is an important complication after aortic valve intervention because of its significant impact on quality of life, cost and risk of mortality. It is also an important consideration when deciding on intervention, given that the PARTNER trial [5] showed that TAVI have higher rates of stroke or transient ischaemic attack than AVR, at 30 days and 1 year.

Although several mechanisms for cerebrovascular events after TAVI are well recognized and intuitive, there may also be an association between pAR and stroke.

Regurgitation around the prosthetic valve implies an increased surface area of foreign material for thrombosis formation. There will be increased turbulence and stasis of blood flow around the altered anatomy of valve replacement when pAR is present. The rate of pAR being higher after TAVI than AVR, for example, 12.2 vs. 0.9% at 30 days in the PARTNER trial [5], similar to cerebrovascular events, supports this assumption. The paper by Sponga et al. [1] also reported the rates of in-hospital stroke to be higher among those with residual AR >1, although not reaching statistical significance (5.2 vs. 3.2%, P = 0.20).

In view of this, I wondered whether Dr Sponga et al. had longitudinal data regarding incident cerebrovascular events after AVR, at least to 1 year, which could support or refute the hypothesis above. Further studies are required to definitively answer this question, which is important because if the association is true, clinicians would need to weigh the benefits and risks of taking a more aggressive approach to intervening or anticoagulation upon any significant pAR after both AVR and TAVI to reduce the rates of stroke over time, while accepting the additional stroke risk of any form of reintervention or bleeding risk from over-anticoagulation.

REFERENCES


LETTER TO THE EDITOR RESPONSE

Reply to Wang

Sandro Sponga* and Pierre Voisine

Quebec Heart and Lung Institute, Laval University, Quebec City, Quebec, Canada

* Corresponding author. Quebec Heart and Lung Institute, Laval University, Quebec City, 2725 Chemin Ste-Foy, Quebec, Canada G1V 4G5. Tel: +1-418-6568711; fax: +1-418-6568711; e-mail: sandro_sponga@yahoo.com (S. Sponga).

Received 4 March 2013; accepted 7 March 2013

Keywords: Aortic valve replacement • Stroke • Neurological events • Aortic regurgitation • Paravalvular leak

We are thankful to Dr Wang [1] for his positive comments on our manuscript as well as his very pertinent question. As he pointed out, we observed a higher rate of stroke in the immediate postoperative period in the group with residual aortic regurgitation (AR), which, however, did not reach statistical significance [2]. Our study was aimed at assessing the impact of residual AR on long-term mortality, for which we have complete data accessible through the Quebec Civil Registry. Unfortunately, long-term rates of complications such as strokes are less readily available, but we do have long-term follow-up data for 64.6% of the AR ≤1 group (median follow-up 4.6 years) and 76.3% of the group with AR >1 (median follow-up 4.7 years). Analysis of those sub-groups revealed no statistically significant difference in freedom from neurological events at 1 (97.3 vs 97.9%), 3 (95.5 vs 95.5%), 5 (93.2 vs 95.5%) and 10 years (89.1 vs 90.3%), respectively (P = 0.72). Of note, a similar incidence of stroke was observed despite a higher percentage of patients receiving mechanical valves in the AR >1 group than in the AR ≤1 group (28.4 vs 6.4%, P < 0.01). The mechanism suggested by Dr Wang to explain potentially higher rates of stroke in patients with residual AR in transcatheter aortic valve implantation [3] remains intuitive and, although not supported by this post hoc sub-group analysis with its inherent limitations, should be verified in a larger study.