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Reply to the Editor:

We thank Osnabrugge and colleagues for their interest in our article and appreciate their comments on the statistical methodology that we adopted for the comparison between the survival of elderly patients undergoing aortic valve replacement (AVR) and the expected survival of the age- and sex-matched general population.

We acknowledge that the method suggested by Osnabrugge and colleagues should be applied in studies with extended long-term follow-up, because of a strong correlation between age and length of follow-up, to avoid underestimation of the general population survival. This statistical method is validated for long-term follow-up, but it is needed less¹ for studies with short follow-up, such as our own, in which it is possible to assume that withdrawal at different times does not affect the estimates of survival. As a matter of fact, it is not surprising that, among many,

such authoritative Institutions as The Cleveland Clinic,² the New York State Department of Health's Cardiac Surgery Reporting System,³ and others⁴ have recently used the same methodology as we to compare the dynamic survival of a surgical population with the static expected survival of a corresponding age- and sex-matched general population.

Moreover, we do not find it intuitively apparent that survival after AVR is unlikely to be comparable to that of the general population unless there is a strict selection of patients, as speculated by Osnabrugge and colleagues when referring to our study cohort. We note that (1) a surgical population is selected by definition (see the limitations of our study listed in the article), (2) progressive accumulation of interstitial myocardial fibrosis may be present in elderly subjects with or without aortic valve stenosis, and (3) we reported the preoperative characteristics of our patients, and our findings are consistent with much growing and converging evidence indicating that long-term survival of octogenarian patients can be equal to the expected survival of an age- and gender-matched population, which should not be neglected by Osnabrugge and colleagues.^{2,4,5}

Any attempt to explain such a favorable outcome after AVR in the elderly population can only be speculative and thus will not be the subject of discussion here. It may, however, have something to do with a lower cumulative incidence of tissue valve-related complications in elderly patients with limited life spans, with a higher incidence of cardiac events among people with severe aortic valve steno-

sis who do not undergo AVR, and with the exclusion—as in our series—of patients undergoing associated myocardial revascularization and/or redo operations.

We apologize for the mentioned errors of transcription from figures to text. A significant ($P < .001$) survival difference was observed between patients operated on in New York Heart Association functional class III or IV and those operated on in functional class I or II. In addition, the 2-year survival for the group undergoing AVR after the age of 80 years group was in fact 87.4%.

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