09.42.18 in the International Pediatric and Congenital Cardiac Code.⁴ This variation appears in 45% of human hearts.^{2,3}

The double RCA is an extremely uncommon coronary artery anomaly with 2 RCAs, coursing toward the right atrioventricular groove, originating marginal branches, terminating by giving off the posterior descending artery in the posterior interventricular groove. It is predominantly seen in male patients and might originate from either single or separate ostia.⁵

We think it is important to standardize the nomenclature in order to have the correct identification of anatomic structures.

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METHODOLOGIC ISSUES REGARDING BACKGROUND MORTALITY IN OBSERVATIONAL STUDIES

To the Editor:

We read with great interest the article "Aortic Valve Replacement: Results and Predictors of Mortality From a Contemporary Series of 2256

Patients," by Di Eusanio and associates,¹ in a recent issue of the *Journal*. We congratulate Di Eusanio and associates on their favorable results after aortic valve replacement (AVR) in octogenarians and on the identification of predictors for postoperative mortality. Some methodologic issues exist, however, concerning the comparison of patient survivals with that of a reference population. The investigators used a static method to compare the 3-year survival for octogenarians who had undergone AVR with the expected survival of an age- and sex-matched regional population and concluded that the survivals were statistically similar ($P = .157$).

Intuitively, it would seem apparent that survival after AVR is unlikely to be comparable to that of the general population. Patients undergoing heart valve replacement are vulnerable to both valve-related and non-valve-related events. In addition, the left ventricle in a patient with aortic valve disease is characterized by progressive accumulation of interstitial myocardial fibrosis and impairment of myocyte ultrastructure, leading to decreased survival. Aortic stenosis has been shown to be an inflammatory process associated with histopathologic changes in the valve leaflets that are similar to those seen in other atherosclerotic diseases, and the presence of calcific valve disease is associated with hypertension, diabetes, and the metabolic syndrome.² A survival comparable to that of the general population can therefore only be achieved by strict selection of patients undergoing AVR, which is the case in the study by Di Eusanio and associates.¹

The major shortcoming in the static comparison method is that a dynamic cohort, in this case the octogenarian study population, is being compared with a static reference cohort from the regional life tables. Changes in the patient group caused by withdrawal at different times make the study cohort dynamic and necessitate a rate adjustment to make the

comparison with the reference cohort more accurate.³⁻⁵ Rate adjustment can be achieved by matching the reference cohort with respect to age, sex, and calendar time at every moment that an event occurs in the study population. The benefit of treatment is overestimated when this mechanism is not taken into account. It would be interesting if Di Eusanio and associates were to compare the survival of the patient group with that of the age-, sex-, and calendar time-matched regional population by means of rate adjustment.

Matching should be performed not only for age and sex but also for other important demographic indicators, such as ethnicity and socioeconomic class.⁵ Unfortunately, these data are seldom available, and consequently comparisons with the general population should be interpreted carefully.

Finally, we question some of the numbers and the corresponding figures in the article. Figure 1 in the article shows a significantly lower survival of patients in New York Heart Association functional class III or IV than of those in class I or II ($P < .001$), whereas in the text a nonsignificant P value of .157 is stated. In addition, Figure 3 in the article shows a 2-year survival of 87.4% for the AVR after 80 years of age group, whereas in the text a 2-year survival of 89.7% is mentioned.

In our opinion, comparison with a reference group should be handled with caution. We suggest the development of specific guidelines to compare an accurately matched population to ensure that all factors are taken into account and that the comparison performed is methodologically correct.

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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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