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Reply to the Editor:

We thank Dr Tavlasioglu and coworkers for their interest in the double-orifice edge-to-edge technique and for the issues they addressed in their letter.

All of the patients included in our study had severe degenerative mitral regurgitation.¹ Therefore, patients with rheumatic, postendocarditic, ischemic, and functional mitral regurgitation were excluded. In our retrospective analysis, we were not able to establish exactly the number of patients who were affected by Barlow's disease and in how many cases, conversely, the typical features of fibroelastic deficiency were present. However, considering the characteristics of the patients who undergo mitral valve repair for pure degenerative mitral regurgitation at San Raffaele University Hospital (which have not significantly changed over the years), we can certainly state that the majority of the patients with bileaflet prolapse enrolled in our series were affected by Barlow's disease. The edge-to-edge technique was initially developed specifically to treat rapidly and effectively the most complex forms of this pathology. In our unit, where approximately 450 mitral repair procedures per year are performed, Barlow's disease was and remains one of

the most important indications for the double-orifice edge-to-edge repair. Tearing of the edge-to-edge suture occurred in 1 patient included in this series, probably as a consequence of an overly superficial stitch. Indeed, particularly when redundancy of the leaflets is pronounced, an appropriate depth of the suture bite is necessary to counteract the tension on the leaflets. Provided that this technical issue is respected, suture tearing has never been a real issue in our experience. The 174 cases reported in this study represent only a small proportion of the overall number of patients treated with the double-orifice edge-to-edge repair at San Raffaele University Hospital during the last 20 years. Suture tearing or rupture has been an exceptional event, and the few cases registered mainly occurred in patients with mitral endocarditis.

Another point raised by Tavlasioglu and coworkers is that "single scallop prolapse can be repaired successfully because opposing normal scallop's marginal chordae can prevent upward displacement of the previously prolapsing scallop when E-to-E repair technique is performed. However, performing the mentioned technique in cases with both A2 and P2 prolapse may not be sufficient enough to suspend the prolapsing scallops, because there are no healthy marginal chords to suspend the combined (sutured) A2-P2 scallops."

Our opinion regarding this and other similar considerations is that, as for any surgical technique, the only way to validate the edge-to-edge repair is by means of standard outcome measures and analysis. Our clinical and echocardiographic data demonstrate excellent long-term results (up to 17.6 years) in terms of freedom from recurrence of mitral regurgitation even in bileaflet and anterior leaflet prolapse, which have traditionally been associated with less satisfactory outcomes compared with posterior leaflet prolapse. Approximately 20 years after their operation, the majority of those patients

can still enjoy the well-known benefits of a successful and durable mitral repair operation. As a matter of fact, this is probably the best proof that most of the supposed drawbacks and hypothetical risks of the edge-to-edge technique are more theoretic than practical.

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CONCERNING EARLY AND LATE RESULTS OF TRAINING IN OFF-PUMP CORONARY ARTERY BYPASS SURGERY

To the Editor:

We recently read the interesting paper by Murzi and colleagues¹ in the "Cardiothoracic Surgical Education and Training" section of the *Journal*. Authors from a group with world-recognized leadership in off-pump coronary artery bypass surgery (OPCAB) compared performances of trainees with those of senior surgeons with respect to early clinical outcome and survival at a mean follow-up of approximately 5 years.

This article deserves attention for the important topic of training in surgical specialties and for some original paths of investigation, in addition to the already known large experience in systematic OPCAB of the Bristol group. We agree that multivessel OPCAB is a safe and reproducible technique that can be diffusely adopted and we concur that teaching OPCAB to cardiothoracic trainees should be a duty. Furthermore, the analysis

performed according to seniority level of residents showed good early outcome across all the Calman year group, and the evolution of the institution's program was interesting: as a matter of fact, a significant number of cases were performed by unsupervised trainees.

However, definitive conclusions cannot be driven. For example, some preoperative patients' characteristics were significantly different between the groups, and although all the study groups achieved very good clinical results, conclusions cannot be driven in a strict statistical sense. We² also compared the results of senior versus trainee surgeons a few years ago, and although we had a more homogeneous risk profile between the 2 groups, some differences still persisted similar to those observed by Murzi's group¹ (eg, prevalence of redo cases). We deem it a reasonable fact because cases were allocated in both studies according to clinical judgment and not by randomization. Also, the presumed growing number of OPCAB procedures performed

without direct supervision in the last years does not follow a linear trend (as clearly depicted in Figure 2 of the article from Bristol), and an univocal change in educational policy cannot be claimed.¹

Regarding late survival, we commend Murzi and colleagues¹ for their results and the importance of reporting long-term data. Nonetheless, owing to the increasing age of our surgical population (multiple risks of death) and to the refinement of modern health care, we think that reporting crude survival is not enough: broader analysis of follow-up data, for example, causes of death, incidence of recurrent angina, or the need for repeated revascularizations, is desirable. In the discussion of their results, Murzi and colleagues¹ complained paucity of investigations on long-term impact of coronary artery bypass grafting performed by residents or attending surgeons, but we are disappointed that they failed to acknowledge our previous contribution on mid-term results of trainees in OPCAB in our center.²

Finally, we support the Bristol group in the effort for investigation and diffusion of OPCAB. Our answer to the question appeared 2 years ago in these pages from Professors Patel and Angelini³: OPCAB is for the many! Certainly for many of the patients; safely for many of the surgeons, both senior and young.

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<http://dx.doi.org/10.1016/j.jtcvs.2012.08.078>

Notice of Correction

Re: Watanabe N, Anagnostopoulos PV, Shinkawa T, Johnson N, Azakie A. Size of the right ventricle-to-pulmonary artery conduit impacts mid-term outcome after the Norwood procedure in patients weighing less than 3 kg. *J Thorac Cardiovasc Surg.* 2012;144:1091-4.

In the above-mentioned article, the phrase "stage 1 mortality" should be "stage 1 survival." The first sentence in the Results section of the abstract is corrected as follows: The cumulative 30-day stage 1 survival was 97% (95% confidence interval, 88%-99%) for all patients (73/75), 20 of whom weighed <3 kg. The second sentence of the section Early and Interstage Results is corrected as follows: The cumulative, 30-day, stage 1 survival was 97% for all patients (73/75).