SAM. According to our data, however, there were no differences in the rate of semirigid ring usage or the average size of the artificial ring between cases with SAM and those without.⁴ The present study lacks several detailed data such as annular diameter or degree of annular reduction. Therefore, this issue lies beyond the scope of the present study and may require further investigation.

Their second comment refers to the relationship between the thickness of the interventricular septum (IVS) and the development of SAM. A bulging septum has been reported as a risk factor for SAM. Its mechanism has not been well defined, but a bulging septum may reduce the distance between mitral valve coaptation and the left ventricular outflow tract, and may increase the blood flow velocity in the left ventricular outflow tract. Said and colleagues⁵ reported 6 cases in which a bulging septum was considered the cause of SAM. According to the multivariate analysis in the present study, the thickness of the IVS is the independent risk factor for SAM. However, we could not demonstrate any cause and effect correlation between the thickness of the IVS and the risk of SAM, as seen in the ejection fraction or end-systolic diameter. We speculate that other factors such as the morphology of the interventricular septum or the diameter of the left ventricular outflow tract may influence the development of SAM. Therefore, this issue also requires further investigation.

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RAY CHU-JENG CHIU, MD, PhD, FRCSC, FACS, FACC (1934-2014) To the Editor:

Ray Chu-Jeng Chiu, MD, Professor and past Director of the Division of Cardiothoracic Surgery at McGill University, passed away on January 4, 2014. Born in Tokyo, Japan, Dr Chiu received an MD degree from the National Taiwan University in Taipei, Taiwan, followed by internship at Baltimore City Hospital. He then undertook residency training in Cardiovascular and Thoracic Surgery under Clarence Dennis at Downstate Medical Center in New York. After obtaining a PhD in Experimental Surgery at McGill University, Dr Chiu joined the faculty at that University in 1972, rising to the rank of full tenured Professor in 1981. He was Division Chief from 1992 to 2000 and spent his entire academic career at McGill University. As a caring doctor, Dr Chiu touched the lives of many patients who benefited from many of his innovative techniques and procedures, such as the spiral vena cava graft, dynamic cardiomyoplasty, retrograde coronary sinus cardioplegia, and stem cell therapy for heart failure. His contributions in the stem cell field included studies on adult stem cells derived from bone marrow, placenta, and fat. His first report on stem cell was in 1992 on cellular cardiomyoplasty, followed by his book in

1997. His publications and original contributions to the literature are extensive, and he was a visiting professor in most major universities worldwide. Perhaps the recognition and stature of Dr Chiu can be exemplified by his participation in the Nominating Committee for the Nobel Prize in Physiology and Medicine in 1982.

Ray touched the lives of all those who came in contact with him. He was an inspiration and role model for his students, residents, and peers. Ray always had a kind voice for those who searched advice from him. He encouraged and helped his trainees to do basic science and clinical research and to write manuscripts, and made them co-authors in his important contributions. Being with Ray discussing research ideas and projects was inspiring and informative, always leading to innovative and productive projects. His honesty and dedication were exemplary. His legacy will be the way in which he touched and enriched the lives of so many who were fortunate to have met him and the influence he had for those of his trainees who pursued academic and leadership careers. He will be sadly missed as one of the giants in cardiac surgery. He leaves behind his wife Jane, daughter Wendy, son Daniel, daughter-in-law Lisa, and granddaughter Anya.

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NOT CONVINCED THAT RIGHT INTERNAL THORACIC ARTERY IS SUPERIOR TO RADIAL ARTERY

To the Editor:

We read with great interest the study of Navia and colleagues¹

comparing the long-term outcomes of patients undergoing all arterial coronary artery bypass grafting (CABG) according to whether the second arterial graft was the right internal thoracic artery (RITA) or the radial artery (RA). We concur with the opinion of Navia and colleagues¹ that there is a need to raise awareness of the importance of multiple arterial grafting in contemporary CABG surgery. This has been a topic of primary interest for our group during the past decade,^{2,3} with RA as our second arterial graft of choice in conjunction with the left internal thoracic artery. Although we agree with the need for wider adoption of multiple arterial grafting in CABG surgery, we have significant concerns regarding the methodology of the analysis of Navia and colleagues¹ and believe that the data, as presented, can not support their conclusion that the RITA is a superior conduit to the RA.

Our specific issues are as follows. First, although the overall study group was composed of 1447 patients receiving bilateral internal thoracic artery (BITA) grafts and 253 patients receiving RA grafts, the many significant baseline differences between the BITA and RA groups meant that a very limited propensity-matched comparison included only 149 matched RA and BITA pairs. This small number severely limits the power of this analysis and consequently any possible conclusions that can be drawn about the impact of the differential grafting strategy on long-term outcomes.

Second, even given the very limited number of patients in the final matched comparison, we are not certain that they were adequately matched. To be able to attribute any survival differences to the differential grafting strategy in these dramatically different study groups would require a robust statistical adjustment. Unfortunately we are not

convinced that this was accomplished. The specific elements used for propensity matching are not explicitly stated. The article's Table 3¹ specifies only 17 elements that were used in deriving the propensity score. If these were the only elements used in the propensity matching, such a modest number of elements raises the possibility that the significant baseline differences among the groups may not have been adequately adjusted by this technique. Notably absent from the elements included in this process were the completeness of revascularization and the rate of transfusion, both of which have been shown to affect long-term survival.^{3,4} We have used propensity matching extensively in our analyses of the impact of the RA use on long-term outcomes and have progressively refined these techniques, relying on ever increasing numbers of patient characteristics, operative factors, and emerging peri-operative events that are increasingly recognized as important determinants of long-term survival. In a recent analysis,³ we relied on a propensity matching process composed of 50 elements. We respectfully submit that the validity of the conclusions of Navia and colleagues¹ would be significantly strengthened by a more robust propensity matching process.

Third, Navia and colleagues¹ reported a remarkably high in-hospital mortality of 5.9% in the unadjusted RA cohort and a 1.3% mortality in the BITA cohort. After propensity matching, the mortality was increased to 4.03% in the BITA cohort and was 3.36% in the RA cohort. These rates seem higher than anticipated in this relatively healthy CABG group. Importantly, these mortalities are significantly higher than the recently reported Society of Thoracic Surgeons mortality of 1.88%.⁵

Fourth, and most importantly, we disagree with the conclusion that the "study provides evidence for the

superiority of the RITA compared to the RA as a second arterial graft." Furthermore, we find no support in the data provided in the article to support the claim in the Discussion section that "the RA is not an equal alternative conduit to the RITA relative to ... survival." Specifically, in the propensity-matched study groups, the long-term survival, arguably the most important long-term outcome, was equivalent ($P = .65$). Navia and colleagues¹ also chose the relatively arbitrary end point of postoperative readmission and re-intervention-free survival as the other metric for judging the superiority of the grafting strategy. Although this clearly is an important outcome, we believe that a more detailed assessment of the components of this benchmark, such as recurrent angina, myocardial infarction, and details of the reintervention required, would be very helpful. Given the fact that the propensity matched groups were not adjusted for the completeness of revascularization (RITA vs RA distal anastomoses number 3.0 vs 2.7; $P < .001$), it is conceivable that the increased reintervention rate may have been more significantly related to this phenomenon, rather than a reflection of the quality of the specific conduit used at the index operation.

Although we believe that the data presented do not in any way answer the question in the title of the study, we still wholeheartedly applaud and support the dedication to and use of multiple arterial grafting in contemporary CABG surgery as a means of enhancing value for the patient and society in an era of increasing resource constraint.

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SHOULD SUTURELESS AORTIC VALVE REPLACEMENT BE PREFERRED ONLY FOR DECREASING AORTIC CROSSCLAMP TIME?

To the Editor:

We have read the article by Minh and colleagues¹ with great interest. We appreciate the authors' innovative approach applied to concomitant aortic and mitral valve diseases. The efforts for defining new treatment modalities have great importance and are indispensable for scientific development. However, the new approach should scientifically correct the shortcomings of the previously performed procedure. With this concern, we would like to discuss the procedure performed by the authors.

The only difference between sutureless aortic valve replacement (AVR) and surgical AVR is the decreased aortic crossclamping time. The potential advantages of the procedure are based on this fact. However, surgical steps, including sternotomy, cardiopulmonary bypass, aortic crossclamping, cardioplegic arrest of the heart, and aortotomy, are all the same when comparing both procedures. Therefore, the desired effect may not be demonstrated in the long-term results, and the superiority of sutureless AVR over conventional AVR in octogenarians may not be provided by only decreasing the aortic crossclamp time, because a large gap cannot be created between the 2 methods when considering aortic crossclamp time. In addition, cardiac damage is related to how the heart is protected rather than how long the clamp is in place.²

The prosthesis is specifically designed. Briefly, it consists of 2 cylindrical ring segments on the proximal ("outflow" ring) and distal ("inflow" ring) sides.³ Outflow and inflow rings have been seated above and below the aortic annulus, respectively. Although the sutureless anchoring system reduces crossclamping time, it produces an outward force affecting the aortic annulus during balloon dilatation of the prosthesis. The force used for dilatation was reported as 3 and 4 atm by Flameng and colleagues⁴ and Hoang Minh and colleagues,¹ respectively, that is, the pressures were equal to 2280 and 3040 mm Hg, respectively. The effects of the force on the annulus were not temporary by means of the acquired constant shape of the prosthesis. This may be the reason for permanent pacemaker implantation¹ and severe bleeding secondary to rupture of the aortic annulus.⁵ Except in the article by D'Onofrio and colleagues,⁵ the regions of life-threatening bleeding were not explained in the articles.^{1,3,4}

Determining whether the procedure is safe or not is important for the readers.

After a successful implantation, interaction between the outward

force of the prosthesis and the opposite resistance forces of the aortic annulus tissue provides tight stabilization. Loosening in the resistance forces of the aortic annulus tissue may cause paravalvular leak over time because the aortic annulus is not a stationary structure; it is always in motion. Although Hoang Minh and colleagues¹ did not observe paravalvular leak, Folliguet and colleagues³ reported 9 patients (4%) with paravalvular leak (7 patients in the early follow-up period and 2 patients in the late follow-up period).

Objective evaluation of the different aspects of the procedure may shed light on new methods to be applied in the future. This approach may be appreciated by the next generation.

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