normal conduct of cardiopulmonary bypass in which the blood flow is kept within a physiologic range, the blood pressure will be the most important determinant of cerebral perfusion.

Dr Brassard² cited studies by himself and others showing that the administration of phenylephrine and norepinephrine reduced the near-infrared spectroscopy-measured regional cerebral oxygen saturation (rScO₂) but that this measurement was not affected by equivalent doses of ephedrine. We caution, however, in interpreting these data as an indication that the cerebral oxygen saturation has been compromised by the use of vasoconstrictors. Clinically available near-infrared spectroscopy monitors use algorithms to subtract light absorption from superficial tissue (eg, scalp, bone, pia) from deeper tissue to provide the rScO₂ measurements.⁴ The importance of extracranial contamination of rScO₂ measurements has recently been highlighted in studies in which scalp ischemia induced by inflation of a circumferential cranial tourniquet affected the rScO₂ readings.⁵ Whether phenylephrine or norepinephrine induces vasoconstriction of the scalp blood vessels, rather than reduces the cerebral oxygenation per se, is an explanation of the lower rScO₂ measurements when these drugs are given has not been excluded. Furthermore, whether this source of error in interpreting the rScO₂ results has clinical importance when near-infrared spectroscopy is used as a trend monitor is unclear. More importantly, the use of rScO₂ as a surrogate of cerebral blood flow for autoregulation monitoring analyzes the correlation of rScO₂ with the mean arterial pressure at the low frequencies associated with autoregulation.⁶⁻⁸ These measurements are not dependent on the relative rScO₂ values but, rather, on the relationship between the changes in rScO₂ and blood pressure. Regardless, our ongoing randomized clinical trial of blood pressure management using

autoregulation monitoring versus the standard of care of empiric blood pressure targets during cardiopulmonary bypass (clinicaltrials.gov registration NCT00769691) could shed light on whether “hypotension” versus its treatment with vasoconstrictors affects patient outcomes. (Our study was funded in part by grant R01HL092259 from the National Institutes of Health to Dr Hogue.)

Masahrio Ono, MD, PhD^a

Kenneth Brady, MD^b

Charles W. Hogue, MD^c

^aDepartment of Cardiac Surgery
Texas Heart Institute
Houston, Tex

^bDepartment of Anesthesiology
Texas Children's Hospital
Houston, Tex

^cDepartment of Anesthesiology and
Critical Care Medicine
Johns Hopkins University School of
Medicine
Baltimore, Md

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FREE RIGHT INTERNAL THORACIC ARTERY GRAFT VERSUS RADIAL ARTERY DURING TOTAL ARTERIAL REVASCULARIZATION OFF-PUMP CORONARY ARTERY BYPASS GRAFTING: TRULY SUPERIOR?

To the Editor:

Although in their recent article Navia and colleagues¹ concluded that their study provided evidence for the superiority of the free right internal thoracic artery (RITA) graft compared with the radial artery (RA) during total arterial revascularization off-pump coronary artery bypass grafting, their data do not support this sweeping conclusion. In fact, their study showed no differences in operative mortality, perioperative complication rates, and intermediate Kaplan Meier-survivals between the propensity matched RITA and RA groups. The unmatched comparisons are not relevant because of the very significant heterogeneity between the groups. Their evidence of the RITA superiority is thus based only on the observed significant difference in the combined postoperative reintervention and readmission rates.

Navia and colleagues¹ did not specify the actual rates of reintervention or readmission, nor did they elucidate the events resulting in reintervention or readmission. Late events were also retrospectively gathered from “direct communication with the patient, their family, and attending physician,” a less than ideal means of follow-up. Complications that often result in readmission are not reported. What were the rates of sternal

infection, stroke, respiratory failure, renal failure, reoperation for bleeding, and myocardial infarction in the matched patients? What were the rates of recatheterization, and what was the graft patency? Readers need to have a better sense of exactly what events contributed to the difference in individual and combined reintervention and readmission rates. Readers also need to decide whether a better combined reintervention and readmission rate defines “superiority” of the RITA when there are no differences in perioperative adverse events or survival between the matched patients?

Finally, a very important issue is that the authors seem to have abandoned RA grafting and thus had very few patients in the RA group (only 8 of 149, or 5% of the matched group) in the last half of their 8½ year study. Why the change? What were the RA antispasm protocols? Was the RA used to bypass target vessels only with greater than 70% stenosis? What was the mode of RA graft failure? Were there graft failures at the end-to-side anastomosis of the RA to left internal thoracic artery? Was there consistency in the RA harvest technique and the harvesting personnel? The answers to these questions might help to clarify whether the harvesting technique, the deployment, or the surgical technique of RA grafting contributed to the differing reintervention and readmission rates.

Navia and colleagues¹ are to be commended for their efforts attempting to define the second best arterial graft during CABG. Their study does not, however, support their conclusion that the RITA is superior to the RA. Most other studies comparing RITA and RA, in fact, have shown better or equivalent event-free survival with the RA.²⁻⁴ In addition, we⁵ recently showed that RA grafts have similar patency to RITA grafts and that RA grafting resulted in fewer perioperative adverse events, with similar early and late survivals to RITA grafting. The debate thus continues; however,

most of the literature supports RA grafting as an excellent alternative to RITA grafting. This appears to be especially true for patients who are older or have chronic obstructive pulmonary disease or diabetes.

Robert F. Tranbaugh, MD
Kamellia R. Dimitrova, MD
Darryl M. Hoffman, MD
Beth Israel Medical Center
New York, NY

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SYSTOLIC ANTERIOR MOTION AFTER MITRAL VALVE REPAIR To the Editor:

We read with interest the article “Influence of left ventricular function on development of systolic anterior motion after mitral valve repair” by Manabe and colleagues.¹ They aimed to investigate the effect of preoperative left ventricular function on the development of systolic anterior motion (SAM) after mitral valve repair. They concluded that high preoperative

ejection fraction and small preoperative systolic left ventricular end-systolic dimension were significant risk factors for the development of SAM after mitral valve repair.

The first point that drew our attention was the article’s Table 1. As seen in that table, there were statistically significant size differences among semirigid ring, tailored ring, and Duran rings. This may disallow the standardization of these groups. In addition, the forms, elastic quality, and effects of these rings on the left ventricle are completely different. The other point that drew our attention was in the article’s Table 3. Despite the fact that the relation between SAM and ejection fraction can be explained by physics, as stated in that table, the relations between SAM and thickness of interventricular septum may not necessarily be considered as a causal correlation. Two findings in reported in that table are very worthwhile. SAM was seen in cases with end-diastolic diameter smaller than 25 mm and with ejection fraction higher than 75% (small, hyperkinetic hearts).

We believe that it is possible to avoid SAM in small, hyperkinetic hearts by suturing the free edges of the anterior and posterior leaflets together. One or two simple sutures that do not narrow the area can prevent the anterior movement of the anterior leaflet during systole and thus prevent the development of SAM.

Burcin Abud, MD
Aykut Sahin, MD
Süreyya Talay, MD
Izmir Tepecik Research and
Education Hospital
Izmir, Turkey

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