TABLE 1. Measurement of coronary flow reserve (CFR) in 20 patients after aortic valve replacement with either a Medtronic Advantage or a St Jude Medical (SJM) standard valve

Variable	Heart rate at rest (beats/min)	Coronary flow at rest (mL/min)	Heart rate with adenosine	Coronary flow with adenosine	CFR
Advantage	62 ± 10	24 ± 2.8	62 ± 10	41 ± 3.4	1.71 ± 0.3
SJM	62 ± 14	19 ± 5	60 ± 15	27 ± 5	1.42 ± 0.2
P value				.01	.2

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Is the single crossclamp technique superior?

To the Editor:

We read the article by Hammon and associates¹ with interest. We congratulate them on their excellent results and share their enthusiasm for using the single crossclamp technique. Since February 2004 we have used this technique consecutively in nearly 200 patients with no adverse neurologic sequelae. All-cause mortality in our small series is 0.5%. Despite this, we believe that the authors, having started with a good hypothesis, lost an opportunity for making a valid scientific statement, possibly because of an unintended bias.

First, the authors have called the single crossclamp technique an "ideal technique." In the rapidly evolving field of cardiac surgery, calling one particular technique "ideal" is problematic, particularly as there is no clear evidence on which to base this claim. A randomized controlled trial comparing this technique with off-pump "no touch" bilateral thoracic artery grafting would be a reasonable starting point. The inclusion of a nonrandomized selected group of off-pump patients in this study,

particularly including them in the statistical comparison, has eroded the scientific basis for the argument rather than strengthening it. We can understand the thinking involved in including this group, but sadly it raises more questions than answers.

Second, the authors have used a specially designed less traumatic clamp in the single crossclamp group, whereas the clamp used in the multiple crossclamp group was by their own admission more traumatic. When the study numbers are so small and the difference between the groups is narrow, such unintentional bias nullifies all achievements. Why they did not use the same type of clamp in both groups is difficult to understand and has not been discussed. Sadly, one could argue that the results are a representation of the differences in clamp type rather than technique.

Last, we were particularly disappointed in the lost opportunity of grading the visible atheroma on transesophageal echocardiograms and of discussing the neurologic outcome. It is accepted that the higher grade of atheroma has been clearly associated with increased risk of embolization.² There was a mention of crossover of some patients from the multiple clamp technique to the single clamp technique as a result of this finding. Was there a particular grade of atheroma that prompted the surgeons to change their practice? Was there a relation of atheroma grade to the few neurologic outcomes or, more important, was there no obvious relation, as this would support the argument to use the technique in everybody?

Our concern is that overly justified claims as to the advantages of one technique over another when not supported by clear supportive arguments in the article that follows could act as a deterrent to constructive and reasonable scientific discussions. We would have suggested to the authors and the editors to have added a question mark at the end of the title of the

paper. Rather than being the definitive answer to this question, this article still is only another contribution from this commendable group on the growing evidence in favor of an exciting technique. We would love to see a multicenter randomized trial looking at this technique in the future.

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Reply to the Editor:

We thank Drs Purohit and Zacharias very much for carefully reviewing our manuscript and making comments regarding the study design, techniques, and outcomes. We sincerely appreciate their comments related to agreeing with the single crossclamp method as a valid technique to improve results.

The authors were critical of our inclusion of a nonrandomized, selected group of off-pump patients in this study and including them in the statistical comparisons. We disagree that this weakened the conclusions from the study in that it supports the general idea that reduced aortic manipulation is a valuable technique to improve neurologic and neurocognitive outcomes.

When designing the study, we wanted to compare two techniques of intraoperative management of patients with coronary

artery disease. The first, multiple aortic crossclamping, was a tried and true method in our hospital until recently. It involved the use of an aortic clamp that we have shown generates more pressure on the walls of the aorta, as well as multiple applications of partial occluding clamps to the aorta. Our newer technique involves a softer aortic crossclamp and no partial occluding clamps at all. The dramatic difference in the 6-month neurocognitive result amply supports the newer technique.

The authors of the letter were disappointed in the lost opportunity of grading atheroma on transesophageal echocardiogram and comparing the results with neurologic outcomes. We reported in our Methods section that each patient received an epiaortic ultrasound scan of the ascending aorta and a transesophageal echocardiogram of the descending aorta. We regret that we failed to publish the fact that there were no statistically significant correlations between ascending or descending aortic atheroma and neurocognitive outcomes in any of the groups. We stated in our Methods section that we believed it was unethical to randomize patients with grade 5 atheromata to multiple aortic clamping, and we still believe that this was the correct decision, as it did not significantly influence outcome.

My coauthors and I would like to thank Drs Purohit and Zacharias for their thoughtful comments. We appreciate their careful reading of our manuscript.

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Primary sutureless repair of total anomalous pulmonary venous connection: The value of intrapleural hilar reapproximation

To the Editor:

I read with great interest the recent article of Meadows and colleagues¹ describing an elegant approach to the repair of an obstructed total anomalous pulmonary venous connection in a critically ill neonate. As clearly demonstrated, the venous confluence was very poorly developed in the patient, necessitating the use of primary sutureless

technique.¹ This technique of suturing the atrium directly to the posterior pericardium around the opened confluence of the pulmonary veins^{2,3} is now frequently used for primary repair of the total anomalous pulmonary venous connection in the adhesion-free pericardial cavity.⁴ Despite the absence of retrocardiac adhesions, operative mortality is not increased with sutureless technique as a primary procedure relative to sutureless repair as a reoperation.⁴

When used for primary repair in adhesion-free thin pericardium in a neonate, how-

ever, the sutureless technique should be applied with caution. A thorough understanding of the anatomic relationship of the pulmonary venous confluence, posterior pleuropericardial junction, and phrenic nerve is required to perform a complication-free repair.⁵ It is crucial to open the venous confluence widely to ensure an unobstructed connection. Longitudinal incision in the pulmonary veins may, however, occasionally violate the thin pleuropericardial junction (Figure 1, A), with subsequent bleeding into the pleural space.⁴ This com-

Figure 1. Transpericardial incision in pulmonary veins (A) may result in violation of thin pleuropericardial junction (B). Pleuropericardial flap is then retracted toward midline, exposing anterior aspect of pulmonary hilum (B). Parietal and visceral pleura can then be approximated to achieve hemostasis. RPA, Right pulmonary artery; SVC, superior vena cava; Ao, aorta; LPA, left pulmonary artery; LV, left ventricle; IVC, inferior vena cava; PVs, pulmonary veins.