entitled “Stentless Bioprostheses Improve Postoperative Coronary Flow More Than Stented Prostheses After Valve Replacement for Aortic Stenosis.” This intriguing magnetic resonance imaging study found that patients receiving stentless valves showed a normal coronary flow reserve (CFR) 6 months after aortic valve replacement (AVR), whereas those receiving stented valves demonstrated a slightly reduced CFR. However, we feel that a few additional comments are necessary.

In recent echocardiographic studies, reduced CFR has been demonstrated in aortic stenosis (AS) patients regardless of the presence or absence of coronary artery disease.²⁻⁶ We found a CFR improvement in AS patients 15 months after AVR with mechanical valves.⁴ This CFR improvement paralleled the regression of left ventricular hypertrophy. However, CFR did not reach normal values, thus suggesting that other factors also play a role in CFR impairment in AS. This has been strengthened by our more recent findings that CFR improvement 3 years after AVR was transient despite unchanged left ventricular mass.⁵

Overall, these results suggest that AVR with mechanical valves is associated with coronary microvascular functional alterations. Further studies in larger AS populations are warranted using more reliable techniques for CFR evaluation (such as magnetic resonance imaging) to verify our disappointing long-term results. Also, potential differences between different kinds of mechanical valves and bioprostheses should be investigated to identify the optimal therapy for coronary circulation.

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Reply to the Editor:

We appreciate the comments of Nemes and associates regarding our recent article and would like to point out the following issues in response to their comments.

The current published clinical trial followed our previous animal studies,^{1,2} in which we investigated the effects of valve design and orientation on acute changes of coronary flow in different mechanical valve substitutes and the native aortic valve. In the group in which we did not replace the aortic valve but only performed 90 minutes of extracorporeal circulation and 60 minutes of myocardial ischemia, coronary flow rates increased significantly. No mechanical aortic valve could achieve this reactive hyperemia, with superior results for the Medtronic Hall and Advantage valves compared with the St Jude standard bileaflet valve. Coronary flow rates depended not only on valve design, but also on valve orientation; the previously defined optimal orientations with respect to hemodynamics also provided the highest coronary artery flow. We explained these findings by lower intraventricular diastolic

pressures and reduced levels of aortic root turbulence in the optimal orientations.

In the recent study, our patients had no history of coronary artery disease, but angina pectoris was present in more than 50%. The improvement of coronary flow reserve (CFR) in the stentless group was independent of the left ventricular mass regression. However, we observed a trend toward accelerated regression of left ventricular mass regression in the stentless group, without any statistical significance ($P = .06$).

We share the opinion that aortic valve design has a significant influence on improvement of CFR after prosthetic aortic valve replacement. The increased long-term mortality described in recent studies in patients with severe aortic valve disease after aortic valve replacement^{3,4} compared with the normal population could be partially caused by these findings. Therefore, CFR should be included if in vivo hemodynamic performance of prosthetic aortic valves is investigated.

Regarding the optimal clinical method for measurement of CFR, we agree with Nemes and colleagues that magnetic resonance imaging can provide more objective results compared with echocardiography; conversely, the echocardiography method is less complex and easier to perform. We have just completed a retrospective study on 20 aortic valve patients (Advantage, $n = 10$; St Jude Medical, $n = 10$; interval from the operation, >6 months). Echocardiographic measurement of CFR (adenosine $140 \mu\text{g} \cdot \text{kg}^{-1} \cdot \text{min}^{-1}$ over 7 minutes) was possible in 17 patients (85%). Normal CFR was demonstrated in only three Advantage patients (Table 1). Because this was not a randomized study, we have not yet published these data, but we have initiated a randomized clinical multicenter study with echocardiographic measurement of CFR between the two bileaflet mechanical valve substitutes with large patient numbers to investigate chronic changes of CFR after mechanical aortic valve replacement.

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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