might be misleading and potentially hazardous.

Therefore, although the authors stress that the presented data are not meant to advocate central cannulation approaches over peripheral cannulation techniques, their comparison of complications and disposition between the two groups might indeed lead to the misconception that the former procedure has potential advantage in terms of clinical outcome over the latter. This conclusion does not seem to be supported by sufficient evidence.

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Reply to the Editor:

We appreciate the comments of Drs Santini and Mazzucco on our study evaluating the safety of centrally cannulating ascending aortic dissections at the University of Virginia. As they point out, our study does have the limitations that are inherent to single-institution retrospective studies. We recognized this fact in designing the study;

however, it was never our goal to prove that central cannulation is superior to the other techniques. We intentionally avoided making any statement or implication about the relative efficacy of this approach. The aim of the study was to show that central cannulation can be done safely in specific situations of ascending aortic dissection. As both Santini and Mazzucco's experience and our manuscript state, central cannulation of the dissected aorta is a technique that can be a safe option for well-selected patients. Furthermore, the response to our publication has made us aware of a broader cumulative experience with this technique. This response has been overwhelmingly positive, both with anecdotal experiences and with two separate international presentations from Germany and Japan on the technique in the past year. We would be happy to participate in a clinical trial on the optimal site of cannulation for ascending aortic dissection should one arise. Again, we appreciate the feedback from Santini and Mazzucco and hope that their input has clarified our central message that central cannulation of the ascending aortic dissection is both feasible and safe for selected patients.

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Central cannulation in acute aortic dissection repair: What else?

To the Editor:

In the article by Reece and colleagues,¹ the authors performed 24 central cannulations in acute aortic dissection repair over a series of 70 patients operated on between 1996 and 2005. The cannulation is performed over a guide wire by a Seldinger technique, after identifying the proper aortic cannulation site by transesophageal echography (TEE) and computed tomographic scan. The cannula is held firmly by hand during cooling because of the low reliability of the dissected aortic wall to hold a purse string. The results of this ap-

proach are remarkable inasmuch as none of the patients had a postoperative malperfusion. More important, the authors did not report any aortic rupture because of the direct cannulation. In light of this interesting series, one question has to be raised: why is the evidence not so obvious for everybody?

Lijoi and colleagues² were the first to report this technique in acute aortic dissection. Yet, they did not report whether they used a purse-string suture to attach the cannula. Furthermore, they did not take any precaution concerning the cannulation of the false lumen since they did not clamp the aorta before reaching deep hypothermia and subsequent circulatory arrest. In 2003, Minatoya and associates,³ from the Hanover group, reported a similar technique, but with moderate hypothermic (28°C) circulatory arrest and antegrade cerebral perfusion during arch replacement. For these authors, cannulation and perfusion of the false lumen was not a serious pitfall. At the 2006 meeting of the European Association for Cardio-thoracic Surgery, Karck and associates,⁴ from the same group, presented a series of 150 dissections over 5 years. Seventy percent were central cannulations, also without technique-related complications.

In our institution, we⁵ started routinely performing central cannulations in February 2005 in type A aortic dissection. We systematically exclude patients with a high suspicion of aortic rupture or important aortic wall hematoma. Like our colleagues in Hanover, we usually put one polypropylene 4-0 purse string in the concavity of the aorta, at the junction between the ascending segment and the arch. The perfusion of the correct lumen is assessed by TEE and by a double arterial pressure control (right radial and left femoral). A malperfusion of the true lumen is accompanied by a dramatic drop of the right radial pressure at crossclamping. In this particular case, we perform a surgical fenestration of the intimal wall at the level of the arch, during a brief circulatory arrest and after releasing the aortic clamp. Over a 2-year period, we have operated on 20 type A aortic dissections using central cannulations in 75%. All the treated patients had a reimplantation valve-sparing technique (David) and, in 80% of the cases, an arch replacement under mild (30°C) hypothermia and antegrade cerebral perfusion. None of the patients had aortic rupture dur-

ing cannulation, despite the purse string attaching the aortic cannula.

In our opinion, central cannulation is safe in acute aortic dissection repair, regardless of the systematic need for an open distal anastomosis, and the race to find the best arterial perfusion site seems useless since the evidence is right before everybody's eyes.

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Reply to the Editor:

We appreciate the comments from Drs Farhat, Sassard, and Jegaden on our article describing our experience with direct cannulation of dissected ascending aorta. They add further evidence speaking to the safety of this technique in specific subpopulations of patients with this disease. Their group uses this technique widely but avoids using it in dissections with suspected rupture or hematoma. Their exclusion criteria were similar to our study. As they describe, we have also placed purse strings in the aorta to secure the cannula without incident. This combined experience speaks to the feasibility of the central cannulation in ascending

aortic dissections, which remains a safe cannulation option in selected situations for surgeons dealing with this disease.

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A meta-analysis of minimally invasive coronary artery bypass versus percutaneous coronary intervention with stenting for isolated left anterior descending artery disease is indispensable To the Editor:

We read with great interest a meta-analysis of randomized trials of off-pump coronary artery bypass versus percutaneous coronary intervention (PCI) by Bainbridge and associates.¹ In response to a meta-analysis by Boodhwani and colleagues² of randomized trials comparing surgical versus percutaneous treatment of isolated left anterior descending (LAD) artery disease, we³ conducted a meta-analysis of then-available 5 trials of minimally invasive direct coronary artery bypass (MIDCAB) versus PCI with stenting in the form of a letter to the editor. In the meta-analysis by Boodhwani and coworkers,² significant heterogeneity existed because of the inclusion of both conventional coronary artery bypass and MIDCAB in the surgical group and both PCI with or without stenting in the percutaneous group. So far as the meta-analysis by Bainbridge and associates¹ is concerned, although 5 of 6 included trials used MIDCAB technique in the majority of patients in the surgical arm, in 1 trial by Eefting and colleagues⁴ (the largest trial in the 6 trials) surgical access to the heart was achieved via median sternotomy in 67%. Although the trial by Eefting and coworkers⁴ included patients with multivessel disease, the other 5 trials were exclusively of LAD stenting versus left internal thoracic artery-to-LAD anastomosis. Furthermore, a flaw of the systematic review by Bainbridge and associates¹ was missing a trial by Kim and colleagues⁵ published in 2005 despite comprehensive searches up until

May 2006. Therefore, we would like to advocate a meta-analysis of currently available 6 homogeneous randomized trials of MIDCAB versus PCI with stenting for isolated LAD disease, including the trial by Kim and coworkers⁵ and 5 trials except for the trial by Eefting and associates⁴ included in the meta-analysis by Bainbridge and colleagues.¹

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doi:10.1016/j.jtcvs.2007.03.047

Reply to the Editor:

Takagi and colleagues are correct in suggesting that the trial by Kim and associates¹ is relevant to our meta-analysis of randomized trials of off-pump coronary artery bypass/minimally invasive direct coronary artery bypass (OPCAB/MIDCAB) versus percutaneous coronary intervention (PCI).² We remain uncertain why this randomized trial was not identified despite multiple independent searches performed by experts. It may be that the trial was not