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Reply to the Editor:

With great interest we read the comment by Takagi and colleagues¹ with regard to the analysis of publication bias in our previous report.² Our systematic review carefully assessed the presence of publication bias by Egger's weighted regression statistic and visual assessment of the funnel plot, tools that are generally recommended for assessment of publication bias. As correctly stated by the authors, both tests revealed evidence of substantial publication bias for any of the analyzed outcomes (any atrial fibrillation: $P = .0003$; new-onset atrial fibrillation: $P = .0001$), with visual examination of the asymmetric funnel blot underscoring a small study effect. Consequently, the results of our systematic review showing a 22% reduction in the unadjusted odds for any type of atrial fibrillation in patients with preoperative statin intake are extensively discussed in light of existing publication bias, as presented in the Discussion, Results, and Limitations sections of our article.

Nevertheless, we disagree with the authors' statements that advocate a deliberate use of the trim and fill method by Duval and Tweedie² for assessment of publication bias for several reasons. The basis of the method is to remove the smaller studies causing funnel plot asymmetry and to provide an estimated adjusted intervention effect based on the filled (ie, missing) studies. However, the trim and fill method is built on the assumption that there must be a symmetric funnel plot, which may not always be true. Second, it does not take into account the true mechanisms of publication bias or reasons for funnel plot asymmetry other

than publication bias. Finally, the method is known to perform poorly in the presence of substantial heterogeneity among studies,^{3,4} which was also present in our report. These are the reasons for not using the trim and fill method in our primary analysis. When taking the aforementioned restrictions into account, the "recalculated" odds ratio provided by Takagi and colleagues for the end point "any atrial fibrillation" should be interpreted cautiously and within the limitations of the trim and fill method, because there is no guarantee that the adjusted intervention effect would have been observed in the absence of publication bias.

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TREATMENT OF RECURRENT AORTIC PROSTHETIC DETACHMENT WITH MODIFIED BENTALL PROCEDURE

To the Editor:

I enjoyed the recent article "The Treatment of Recurrent Aortic Pros-

thetic Detachment with Modified Bentall Procedure: Results of Two Cases."¹ The authors described 2 cases of surgical management using a translocated Bentall procedure with a mechanical prosthesis. I have used a similar technique with a porcine or bovine prosthesis in patients with severe endocarditis in whom a valve homograft was not available. A second useful application is in the case of an aortic root that requires replacement and a bioprosthesis is the prosthesis of choice. The technique has the advantage of being more hemostatic because a rigid mechanical or bioprosthetic ring is not positioned on the native aortic annulus at the root. In the event of bleeding at the root, it is much easier to place a suture into the cuff of the conduit rather than the rigid aortic valve prosthesis resting on the annulus.

In addition, if the aortic valve requires replacement, it is easier to change the aortic valve than redo the aortic root replacement.

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Reply to the Editor:

We appreciate the comments by Dr Frank A. Baciewicz on our article, "The Treatment of Recurrent Aortic Prosthetic Detachment with Modified Bentall Procedure: Results of Two Cases." His respectable clinical experiences briefly describe a similar technique with a bioprosthesis in patients with endocarditis or requirement of aortic root replacement and its advantages.