FIGURE 1. Meta-regression plot of male proportion (%) on standardized mean difference (SMD) of myocardial enzyme levels (coefficient, -0.022 ; $P = .098$). The area of each circle is inversely proportional to the variance of the SMD. Data taken from Zhou and colleagues.⁵

conditioning.⁴ Moreover, our recent study,⁵ including 10 randomized trials using the meta-regression method found an increased effect size (standardized mean difference) in postintervention myocardial enzyme levels of ischemic postconditioning by 0.22 per 10% increase in male proportion (Figure 1), indicating that a study with more male patients may result in a more pronounced effect than a study with fewer male patients. Hence, in their fundamental study, the authors cannot exclude the potential role of gender disparity in the quasi preservation of cardioprotection by RIPC with isoflurane, but not propofol, in CABG. At least, as the authors stated in the limitation section, both positive and negative controls are essential in one study, especially for the exploration of the role of STAT 5. The cardioprotective difference between isoflurane and propofol in the setting of RIPC needs further more rigorous studies.

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Reply to the Editor:

We thank Zhou and colleagues for their comments and interest in our recent work.^{1,2} We appreciate that they considered us to have done a series of impressive translational studies to explore the mechanisms of cardioprotection by remote ischemic preconditioning (RIPC) with isoflurane

or propofol in patients undergoing coronary artery bypass grafting (CABG) surgery.¹⁻³

Zhou and colleagues speculate that there might be an unrecognized effect of gender on the protection provided by RIPC and, thus, on our results. They underline this speculation by a meta-analysis including 10 randomized trials. However, these data are derived from studies related to ischemic postconditioning,⁴ rather than RIPC, and these 2 phenomena may be mechanistically different.⁵

In our studies, in contrast to others that have not addressed the impact of anesthetics,⁶ we showed that propofol interferes with the protection by RIPC, whereas the volatile anesthetic isoflurane² does not. Furthermore, the activation of signal transducer and activator of transcription 5 phosphorylation may be part of the mechanism by which RIPC exerts its cardioprotection during isoflurane anesthesia.^{1,3} Indeed, the gender distribution in our studies was not comparable in all subgroups.^{1,2} Certainly, therefore, effects provided by RIPC may be modified by gender, but this has not been studied. We agree that it would be worthwhile to examine whether there is a gender-specific effect of cardioprotection not only by ischemic postconditioning but also by RIPC. However, our studies were neither designed nor powered to examine a potential role of gender disparity. Therefore, we can neither exclude nor confirm gender effects on cardioprotection provided by RIPC.

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AVOIDING TRACHEOSTOMY COMPLICATION

To the Editor:

I enjoyed the recent article, “Corkscrew stenosis”: Defining and preventing a complication of percutaneous dilatational tracheostomy,” by Jacobs and colleagues¹ in the February issue of *The Journal of Thoracic and Cardiovascular Surgery*.

We have performed percutaneous tracheostomy for longer than 10 years at our Medical Center. We have not seen an increase in postpercutaneous tracheal stenosis, and have not performed a tracheal resection for this complication. When performing percutaneous tracheostomy, we attempt to place the tracheostomy lower on the anterior wall of the trachea (ie, just below the thyroid isthmus) to avoid the isthmus and subsequent bleeding into the tracheobronchial tree.

If the tracheostomy with dilator does not pass easily into the trachea, we dissect the subcutaneous tissue and/or dilate the tracheal opening with a standard tracheal dilator or

clamp. These steps usually allow easy passage of the tracheostomy.

We instruct our bronchoscopist, usually the anesthesiologist, to pull back the endotracheal tube so that we enter the trachea without impaling the endotracheal tube. Under these circumstances, the endotracheal tube is just proximal in the airway to where the tracheostomy is being inserted. When the endotracheal tube is positioned just proximal to the tracheostomy insertion site, anterior tracheal ring fracture is prevented (as described by the authors).

We agree with the authors that the positioning of the supportive endotracheal tube is important in preventing anterior tracheal ring fracture. We also believe that placing the tracheostomy just inferior to the thyroid isthmus, which may serve as a cushion for the anterior tracheal ring, and dilating the skin incision or tracheal opening itself have prevented us from experiencing this complication.

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<http://dx.doi.org/10.1016/j.jtcvs.2013.03.042>

Reply to the Editor:

We greatly appreciated the thoughtful review of our article provided by Dr Baciewicz. Furthermore, the suggestions offered by Dr Baciewicz are of great interest and work well at his center, where he has not seen “corkscrew” tracheal stenosis after percutaneous dilatational tracheostomy (PDT)

in the same way that we have been receiving them at our referral center.

We agree that adequate dissection of soft tissue anterior to the trachea is critical in avoiding excessive force and torque when advancing the dilator, which can lead to tracheal ring fractures, as described. Because this is a minimally invasive means of providing a tracheostomy, there is often a tendency to make the skin incision as small as possible. This should be avoided. An incision large enough to accommodate the dilator and then the tracheostomy without great resistance should be made (at least 1.5-2 cm). Furthermore, using a clamp to dissect and spread the soft tissue anterior to the trachea is similarly important and done routinely at our institution.

Dr Baciewicz also describes dilating the actual trachea with a tracheal dilator or clamp when there is difficulty passing the tracheostomy with the dilator. This was first described during the percutaneous technique by Griggs and colleagues¹ in 1990 using guidewire dilating forceps. Since then, this technique has been compared with the single-step curved dilator technique in multiple small randomized studies, most of which found the 2 techniques similar.²⁻⁴ We use the Griggs technique in a few of our cases (3%) when we encounter a trachea that is particularly difficult to dilate. These tenacious tracheas are most often encountered in young patients, and we agree that the Griggs technique is an important maneuver that should be in the armamentarium of those performing PDTs.

We appreciate the comments provided regarding the location of placement along the trachea. Dr Baciewicz recommends placing the tracheostomy lower along the anterior tracheal wall just inferior to the thyroid isthmus. We agree with the “low” placement of the tracheostomy to a certain extent. The tracheostomy placement should be low enough that it does not risk injuring the cricoid