

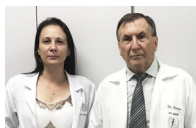
Authors have nothing to disclose with regard to commercial support.

Derrick Y. Tam, MD
 Stephen E. Fremes, MD, MSc
 Division of Cardiac Surgery
 Schulich Heart Centre
 Department of Surgery
 Sunnybrook Health Sciences Centre
 University of Toronto
 Toronto, Ontario, Canada

References

1. Allen KB, Thourani VH, Naka Y, Grubb KJ, Grehan J, Patel N, et al. Randomized, multicenter trial comparing sternotomy closure with rigid plate fixation to wire cerclage. *J Thorac Cardiovasc Surg*. 2017;153:888-96.e1.
2. Schimmer C, Reents W, Berneder S, Eigel P. Prevention of sternal dehiscence and infection in high-risk patients: a prospective randomized multicenter trial. *Ann Thorac Surg*. 2008;86:1897-904.
3. Schimmer C, Sommer S-P, Bensch M, Bohrer T, Aleksic I, Leyh R. Sternal closure techniques and postoperative sternal wound complications in elderly patients. *Eur J Cardiothorac Surg*. 2008;34:132-8.

<https://doi.org/10.1016/j.jtcvs.2018.02.072>



MORE WIRES, MORE TIME

Reply to the Editor:

We thank colleagues Tam and Fremes for their interest in our meta-analysis, and for the opportunity to defend our review without taking

advantage of either of the 2 interventions used. With regard to the completion of the review, we at no time assert that the method should not be used. In our abstract conclusions, we wrote that steel wire anchoring techniques to prevent bone breakdown in sternal closure make little or no difference relative to standard closing in high-risk patients. In our full article Conclusions section, we noted that in this review we could say that there is moderate quality evidence that anchoring techniques involving steel wires to prevent bone rupture (nonstandard) for the closure of median sternotomies (Robicsek original or modified or stapler), probably make little or no difference relative to the standard closure in the postoperative period of patients with risk factors. The findings of this review can be applied to sternal closure in high-risk patients undergoing elective cardiac surgery. These findings cannot be applied to reoperation for correction of sternal separation if it has already occurred. What Tam and Fremes say was stated in our conclusions was actually said in the perspective statement:

nonstandard closure is more expensive and time-consuming. Because nonstandard closure was shown to make a nonsignificant difference, a standard closure should be preferred where resources and time are limited. We understand that the perspective statement provides a particular way of considering something to express an opinion. This is simply our opinion, which could appropriately be inserted in the discussion but not the conclusions. If we had studied the costs, it would have been in the conclusions; it was only our surmise, however, that you spend more on wire, because more wires are needed to make the anchorage, and also that you spend more time to pass these wires. There is no doubt that we cannot stop time to pass these wires.

Even if the primary studies had a comparison of costs, such as that made by Allen and colleagues,¹ we would not dare put such in our review, because the costs of hospitalization in our country are very different from the costs in several other countries where primary studies were conducted (Turkey, India, and Germany). Allen and colleagues¹ studied costs at 12 US centers, so their costs are valid in the United States and probably not in those other countries. We did not want to compare anything; we simply said that more wires are needed to make the reinforced parasternal wiring technique and that we need additional time to pass these wires. With regard to the introduction of the “any sternal complication” outcome, this outcome was considered when we defined the protocol of the review but discarded because we found it to be very vague, and we do not know whether it would contribute to the study because we would be adding entities with many different clinical meanings. With regard to sternal dehiscence, all 7 studies report this outcome; they cannot be combined, however, because only some authors of the primary studies considered dehiscence to include both deep sternal wound infection (DSWI) and sternal instability (SI), which was dehiscence of the sternum without infection. According to Aykut and associates,² “There were 7 patients (9.3%) with sternal dehiscence in this group. Two (2.6%) of these patients experienced mediastinitis caused by *Staphylococcus aureus* and underwent reoperation. Another 5 (6.6%) patients in this group had noninfectious sternal dehiscence that was defined as sternal instability without fever, leukocytosis, or serosanguineous or purulent discharge from the incision.” Thus 2 patients were considered to have DSWI and 5 to have SI. The 5 dehiscences in the Iriz study were placed in the meta-analysis as DSWI. In the Narang study, the dehiscences were noninfective sternal dehiscence, considered as SI. Okutan did not have either DSWI or SI. Schimmer and colleagues^{3,4} considered dehiscence to encompass only SI; DSWI was another outcome studied separately. Sharma, on the contrary considered dehiscence to be DSWI, whereas SI was the previous occurrence leading to dehiscence. We therefore see that dehiscence

Authors have nothing to disclose with regard to commercial support.

was defined in different ways, preventing the combination to perform the meta-analysis, whereas DSWI and SI were easy to combine according to the definitions presented by the authors, and included dehiscences.

With regard to the sensitivity test, if we withdraw the Schimmer and colleagues³ study, the heterogeneity drops to zero in the DSWI outcome; this would not be correct, however, because we would be excluding the best study (multicenter, with a greater number of cases, lower confidence interval in this outcome, and lower risk of bias). We can perform sensitivity tests with doubtful studies, but we cannot remove that of Schimmer and colleagues³ only because their results differ from other studies and increase heterogeneity. In fact, the weight of that study is very high, because it has the highest number of cases and a very small confidence interval. We therefore did not consider it necessary to perform an n-fold cross-validation test to withdraw a study on the basis of statistical data; rather, we considered that the rational evaluation of each study was most important, so the inclusion of these was done with the agreement of all authors. For Schimmer and colleagues' studies, there is in fact an overlap in the study period, because one was performed from 2005 to 2006 and the other from 2006 to 2007; although the inclusion criteria were different (one study included only elderly patients >75 years of age and the other included every patient with a risk factor), there may have been double counts of some patients from the year 2006. We thought that the withdrawal of the Schimmer and colleagues⁴ study that included only elderly patients would be unfair to those who believe in the technique of Robicsek, but its withdrawal does not change the results. See the results with the 2 Schimmer and colleagues studies and without the Schimmer and colleagues study with elderly patients,⁴ respectively. With both studies, DSWI relative risk (RR) was 0.42 (95% confidence interval [CI], 0.11-1.59); without the one, RR was 0.45 (95% CI, 0.10-2.03). SWI was only analyzed in 1 Schimmer and

colleagues study (Schimmer and colleagues³). With the studies, RR for death was 0.93 (95% CI, 0.47-1.84); without the study with elderly patients, RR was 0.98 (95% CI, 0.48-1.99). SI continues without combination conditions because of high heterogeneity. With regard to the subgroup analysis for the primary end points, including only the studies that used original Robicsek also will not change the results. For DSWI, the result would be RR of 0.74 (95% CI, 0.18-3.10); for SWI, it would be RR of 1.52 (95% CI, 0.87-2.68).

Thanks again to our colleagues Tam and Fremes for their important comments. We congratulate them on the expertise shown in meta-analyses already published, especially that presented at the Scientific Forum of the American College of Surgeons in October 2017,⁵ which deals with the same theme.

Antônio J. M. Cataneo, MD, PhD^a

Daniele C. Cataneo, MD, PhD^b

^aDivision of Thoracic Surgery

Department of Surgery

Botucatu School of Medicine

São Paulo State University

São Paulo, Brazil

^bDepartment of Surgery

Botucatu School of Medicine

São Paulo State University

São Paulo, Brazil

References

1. Allen KB, Thourani VH, Naka Y, Grubb KJ, Grehan J, Patel N, et al. Randomized, multicenter trial comparing sternotomy closure with rigid plate fixation to wire cerclage. *J Thorac Cardiovasc Surg*. 2017;153:888-96.
2. Aykut K, Celik B, Acikel U. Figure-of-eight versus prophylactic sternal weave closure of median sternotomy in diabetic obese patients undergoing coronary artery bypass grafting. *Ann Thorac Surg*. 2011;92:638-41.
3. Schimmer C, Reents W, Berneder S, Eigel P, Sezer O, Scheld H, et al. Prevention of sternal dehiscence and infection in high-risk patients: a prospective randomized multicenter trial. *Ann Thorac Surg*. 2008;86:1897-904.
4. Schimmer C, Sommer SP, Bensch M, Bohrer T, Aleksic I, Leyh R. Sternal closure techniques and postoperative sternal wound complications in elderly patients. *Eur J Cardiothorac Surg*. 2008;34:132-8.
5. Tam DY, Nedadur R, Yu M, Fremes SE. Rigid plate fixation vs conventional wire cerclage closure for sternotomy after cardiac surgery: a systematic review and meta-analysis. *J Am Coll Surg*. 2017;225(4 Suppl 1):S34.

<https://doi.org/10.1016/j.jtcvs.2018.03.021>