References


https://doi.org/10.1016/j.jtcvs.2018.02.013

MORE THAN ONE WAY TO WIRE A CHEST

To the Editor:

We read with great interest the meta-analysis by Pinotti and colleagues that examined 7 randomized controlled trials comparing a standard sternal closure technique with a reinforced parasternal wiring technique in 2138 patients. Pinotti and colleagues conclude that a parasternal wire closure is more expensive and time-consuming, and should not be used given that there are no differences in sternal complication rates between standard care and anchoring techniques.

We raise some concerns with regard to the authors’ conclusions and include some suggestions to further evaluate the robustness of their analysis. First, their conclusion regarding the parasternal technique being more expensive and time-consuming is not supported because the authors did not examine the operative time or cost differences between the treatment groups in their analysis. In the absence of a formal economic evaluation, it would be inappropriate to draw conclusions regarding the cost consequences of adopting (or not adopting) the parasternal wiring technique. Allen and colleagues showed that index hospitalization cost was higher with rigid plate fixation compared with wire cerclage because of the high acquisition costs of sternal plates. However, rigid plate fixation was cost-effective at 6 months, driven by higher rates of readmission in the wire cerclage group. Second, although there was no difference for the primary outcome of deep sternal wound infection, we wonder whether the inclusion of a composite outcome of “any sternal complication” or the reporting of additional clinically relevant outcomes such as sternal dehiscence (reported in 5 of the studies) may have demonstrated a difference between the 2 arms. Finally, the presence of moderate heterogeneity seen in the outcome of deep sternal wound infection requires additional post hoc sensitivity analyses to assess the robustness of the findings and look for potential causes of heterogeneity. We advocate that a leave-one-out analysis of the robustness of the results may be critically important because the removal of the study by Schimmer and colleagues2 (responsible for 39.1% of the weighting) may have a significant impact on the outcome. A leave-one-out analysis is also prudent in this specific study because they elected to include 2 studies that shared a potentially overlapping population that in essence “double counts” the patients included. The use of post hoc subgroup analyses may also help determine the cause of significant heterogeneity. It would be interesting to examine whether differences existed for the primary outcome between those treated with a modified Robicsek versus the traditional Robicsek closure when compared with standard closure.

Again, we commend the authors for drawing their attention to this very important issue in cardiac surgery. However, the optimal sternal wire closure technique to reduce sternal complications remains uncertain. We believe that any technique that follows the principles of bone healing, namely, reapproximation, stabilization, and immobilization through some form of rigid fixation, will allow the sternum the optimal environment to heal.
MORE WIRES, MORE TIME

Reply to the Editor:

We thank colleagues Tam and Fремес 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 breakage 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 Fремес 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 hospitalsization 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 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 colleagues3,4 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

References