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doi:10.1016/j.jtcvs.2008.02.056

Reply to the editor:

I appreciate the earnest comments of my colleagues.

Evidently, we see the potentials of tele-robotics in a different perspective. They believe not only that robotics is the future of cardiac surgery, but also that the future is already here! I respectfully disagree.

They kindly recall my role as an “innovator.” With my 12 patents (the last one in 2008), I would not deny it. However, I was also the one who, as the invited discussant to the first lecture of Andreas Gruntzig, stated that coronary balloon angioplasty will have an unacceptable recurrence rate, and I also proved soon after its introduction that aortic balloon valvuloplasty will be a disaster. To be an innovator means not to “change” but to change toward the better.

In today’s era of evidence-based medicine, the rules of innovation are strict. Informed consent is not a nebulous concept anymore. The margins of clinic and industry are better defined. Novel methods must undergo intense scrutiny not only from a clinical standpoint but also from an ethical and economic aspect. Because results of telerobotics might have reached, but certainly did not exceed, those obtained with simpler minimally invasive methods and because telerobotics has definite marketing undertones, with its costs now exceeding a billion dollars in equipment alone, we are obligated to ask specific questions. In my editorial I was asking these questions with an open mind but also with a healthy dose of old-fashioned skepticism.

The closing words of my editorial were these:

Elements of robotics could indeed be part of the future technology of minimally invasive cardiac interventions. The proper way to proceed with this endeavor however, is not to flood the market with billions of dollars worth of hardware, most of it already obsolete, but to limit their use to a few centers with sincere professional interest, until in well-controlled trials, the technology proves itself economically sound, as well as clinically superior to that which we already have today.

The proof is on you, gentlemen.

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Simultaneous hybrid coronary revascularization reduces postoperative morbidity compared with results from conventional off-pump coronary artery bypass

To the Editor:

This study describes a single-center experience with hybrid procedures in a small patient group of 15 hybrid patients compared with a group of 30 matched off-pump patients. The study has 2 major shortcomings I want to address. First, the grafts used differed between the patients because vein grafts, bilateral internal thoracic arteries, and radial arteries were used. The choice of graft influences patency, as well as potential blood loss. Second, the blood loss in their off-pump group was rather high (>1000 mL). Third, and most important, the quality of revascularization remains an issue in stent implantation. Although the authors tried to address this, they used the wrong method because computed tomographic angiography does not allow for sufficient detection of in-stent restenosis. Additionally, the group sizes are too small to allow for a determination of a trend toward better 1-year graft patency; a single stent occlusion would turn the whole trend. Finally, off-pump coronary artery bypass does not require occlusion of coronary arteries for 8 to 12 minutes for each anastomosis; by inserting a shunt, which is standard in most centers, this occlusion can be reduced to less than 1 minute. For these reasons, the conclusions drawn from the results of this study are that the hybrid procedure is possible, even with good clinical long-term results. The study does not provide sufficient data to prove any superiority of the hybrid approach compared with the off-pump coronary artery bypass procedure with complete surgical revascularization.

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Reply to the Editor:

A small single-center study cannot provide definitive answers to any of the important questions posed by Dr Gulbins. We agree that our data do not support the conclusion that the hybrid approach is superior to the off-pump coronary artery bypass (OP-CAB) procedure. An important goal of this article is merely to provoke debate about how innovations such as this might be used to improve the results for surgical revascularization.

There is no question that differences in clinical outcomes between the 2 groups were driven by variables other than the use of a small thoracotomy instead of a median sternotomy. For example, differences in the choice of conduits undoubtedly influenced outcome. A key advantage of the hybrid procedure is the use of percutaneous coronary intervention/stenting as a means of avoiding the well-known shortcomings of saphenous vein grafts that is used in more than 95% of coronary artery bypass grafting procedures done through a sternotomy. Computed tomographic angiographic follow-up is not adequate to detect in-stent restenosis, but the use of coated stents according to the entry criteria of our study minimized the expected risk of this problem to less than 10% in our cohort.¹ The risk of thrombosis is increased with these types of stents, but thrombosis has a dramatic presentation that can be reliably diagnosed by clinical means. Therefore the advantages provided by the use of invasive angiography instead of computed tomographic angiography for investigating the benefits of using stents versus the saphenous vein graft in our cohort would have been modest.

We agree that the blood loss in the OPCAB group was higher than has been reported by other groups. This might have reflected a more rigorous assessment of intraoperative losses, including the volume of blood retrieved by the cell saver; weighing of sponges; and estimation of other losses. It also might reflect a higher-risk population of patients than have been enrolled in prior OPCAB analyses. Nonetheless, the blood loss and rate of transfusions for the minimally invasive group was clearly less than has been reported in most other OPCAB reports.²

Finally, we agree that OPCAB does not require occlusion of coronary arteries during each anastomosis but disagree that the routine insertion of a shunt is the standard by which most cardiac centers practice OPCAB. There are concerns about the risk of coronary injury with the insertion of a shunt,³ particularly in the population of patients referred for coronary artery bypass grafting with poor-quality targets. Because this injury might also provoke regional inflammation and thrombosis, we have used shunts selectively for those with evidence of ischemia after brief coronary occlusion. Admittedly, this is an area that requires further analysis.

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Minimally invasive implantation of a cardioverter in children

To the Editor:

We read with interest the case report of Snyder and coworkers¹ regarding the implantation of a cardioverter in a 3-year-old child with a minimally invasive approach. In this case the authors used a dual-coil shocking lead looped around the heart and fixed on the epicardium. The sensing electrode was set on the right ventricle, and the implantable cardioverter-defibrillator was secured in the left abdominal wall.

We fully agree with the authors that defibrillators should be considered more often in the pediatric population and that an epicardial approach is the only acceptable route in this population.² However, we have some concerns with their proposed approach. The looping coil might lead to progressive compression of epicardial vessels (coronary veins and even arteries) with the patient's growth and might further get dislodged or even break with growth. Breakage is prone to occur when the coil is readily fixed at different points (here by adhesions and suturing on the heart), and the risk is greater in a dual coil than in a simple one. Finally, devices set in the lower abdominal wall result in a large gap between electrodes, coils, and device with growth (much larger than when set in the thoracic cavity) and can induce abdominal complications.³

We have also developed an epicardial technique for this population in which the heart is set between the coil and the defibrillator, which acts as an active pole.⁴

The coil is positioned under the parietal pleura at the apex of the left thorax, and the defibrillator is positioned underneath the heart, within the diaphragm layers (between the aponeurosis and muscle). Sensing electrodes are positioned on the epicardium of the left ventricle (which has superior sensing parameters compared with the right ventricle) (Figure 1).⁵ The leads are looped in the left pleural cavity. Growth results in a progressive unlooping of the leads but without any tension on the heart and with minimal modification of the relationship between the coil and the defibrillator, which basically remain around the heart. Like the authors, we avoid a sternotomy. Change of an element of the defibrillator because of a structural failure (if any should occur with time) would not be a problem because of the absence of significant adhesions on the heart.

We have used this technique in 14 children with ages ranging from 2.9 to 13.2 years, with excellent sensing and defibrillation thresholds (successful defibrillation is usually achieved with 10 to 15 J). The Medtronic wireless communication system allows effective remote telemetry of the device (something not possible with any telemetric system). We have not seen any structural failure caused by somatic growth.

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Figure 1. Frontal and lateral chest radiographs of a 6-year-old boy with subpleural placement of the defibrillator electrode and intra-diaphragmatic position of the device.