Letters to the Editor

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- Type with double-spacing
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To the Editor:

We carefully read the interesting article by Bainbridge and colleagues¹ comparing, by means of a meta-analytic approach, the available evidence on off-pump coronary artery bypass surgery (OPCABG) with percutaneous coronary intervention (PCI). Six trials involving 989 patients were included. All but 1 trial included only patients with isolated proximal left anterior descending disease. Only 1 trial used paclitaxel or sirolimus-eluting stents. The authors concluded that OPCABG is associated with better short-term and midterm clinical outcomes but also with an increased length of hospital stay.

The authors correctly suggested projecting the results to a population larger than that of the included studies. For example, for every 1000 low-risk patients undergoing OPCABG, there will be approximately 105 fewer patients with recurrent angina, 143 fewer patients with target vessel failure, and 125 fewer patients with major adverse cardiovascular events (MACE) at 1 to 5 years. These statements are consistent with the findings of the meta-analysis, but some issues deserve attention and a deeper examination to avoid possibly misleading conclusions.

One of our concerns is the reliability of the results considering the widespread diffusion of drug-eluting stents (DES), which greatly reduced the risk of restenosis compared with bare metal stents in short-, mid-, and long-term follow-ups. Only 1 study adopted DES and kept the double antiplatelet therapy for only 6 months. A high rate of restenosis probably fixed the difference observed in the rate of target vessel failure and then of MACE. It is conceivable, and the authors correctly did not exclude such a possibility, that the results of a comparison between DES and OPCABG may be different particularly when considering the risk of recurrent angina and reintervention, and probably of MACE, although there is no clear evidence of the superiority of DES over bare metal stents for this point.²

On analysis of Table 3,¹ some points require further in-depth evaluation. All of

the end points that significantly differ between OPCABG and PCI can be ascribed to the “restenosis” phenomenon because death, myocardial infarction, and stroke did not significantly differ. The risk of death, in particular, deserves a larger discussion. The difference did not reach statistical significance, and the confidence interval was wide (ie, the events were few and/or the population was small and underpowered), but, in absolute terms, and this point is not negligible in our opinion, such risk was more than 3-fold higher with OPCABG than PCI in the hospital and approximately 2-fold higher at 6 months and at 1 to 5 years. Thus, we think that it is at least questionable to state that OPCABG is as safe as PCI.

Moreover, most of the included population had isolated proximal left anterior descending disease: This point is crucial. As available evidence referred to a medical “environment” in which the use of DES is widespread, current guidelines advise against PCI only in the setting of left main disease, isolated or associated with a larger coronary disease.² Evidence-based medicine is mandatory.

The results of such a well-conducted, well-written meta-analysis would probably have been useful as an interpretative tool a few years ago. Today, the continuous progress of the percutaneous approach to coronary artery disease in terms of materials and techniques allows the physician to propose a less-invasive approach with an excellent chance for good results; thus, it may be misleading to state the “superiority” of OPCABG in the “Conclusion” section.

We think that an updated comparison of OPCABG versus PCI with DES is necessary, but one point finally remains: One of the duties of any physician is to suggest a range of possibilities (when available) to treat the same disease.

In our experience of research³ and medical practice in different countries, one of the most important things to take into account is the patient’s will. Even if this matter is not “evidence based,” we think that when choosing between two strategies

everyone would prefer the less-invasive one. This issue is not quantifiable; on the other hand, suggesting the superiority of the OPCABG over PCI for a single-vessel disease on the basis of a non-updated analysis may be misleading for those physicians aiming at explaining a complete “state of the art.”

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Fixed subaortic stenosis

To the Editor:

We read with great interest the article entitled “Geometry of the Left Ventricular Outflow Tract in Fixed Subaortic Stenosis and Intact Ventricular Septum: An Echo-

cardiographic Study in Children and Adults.”¹ We have a few points to discuss.

First, we agree with the authors in choosing the wider mitral-aortic separation, steeper aortoseptal angle, smaller left ventricular outflow tract width, aortic valve dextroposition, increased left ventricle wall thickness, increased septal thickness, and indexed left ventricular end-diastolic and end-systolic diameters as the echocardiographic parameters to check in cases of subaortic stenosis.

Second, we think that left ventricular outflow tract obstruction in cases of Shone complex is a true challenge. We prefer not to intervene early, giving the heart and body time to grow, which is contrary to the general preference to intervene early in the other cases of subaortic stenosis. Discrete subaortic stenosis can be cured in most patients by membranectomy associated with myotomy or myectomy. Because the anatomic substrate is not addressed by these surgical techniques, however, recurrences are likely during long-term follow-up, particularly in patients who have undergone previous operations for an aortic coarctation and in patients with less than optimal relief of the left ventricular outflow tract gradient. In this subset of patients, the optimal surgical technique remains to be described. Intraoperative recording of the left ventricle-aorta gradient by transesophageal echocardiography or pressure measurement remains an important tool for more aggressive subaortic resection in case of a residual gradient greater than 30 mm Hg. According to the size and function of the aortic valve, the Ross-Konno procedure or the modified Konno procedure by patch septoplasty seems to be the appropriate surgical technique. Tunnel subaortic stenosis represents a more severe and challenging cause of left ventricular outflow tract obstruction, particularly when symptoms start early in life. Although multivariate analysis did not demonstrate this anatomic form as an independent risk factor for overall mortality and recurrence rates, in univariate analyses it was associated with a higher overall mortality rate and reoperation rate, along with other factors of the Shone complex (ie, hypoplastic aortic anulus, mitral stenosis, and existence of coarctation).²

Third, we have a remark on Table 3. We expected the indexed left ventricular end-systolic dimension to decrease postoperatively especially in group 1, which was not

the case. The left ventricular end-systolic dimension increased in group 1, although it decreased in groups 2 and 3. Do you have an explanation?

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Reply to the Editor:

We thank Drs Sersar, Jamjoom, and Baslaim for their comments on our study.¹

Shone complex was originally described as comprising 4 coexisting obstructive lesions: Supravalvar ring of the left atrium, “parachute” deformity of the mitral valve, subaortic stenosis, and coarctation of the aorta are considerably more challenging than is an isolated subaortic stenosis.

On the basis of mean values, there are some differences in the left ventricular end-systolic dimension between groups, despite being statistically insignificant. This may be attributable to differences in left ventricular compliance to changes in loading conditions. This remains to be reexamined in a larger group of patients.

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