IMPACT OF METHODOLOGY AND ASSUMPTIONS IN A COST-EFFECTIVENESS ANALYSIS ON TRANSCATHETER AORTIC VALVE REPLACEMENT

To the Editor:

With great interest we read the recent article on cost-effectiveness of transcatheter aortic valve implantation (TAVI) by Doble and colleagues.¹ We congratulate Doble and colleagues¹ on their well-designed analysis of this timely and important topic. They reported a base case incremental cost-effectiveness ratio (ICER) of \$51,324/quality-adjusted life-year for TAVI versus standard management in surgically inoperable patients. In high-risk patients, TAVI was economically dominated by surgical aortic valve replacement (SAVR). Doble and colleagues¹ concluded that TAVI is a cost-effective treatment option for inoperable patients but not for high-risk patients. However, some methodologic issues and questionable assumptions influenced this conclusion.

The quality-of-life utilities in the article of Doble and colleagues¹ were based on a conversion of New York Heart Association functional classes, although direct EQ-5D utilities from the PARTNER (Placement of Aortic Transcatheter Valve) trial have been available since their presentation November 7, 2011, at the Transcatheter Cardiovascular Therapeutics 2011 Interventional Conference in San Francisco.² Moreover, TAVI through the

transfemoral route is associated with improved quality of life compared to surgery, whereas the same has not been demonstrated for the transapical route.³ Doble and colleagues¹ lumped the quality-of-life improvement with these 2 distinct techniques together and reported 0.102 less quality-adjusted life years after TAVI than after SAVR. This decrease is inconsistent with the quality-of-life results of transfemoral TAVI in the PARTNER trial, and its use resulted in a too pessimistic ICER for TAVI versus SAVR in high-risk patients.

Doble and colleagues¹ used Canadian life tables to simulate long-term survival in all treatment groups, whereas a survival comparable to that of the general population is highly unlikely.⁴ Table 2 in the article of Doble and colleagues¹ shows that the method for extrapolating survival had a large influence on the ICER of TAVI versus SAVR. A better approach would have been to fit survival curves separately for the treatment groups with Weibull, log-normal, and other models. In that way comorbidities such as diabetes mellitus, coronary artery disease, and previous myocardial infarction could have been taken into account as covariables.

The inputs for the model came from a variety of sources, and some assumptions are questionable. Although unadjusted costs of balloon valvuloplasty were directly plugged in from a 23-year-old study, Figure 2 in the article¹ showed that these costs actually have a major influence on overall cost-effectiveness. Also, the investigators used an excessive 36-day hospital stay after SAVR and based the procedural costs of SAVR on those for septuagenarians, rather than high-risk octogenarians. Furthermore, the short-term probability of acute kidney injury was estimated at 0.112, whereas 0.011 seems appropriate according to the PARTNER cohort B data.⁵

Doble and colleagues¹ mentioned that the ICER of TAVI versus SAVR would have been more favorable

toward TAVI if the costs of the TAVI procedure had been lower. It would have been interesting if they had reported what the price of a TAVI valve would need to be for it to be a cost-effective alternative to SAVR.⁶ Also, an elaboration on the differences with the PARTNER cost-effectiveness analyses would have been valuable.

Methodology and assumptions influence the estimated outcomes in cost-effectiveness analyses. It would be interesting to see what the impact of our remarks would be in a re-analysis of the cost-effectiveness estimates of TAVI.

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