such risk-mitigating actions as CT screening and smoking cessation.

Robert P. Young, BMedSc, MBChB,
DPhil, FRACP, FRCF^a
Raewyn J. Hopkins, BN, MPH^b
^aSchool of Biological Science
^bFaculty of Medical and Health
Sciences
University of Auckland
Auckland, New Zealand

References

1. Jaklitsch MT, Jacobson FL, Austin JH, Field JK, Jett JR, Keshavjee S, et al. The American Association of Thoracic Surgery guidelines for lung cancer screening using low-dose computed tomography scans for lung cancer survivors and other high risk groups. *J Thoracic Cardiovasc Surg*. 2012;144:33-8.
2. Bach PB, Mirkin JN, Oliver TK, Azzoli CG, Berry DA, Brawley OW, et al. Benefits and harms of CT screening for lung cancer: a systematic review. *JAMA*. 2012;307:2418-29.
3. Young RP, Hopkins RJ. Lung cancer risk prediction to select smokers for CT screening—letter. *Cancer Prev Res*. 2012;5:697-8.
4. Woloshin S, Schwartz LM, Welch HG. The risk of death by age, sex, and smoking status in the United States: putting health risks in context. *J Natl Cancer Inst*. 2008;100:845-53.
5. Young RP, Hopkins RJ, Whittington CF, Hay BA, Epton MJ, Gamble GD. Individual and cumulative effects of GWAS susceptibility loci in lung cancer: associations after sub-phenotyping for COPD. *PLoS One*. 2011;6:e16476.
6. Young RP, Hopkins RJ. Increasing smokers' risk perception improves CT screening participation. *Thorax*. 2012;67:834-5.

<http://dx.doi.org/10.1016/j.jtcvs.2012.08.074>

Reply to the Editor:

We thank Drs Young and Hopkins for their kind remarks in favor of the American Association for Thoracic Surgery guidelines for lung cancer screening with low-dose chest computed tomographic scans. In creating guidelines on the basis of a successful research protocol, the Association chose to expand the age of the screening population beyond the ages of the participants in the National Lung Screening Trial.¹ The rationale behind this decision included the age distribution of the disease in North America, the fact that increasing age is an independent risk factor for development of cancer, and the improvement in

quality life years expected up to the 9th decade of life. Drs Young and Hopkins offer additional justification for this guideline, namely the age-specific lung cancer mortality in the United States increases exponentially after the age of 50, with a peak at the age of 80 years.

We envision a Web-based program that would allow each citizen to calculate his or her own absolute risk of lung cancer, dissemination of easily updated educational materials, and potential data collection for specific populations. It is our hope that such risk assessment would in turn lead to risk modification and smoking cessation as integrated components of patient care. Personal risk calculators are currently available, but they are not easily accessible to the public. Such a Web-based tool might convert a guideline or instruction into a conversation between physician and patient, including the opportunity to further patient interest in smoking cessation.

Michael T. Jaklitsch, MD
Francine L. Jacobson, MD, MPH
Brigham and Women's Hospital
Boston, Mass

Reference

1. National Lung Screening Trial Research Team, Aberle DR, Adams AM, Berg CD, Black WC, Clapp JD, et al. Reduced lung-cancer mortality with low-dose computed tomographic screening. *N Engl J Med*. 2011;365:395-409.

<http://dx.doi.org/10.1016/j.jtcvs.2012.09.029>

CORRECTION OF FAULTY ASSUMPTIONS IN COST-EFFECTIVENESS ANALYSIS To the Editor:

We read with interest the Canadian cost-effectiveness analysis by Doble and colleagues¹ comparing transcatheter aortic valve replacement (TAVR) with standard management (SM) for inoperable patients and with surgical aortic valve replacement (SAVR) for high-risk patients with severe,

operable aortic stenosis. Although we found many aspects of this work to be well done, we believe that the published analysis contains a few important factual errors and a few assumptions that have been contradicted by recently published follow-up data from the PARTNER trial.

First, the Sapien valve (Edwards Lifesciences LLC, Irvine, Calif) price in Doble and colleagues' analysis¹ (\$37,606) is \$13,606 greater than the current Canadian price of \$24,000 (all figures are in Canadian dollars). With no other changes to the model of Doble and colleagues,¹ correction of the Sapien valve price would make TAVR slightly cost saving (by \$2453) relative to SAVR in high-risk surgical patients and would reduce the incremental cost-effectiveness ratio for TAVR relative to SM from \$36,458 to \$20,497 per life year gained (or from \$51,324 to \$29,037 per quality-adjusted life year gained). Second, Doble and colleagues¹ estimated the costs of SAVR in Ontario from provincial data for patients aged 70 years and older in Case Mix Group (CMG) 165, cardiac valve repair. SAVR procedures are not coded under this CMG in Ontario, however, but rather under CMG 162 (cardiac valve replacement), which has slightly higher reimbursement. More importantly, we believe that "average" reimbursement values for SAVR in patients aged 70 years and older are likely to underestimate the true costs of SAVR among patients like those in the PARTNER trial, whose baseline characteristics in the clinical trial placed them in the highest 5% to 10% of predicted operative risk.²

In projecting survival for inoperable patients, Doble and colleagues¹ used Canadian life table data for years 2 through 20 of their model for both TAVR and SM patient cohorts. This approach assumes that survivals beyond 1 year would be similar for the two groups, in essence ignoring the

well-documented excess long-term mortality associated with uncorrected severe aortic stenosis. In contrast, recently published follow-up data from the PARTNER trial³ showed that among 1-year survivors, mortality during year 2 of follow-up was significantly lower among TAVR patients than among SM patients (18% vs 35%; hazard ratio, 0.58; $P = .02$). Doble and colleagues' approach¹ is therefore very likely to have underestimated the survival benefit that TAVR provides for inoperable patients.

Finally, in their model for high-risk surgical candidates, Doble and colleagues¹ assumed that the increased risk of stroke with TAVR relative to SAVR observed in the first year of follow-up in the PARTNER trial would continue at a constant rate through the 20-year time horizon of the model. Detailed analyses of neurologic events in the PARTNER trial,⁴ as well as recently published longer-term follow-up data,⁵ suggest that after the first 30 days of follow-up, stroke rates for TAVR and SAVR were similar.

We believe that Doble and colleagues' cost-effectiveness model¹ should be updated in light of these important new data. If this were to be done, the articles conclusions could be significantly altered.

Matthew Reynolds, MD, MSc^a

Ann Thompson, BSN, MBA^b

Francis Duhay, MD, MBA^b

^aHarvard Clinical Research Institute
Boston VA Healthcare System
Boston, Mass

^bEdwards Lifesciences LLC
Irvine, Calif

References

1. Doble B, Blackhouse G, Goeree R, Xie F. Cost-effectiveness of the Edwards SAPIEN transcatheter heart valve compared with standard management and surgical aortic valve replacement in patients with severe symptomatic aortic stenosis: a Canadian perspective. *J Thorac Cardiovasc Surg*. Epub 2012 Jul 11.
2. Smith CR, Leon MB, Mack MJ, Miller DC, Moses JW, Svensson LG, et al. Transcatheter versus surgical aortic-valve replacement in high-risk patients. *N Engl J Med*. 2011;364:2187-98.
3. Makkar RR, Fontana GP, Jilaihawi H, Kapadia S, Pichard AD, Douglas PS, et al. Transcatheter aortic-valve replacement for inoperable severe aortic stenosis. *N Engl J Med*. 2012;366:1696-704.
4. Miller DC, Blackstone EH, Mack MJ, Svensson LG, Kodali SK, Kapadia S, et al. Transcatheter (TAVR) versus surgical (AVR) aortic valve replacement: occurrence, hazard, risk factors, and consequences of neurologic events in the PARTNER trial. *J Thorac Cardiovasc Surg*. 2012;143:832-43.e13.
5. Kodali SK, Williams MR, Smith CR, Svensson LG, Webb JG, Makkar RR, et al. Two-year outcomes after transcatheter or surgical aortic-valve replacement. *N Engl J Med*. 2012;366:1686-95.

<http://dx.doi.org/10.1016/j.jtcvs.2012.09.038>

DOES ALBUMIN INTERFERE WITH COAGULATION-RELATED OUTCOMES WHEN COMPARING COLLOIDS IN CARDIAC SURGERY?

To the Editor:

The publication by Navickis and colleagues¹ highlights a subject with conflicting results in the literature, the effect of hydroxyethyl starch (HES) on bleeding after cardiac surgery. Their study included 18 trials with 970 patients reported between 1982 and 2008. The primary end point was postoperative blood loss during the first 24 postoperative hours, and secondary end points were reoperation for bleeding and blood product transfusion within the same period. Navickis and colleagues¹ concluded that HES 450/0.7 and 200/0.5 increased postoperative blood loss, reoperation rates and blood product transfusion relative to albumin, with the statement that despite the lack of sufficient direct data for HES 130/0.4, its effects could be assumed equivalent to those of HES 200/0.5 on the basis of head-to-head comparisons.

In this valuable meta-analysis, we think that there is yet another topic to be discussed. The deleterious effects of HES solutions on bleeding have been emphasized, but the possible positive effects of albumin on

coagulation were not discussed. The older meta-analysis by the same group published in 2001 discussed this topic in detail.² In that article, they stated that albumin, with its antioxidant and free-radical scavenging activity, may serve to avert bleeding after cardiopulmonary bypass (CPB).² They also declared that albumin was shown to block erythrocyte crenation caused by CPB and to preserve functional and morphologic integrity of platelets, which are heavily affected during CPB. They speculated that the difference in bleeding tendency could be attributable to the deleterious effect of HES solutions alone or in combination with protective effects of albumin. Lange and associates³ in their recent and very detailed review on use of colloids in cardiac surgery, defined the anticoagulant action of albumin while summarizing the characteristics of colloids.³ Many controversial results are being published regarding effects of colloids on coagulation, so the effects of albumin on coagulation could be detailed in this valuable meta-analysis.

We believe that the discussion on HES products and their effects on coagulation will continue. In particular, since the retraction of the publications of Boldt, who made substantial contributions to the colloid-colloid and colloid-crystalloid debates in favor of HES, the subject has shifted to a more complicated pattern, leading readers to a more skeptical approach. Physicians can benefit greatly from meta-analyses such as that of Navickis and colleagues.¹ We as readers thank the authors for sharing their knowledge and the experience of their detailed research.

Ahmet Baris Durukan, MD^a

Murat Tavlasoglu, MD^b

Hasan Alper Gurbuz, MD^a

^aDepartment of Cardiovascular
Surgery
Medicana International Ankara
Hospital