Letters to the Editor

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- Include no more than 500 words of text, three authors, and five references
- Type with double-spacing
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Perspectives on patients with diabetes mellitus and triple-vessel disease undergoing coronary artery bypass grafting after previous percutaneous coronary intervention

To the Editor:

In their recent article, Thielmann and colleagues¹ concluded that previous percutaneous coronary intervention (PCI) in patients with diabetes and triple-vessel disease is an independent predictor of death and major adverse cardiac events for patients undergoing coronary artery bypass grafting (CABG). We wish to point out a few confounders and issues that we believe readers should take into consideration when interpreting this study. First, as acknowledged by the authors, the statistical analysis was based on pooled registry data, obtained in a nonrandomized manner and analyzed retrospectively. In addition, the preoperative characteristics differed significantly between groups in important prognostic markers of adverse outcome after CABG, including renal dysfunction and previous myocardial infarction.² Interestingly, the same univariate analysis that identified previous PCI as a predictor of adverse outcome after CABG did not do the same for renal impairment. If the authors conclude the former, are they then implying that previous PCI is a more potent predictor of adverse outcome than the well-recognized problem of renal impairment?

In addition, patients who underwent CABG after PCI constituted a cohort who had already been failed by PCI in combination with optimal medical therapy as a result of either restenosis or progression of de novo disease. This suggests that these patients were in effect a cohort with aggressive coronary disease, and that they might have had poorer outcomes no matter what strategy was offered first. It is therefore difficult to compare this cohort with a group of patients with coronary artery disease without previous treatment failure who were offered CABG as a first revascularization strategy.

Although it may be true that patients with a history of previous PCI may have a higher risk of perioperative complications, it is important to keep in perspective the limitations of this study. We do, however, agree with the authors that current evidence supports revascularization with CABG rather than PCI in the diabetic population. Despite this advice, many patients still opt for less-invasive strategies such as PCI in the first instance. Certainly, the widespread availability of drug-eluting stents has led to interventional cardiologists tackling more difficult, previously unfavorable lesions, not amenable to surgery. Therefore in an evolving area, it is not unexpected that surgeons will be confronted with more challenging patient cohorts.

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Meta-analysis of randomized controlled trials of cognitive decline after on-pump versus off-pump coronary artery bypass graft surgery

To the Editor:

We read with interest the meta-analysis of randomized studies of neurocognitive decline after on-pump versus off-pump

coronary artery bypass graft surgery (CABG) by Takagi and colleagues.¹ The inclusion criteria comprised prospective randomized studies with the incidence of postoperative neurocognitive decline as the primary end point. However, there was significant clinical heterogeneity in the studies that were included in the meta-analysis. In particular, the definitions used to define neurocognitive decline between the trials varied markedly.^{2,3} As a result, we question whether the incidences of neurocognitive decline can be reliably compared. Furthermore, studies that derive an incidence value for neurocognitive impairment after CABG are prone to the statistical phenomenon of regression to the mean (RTM).⁴ RTM explains much of the neurocognitive deficits that have been reported with the use of incidence analysis.

At present, there is no agreement as to what degree of change constitutes neuropsychological dysfunction. In one study of patients undergoing CABG, the incidence of postoperative neurocognitive dysfunction varied considerably according to the definition of neurocognitive decline.⁵ Incidence figures superficially provide a convenient summation of the extent of impairment, but they are calculated by imposing an arbitrary statistical constraint on individual test measures. As such, overall incidence data will vary according to which statistical criteria are used, the sensitivity and number of tests used, and the type of cognitive domains they assess.⁵ Any approach that essentially dichotomizes patients as “impaired” or “unimpaired” is a costly way of data handling that reduces statistical power.

Whereas a specific definition of neurocognitive impairment is required when the study objective is to determine the incidence of postoperative neurocognitive dysfunction, it is not necessary when one wants to test a specific hypothesis such as the effects of an intervention, for example, off-pump surgery. It is far better to use continuous measures inasmuch as they provide greater statistical power and allow more sophisticated analyses. Recently, we⁶ reported neurocognitive outcomes in 212 patients prospectively randomized to on-pump or off-pump CABG. We derived composite scores from all the individual test scores and avoided categorizing patients as “cognitively impaired or not,” thus treating the test scores as a continuous variable. Therefore, any subtle changes in test scores will

have contributed to the overall effect size. In addition, we compared the postoperative scores having adjusted for preoperative scores by using analysis of covariance. This takes RTM into account and is a powerful method of analysing test–retest data.

In summary, asking how often neurocognitive dysfunction occurs after CABG, and expecting a simple one-sided answer to suffice, is simplistic, however desirable. When neuropsychologic tests are used to test a certain hypothesis, as in this investigation, one does not need to set an arbitrary definition of neurocognitive decline to categorize individuals into those with or without a neurocognitive deficit. Instead, one can look at change and its relative difference between groups, which greatly enhances the power of the analysis.

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Reply to the Editor:

We appreciate Drs Motallebzadeh and Jahangiri's letter to the editor regarding our meta-analysis¹ of randomized controlled trials of neurocognitive decline after off-pump versus on-pump coronary artery bypass graft surgery. Although the definitions of neurocognitive decline varied among the trials, as mentioned in their letter, we could not but include only studies reporting neurocognitive dysfunction dichotomously because there is no standard definition for neurocognitive dysfunction. We excluded a randomized controlled trial by Dr Motallebzadeh and associates² in our meta-analysis¹ because it did not report neurocognitive dysfunction dichotomously, and all except for the trial did not report composite neurocognitive scores. Dr Motallebzadeh and colleagues derived composite neurocognitive scores from all of the individual test scores and avoided categorizing patients dichotomously, thus treating the test scores as a continuous variable. If a number of randomized controlled trials reporting composite neurocognitive scores are published, we would like to perform another meta-analysis.

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Surgical techniques for posterior aortic root enlargement

To the Editor:

We have read with extreme interest the paper by Dhareshwar and colleagues,¹ published in a recent issue, describing their results with patients undergoing posterior aortic root enlargement.