Coronary Artery Bypass Surgery, Hospital Volume, and Risk

To the Editor:

The recent article by Goodney and colleagues in Circulation found that differences in 30-day mortality rates between low- and high-volume hospitals were consistent across both low- and high-risk surgical groups, leading the authors to conclude that a policy of targeted regionalization is unlikely to be of value in coronary artery bypass surgery (CABG). On the surface, this finding appears to directly contradict results of an earlier analysis performed by our group, in which we observed that differences in in-hospital mortality rates after CABG may be modified by a patient’s predicted operative risk. So how can discrepancies between these 2 studies be resolved?

Although there are indeed several methodological differences between the studies (which Goodney et al carefully point out), we believe the authors minimized the most likely explanation for dissimilar results: Dramatically different definitions of low-risk. Specifically, the lowest-risk group in our study—i.e., the group in which hospital volume appeared to have no substantial effect—had a predicted operative mortality rate of <0.5%. This is in comparison to a predicted operative mortality rate of 4.3% for the corresponding lowest-risk group in the study by Goodney et al (broadly defined as the lower 75th risk percentile). In fact, if one scrutinizes their results, the study by Goodney and colleagues re-confirms one of our key findings: we also noted that hospital volume has an important and consistent effect on patients with predicted operative mortality rates between 2% to 20% (i.e., moderate-to-high-risk categories in our study).

We therefore are curious as to the results of the additional (but unreported) risk group analyses referred to in the article’s Methods section. In particular, did the authors find important differences across volume categories in patients with a predicted operative mortality rate of <0.5%? To us, the results of such an analysis would be much more relevant to the current debate regarding targeted regionalization, where the fundamental question is not whether volume standards for CABG should focus only on high-risk patients, but whether the benefits of regionalization can be extended to those at very low-risk. Our suspicion is that absolute-risk differences between volume categories in patients at very-low-risk will be minor in comparison to those at high-risk (even if relative-risk differences are significant). Indeed, such a pattern is already apparent in their results; the study by Goodney and colleagues re-confirms one of our key findings: we also noted that hospital volume has an important and consistent effect on patients with predicted operative mortality rates between 2% to 20% (i.e., moderate-to-high-risk categories in our study).

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We are skeptical of the report by Goodney and colleagues that hospital volume-based referral may be appropriate for low-risk cardiac surgery patients. The relevant question for policy-makers is not whether low risk patients treated at very–low-volume hospitals would experience a survival benefit by being treated at very–high-volume hospitals, but whether there is a cohort of patients whose baseline mortality risk is low enough that any regionalization-associated relative risk reduction would translate into a meaningless reduction in absolute mortality.

The analysis by Goodney et al does not address this question. Although the authors stated they were interested in identifying a low-risk cohort, their analysis was limited to a cohort of patients ≥65 years of age. Not only are Medicare patients at higher risk than patients <65 years of age, they represent only half of patients undergoing cardiac surgery. Thus, Goodney et al cannot be reasonably certain that their findings are generalizable to younger patient populations. Moreover, Goodney et al have skewed their comparison of volume effects by restricting their analysis to patients in very–low-volume (<230 coronary artery bypass grafts [CABGs]) and very–high-volume hospitals (>849 CABGs). By comparing only hospitals at the extremes, their analysis does not reflect the reality of a regionalization policy that would affect patients at hospitals with CABG volumes between 230 and 849. The average volume-associated benefit from such a policy likely would be smaller than reported by Goodney et al.

Absent from this discussion is an appreciation of the uncertainty accompanying regionalization. Regionalization would require a redistribution of surgical resources and patients on an unprecedented scale. Furthermore, expectations of a mortality benefit are based on statistical comparisons and not on any real-world regionalization experience. Rather than relying on the crude proxy of hospital volume, purchasers and patients should evaluate cardiac surgery programs on their own merits. Superior quality care—not larger volumes—will ensure patients achieve optimal outcomes.

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Response

Nallamothu and colleagues wonder about the effect of hospital volume in the lowest risk patients in our database. In our study population, approximately 10% of patients had a predicted mortality risk of less than 3%. Within this group, low-volume and high-volume hospitals had mortality rates of 2.5% and 1.9%, respectively. In terms of relative risk (RR), hospital volume had a similar effect in these low-risk patients (RR 0.75, low- versus high-volume) as in higher-risk subgroups previously reported. Of course, absolute differences in mortality were smaller in low-risk patients (0.6%, low- versus high-volume) than in higher-risk patients.

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Response

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Unfortunately, we were unable to replicate the analysis in the original work of Nallamothu et al\(^2\) on the effect of volume in ultra-low-risk patients. In their study, almost half of the study population had predicted risks of 0.5% or lower. In contrast, none of the 800,000 coronary artery bypass grafting patients in our series had a predicted risk that low. Among many reasons for the discrepancy was the fact that we used a broader definition of operative mortality (all in-hospital deaths or within 30 days), whereas Nallamothu et al\(^2\) relied on in-hospital mortality alone.

Second, we studied only Medicare patients over age 65 years, whereas Nallamothu et al included patients from all age strata. Finally, differences in the two studies may be related to different methods used to predict patient risk. Specifically, Nallamothu et al\(^2\) assessed severity of illness using the All-Patient Refined-Diagnosis Related Groups (APR-DRG) index, which often fails to distinguish between pre-existing illnesses and postoperative complications. Such models may exaggerate true differences in baseline risk and predict implausibly low mortality risks for some patient groups.

Methodological issues aside, results from our study\(^1\) and those from the study by Nallamothu et al\(^2\) suggest that hospital volume has a greater effect on absolute mortality rates with coronary artery bypass for high-risk patients than for lower-risk patients. Unfortunately, neither study points to where to set the bar. Is a 2% absolute reduction in operative mortality a large enough difference to warrant regionalization? A 1% difference? A 0.5% percent difference? Although Epstein et al suggest differences between some groups may be so low as to be clinically meaningless, it is difficult to establish this threshold empirically. Thus, maybe we should just let patients decide for themselves.

Our study was not intended to lobby for or against regionalization policies in cardiovascular surgery. We agree with Epstein et al that such debates must consider the potential harms as well as benefits of specific policies. We also agree that the current enthusiasm for using volume as a proxy for quality would quickly fade if hospitals began making direct information about their performance available to patients and providers.

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