Optimal target vessel stenosis for radial artery grafting

To the Editor:

The article by Bleiziffer and colleagues¹ is the latest in a growing body of literature²⁻⁶ reporting worse radial artery graft patency when anastomosed to coronary targets with moderate disease (eg, 70%–90% stenosis) compared with targets with severe stenosis (>90% stenosis). This finding is highly reproducible, as evidenced by analyses of both retrospectively²⁻⁵ and prospectively^{1,6} collected angiographic data reported by multiple authors at multiple institutions spanning multiple continents. Arguably, the convergence of the published literature to the same target stenosis–radial patency relationship coupled with the available objective mechanistic explanation (competitive flow) for this finding should put this question to rest. More specifically, when one is faced with the choice, radial artery grafts should be preferentially anastomosed to the most severely diseased coronary targets (>90% stenosis). These studies, however, have not objectively addressed the question of which conduit type (vein vs radial) should be used for coronary targets with less severe disease (eg, <90% stenosis). Yet, in their article, Bleiziffer and coworkers concluded the following: “On the basis of our results, we attempt to use the radial artery only for target coronary arteries with 90% or greater stenosis.” I contend that the graft patency data presented to date by these authors and others²⁻⁶ do not justify such a limitation on radial artery grafting, and that such a position represents a case of over-concluding. More specifically, some (perhaps including the authors¹) may extrapolate this conclusion, which is based only on data for radial artery patency, to suggest that saphenous vein grafts would then be superior to radial artery grafts when placed in coronary targets with less than 90% disease. I am unaware of any such data. To the contrary, in their most recent report, Desai and colleagues⁷ analyzed prospectively collected radial and vein graft patency data and reported the following: 1) Radial patency increases with increased target stenosis; 2) vein patency does not vary with target stenosis; and, most important, 3) radial patency is superior to that of vein patency when placed in both moderately and severely diseased coronary targets. These data indicate that the radial artery should be preferred to the vein even for

moderately diseased coronary targets, a conclusion that contradicts the authors’ recommendation in this article.¹

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Reply to the Editor:

We fully agree with Habib that, on the basis of the current literature, radial artery patency is supposed to be superior to saphenous vein patency even when the grafts are anastomosed to moderately stenosed coronary targets. Nevertheless, we believe that the radial artery as a graft for coronary artery bypass grafting should be used under the most promising conditions, that is, for highly

stenosed coronary vessels. Radial artery harvesting is more complex than vein harvesting and may lead to more serious complications, such as circulatory or sensory disturbances at the donor arm. Considering this in a risk–benefit analysis, we argue that the radial artery should be harvested only when optimum patency rates are expected for the individual patient. Therefore, we adhere to our recommendation to place the radial artery only to highly stenosed target vessels.

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Prolonged mechanical ventilation after cardiovascular surgery

To the Editor:

I read with great interest the excellent article by Dr Murthy and colleagues¹ describing prolonged ventilatory dependence after cardiovascular surgery. The following clinical questions have arisen since my review of this study:

1. What are the possible explanations for the significant decrease in ventilatory dependence over time? What major changes in delivery of care occurred during the study period (1998–2001)?
2. What was aprotinin exposure in the study cohort? What are the possible ways aprotinin exposure affected the study results? Aprotinin has been associated with a tendency to reduced ventilation time and reduced pulmonary complications after thoracic aortic surgery.² Furthermore, in meta-analysis, aprotinin was the only antifibrinolytic associated with a significantly decreased rate of reoperation for bleeding.³ Both aortic surgery and reoperation for bleeding were significant predictors for ventilatory dependence in your published study.
3. Were there any major changes in anesthetic techniques during the study period that may have affected the incidence of ventilatory dependence?