As we stated in our discussion, the 0.4% absolute difference in mortality between patients with and without myocardial infarction (MI) after successful intervention could still be clinically meaningful. But we must reject the other criticisms of Dr. Ioannidis and colleagues regarding the limitations of our study. The inferences are not based on a small number of unsuccessful procedures, but on an analysis of 5,850 patients with over 100 deaths, for which an unsuccessful procedure was one of the most significant independent predictors of one-year mortality. Most importantly, we are concerned with the misinterpretation of our identification of successful and unsuccessful procedures. We were careful to select unsuccessful procedures using criteria on which most operators would concur in the context of current stenting techniques. We agree many would choose to broaden these criteria and thus further purify the successful group. Regardless of where this line of success is drawn, however, it is clear that the effect of CK-MB elevation among truly successful procedures in this patient cohort would be small to nonexistent.

We also agree that this finding is worth validating in larger numbers of patients. Doing so will require access to databases where the pre-procedure risk and results of successful and unsuccessful procedures are clearly identified, thereby avoiding unnecessary panic among patients and their physicians when small elevations in CK-MB are detected following an otherwise successful procedure.

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Cardiac Rehabilitation Following Myocardial Infarction

In an observational study Witt et al. (1) report a striking survival advantage among patients attending cardiac rehabilitation. They employ a rather unusual adjustment to compare patients of very different ages, a “propensity to attend cardiac rehabilitation” rather than the more usual inclusion of prognostic risk factors in multivariate analyses.

Their findings are not borne out by randomized trials. In discussion, they comment that early (small) trials may not be generalized to contemporary practice. Too true. Pooling of all trials undertaken since the World Health Organization European collaborative (but excluding ours, see the next sentence) show collectively no significant effect on mortality (2). The only multicenter trial undertaken since widespread use of thrombolysis, aspirin, beta-blocker, angiotensin-converting enzyme inhibitor, and statin shows no effect on mortality (3).

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REPLY

We appreciate the interest of Dr. West in our work (1). We respectfully take issue with the statement that the use of propensity-score methodology is unusual. Indeed, the use of propensity score is a commonly used, well-accepted method of statistical adjustment (2,3). It is considered by many to be preferable to conventional regression analysis to adjust for differences in baseline characteristics and control for confounding by indication. As in any observational study, however, we cannot rule out residual confounding related to unmeasured characteristics. This point, which was emphasized in our report, is important to underscore in the interpretation of our data.

As underscored by Dr. West, and as stated in our study, randomized controlled trials constitute the methodological gold standard to test the effect of an intervention. Dr. West quotes one meta-analysis of four trials (4) and one multicenter randomized trial (4). Both of these are published only in abstract format, and neither one provides sufficient information to interpret the findings. For example, the trial inclusion criteria or components of the rehabilitation programs may be substantially different from what is reported in our community-based myocardial incidence cohort (1). These differences could, in turn, explain the observed differences in survival. More importantly, the duration of follow-up in the randomized trial is only 12 months (5), shorter than in our published follow-up of 6.6 years (1). Finally, the apparent age and gender disparities in the delivery of care noted in our analysis could...