treat patients—that do not fit in the current recommendations—according our current clinical experience and judgment.

> "*On the mountains of truth you can never climb in vain: either you will reach a point higher up today, or you will be training your powers so that you will be able to climb higher tomorrow.*"
> Friedrich Nietzsche (20)

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Reply

We are grateful to Dr. Meliga and colleagues for their interest in our article (1). They raise several important issues that we will address in a similar order.

The first issue deals with mortality and morbidity after coronary artery bypass grafting (CABG). As referenced in our article (1), the mortality for all 5,003 patients with left main stem stenosis undergoing CABG in the United Kingdom in 2003 was 3% (and <2% in 17,000 without left main stem [LMS] stenosis and 1% in 3,102 patients in the ART [Arterial Revascularisation] trial). Because enough is known about post-CABG complications, risk models have been developed to reliably predict their occurrence, whereas similar data are quite lacking in the percutaneous coronary intervention (PCI) domain.

In addition, although all postoperative morbidity is unsatisfactory, the reality is that, with the exception of stroke (1% to 2%), most of it is self-limiting and of little consequence to the patient over the long term. To equate early postoperative morbidity to the reduced survival and marked increase in the need for reintervention with PCI over the long term is arguably a false economy. Furthermore, long-term mortality from CABG (as well as PCI) may also reflect other co-existing morbidities, rather than being attributable to ischemic heart disease.

With regard to bare-metal stents, we stated explicitly that superior results were obtained in lower-risk patients and that, as for CABG, the results of PCI would also be disadvantaged by greater-risk patients. Although Dr. Meliga and colleagues state that there was no significant difference in mortality between CABG and PCI in the SoS (Stent or Surgery) trial at 1 year, it should be noted that, at 5-year follow-up in this study (2), there was a significant reduction in the risk of mortality with CABG (6.6%) versus PCI (10.9%), reinforcing the well-known observation that the benefit of CABG often accrues with time. We agree with Dr. Meliga and colleagues that substantial heterogeneity among drug-eluting stent trials precludes pooling them together. Accordingly, we did not perform a meta-analysis. Our aim was simply to present all the published studies in the literature.

The complexity and precise anatomical location of distal left main stem disease, along with its frequently associated multivessel coronary disease, is not relevant during CABG because bypass

grafts are placed to the midcoronary vessels, well beyond the left main and proximal coronary disease (in contradistinction to their importance for diminishing the likely success of PCI). Furthermore, and again in contrast to PCI, this location of bypass grafts offers prophylaxis against the development of new disease and likely contributes to the survival benefit of CABG. The authors would surely acknowledge that, without repeat intervention for restenosis in distal left main stem disease, mortality would almost certainly be increased. In addition, Dr. Meliga and colleagues themselves have emphasized the crucial impact of distal LMS stenosis in predicting adverse outcomes with an almost 3-fold increase in risk in patients with distal LMS stenosis (30%) compared with those without (11%) at a median of 18 months (3). Finally, as stated in our article (1), 2 independent groups have stressed that because restenosis in this critical location is frequently asymptomatic (4,5), a serial angiographic follow-up is mandated.

The assertion of Dr. Meliga and colleagues that in all major institutions all interventional decisions are evaluated by both interventional cardiologists and cardiac surgeons is not borne out by the facts. Indeed, many studies explicitly state that the interventional strategy was decided by the interventional physician or "according to patient preference." However, we strongly agree, and have repeatedly advocated (6,7), that a multidisciplinary team involving a surgeon should be the standard of care to ensure real patient choice and genuine informed consent not only for LMS stenosis but for all multivessel coronary artery disease.

The authors refer to the study by Bravata et al. (8) of 23 randomized trials of PCI and CABG reporting no difference in mortality at 10 years. These findings contrast with the findings of Hoffman et al. (9) who, in a meta-analyses of randomized trials of PCI and CABG, found a statistically significant survival advantage for CABG as well as a 4-fold reduction in the need for reintervention. In any event, the vast majority of patients in these trials had single- or double-vessel coronary artery disease and normal ventricular function (10), a population in whom it had been well established more than a decade earlier that there was no prognostic benefit from surgery (11).

This population is also very different from those who undergo CABG in clinical practice, and to imply that these trials are representative of most patients undergoing coronary revascularization in clinical practice is misleading (6). Indeed, several large contemporary registries have consistently shown a marked survival benefit and freedom from reintervention with CABG in comparison with PCI (12). The most recent registry (MAIN-COMPARE [Unprotected Left MAIN Coronary Artery Stenosis: COMparison of Percutaneous Coronary Angioplasty Versus Surgical Revascularization]) showed a mortality hazard ratio of 1.18 (95% confidence interval: 0.77 to 1.80) for PCI compared with CABG. Although this difference was not statistically significant (because of inadequate power), the data are compatible with as much as a 23% improvement or as much as a 80% worsening in mortality with PCI compared with CABG (13).

As stated in our article (1), although the LEMANS trial is the only randomized trial comparing PCI versus CABG in 2 groups of approximately 50 patients, the relatively high operative mortality in these low-risk patients, allied with the fact that only 72% received an internal mammary artery graft, suggests that surgery was less than optimal.

With regards to the justification for randomized trials in LMS disease, we believe we presented both sides of the arguments in a fair and balanced manner. We argued that although CABG has been shown to be superior to medical therapy for left main and 3-vessel disease in randomized controlled trials, this is not the case for PCI and, because CABG has been consistently demonstrated to be superior to PCI in several large registries, it is not justifiable to present the information to a patient as there being equipoise between PCI and CABG. This is particularly true when viewed from a comparative effectiveness perspective that is patient centric in focus. Finally, the cogent comments of Dr. Meliga and colleagues faithfully reflect the current evidence base: "CABG should remain the preferred revascularization strategy in good surgical candidates with left main coronary artery disease" (14).

> "*The truth is incontrovertible, malice may attack it, ignorance may deride it, but in the end there it is.*"
>
> Winston Churchill (15)

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