Letter to the Editor Regarding "Efficacy of Radial Versus Femoral Access in the Acute Coronary Syndrome: Is it the operator or the operation that matters?" by Michel LeMay MD and colleagues

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This manuscript was prepared without external funding; Dr. Rao reports research funding from Bellerophon; consulting honoraria from Astra Zeneca, Medtronic, Terumo Interventional Systems, Merck, and CSI; Dr. Valgimigli reports research funding from The Medicines Company and Terumo

To the Editor:

We read with great interest and concern the paper by LeMay and colleagues regarding the role of radial approach in patients with acute coronary syndrome (ACS). Our concerns are due to LeMay's inappropriate interpretation of clinical trial data and spurious arguments against radial access that runs of the risk of slowing the adoption of transradial procedures and thus potentially lead to increased adverse outcomes among high-risk patients undergoing PCI.

LeMay's concerns over multiple comparisons in the MATRIX trial are unfounded. While there are several techniques to account for multiple comparisons, the Bonferroni-Holm correction, which was pre-specified in the MATRIX trial (1), is by far the most conservative one. In the MATRIX trial, radial approach significantly reduced the incidence of 30-day NACE (p=0.009). These results are robust even when accounting for 4 comparisons. In addition, many of the large seminal trials of acute coronary syndromes have included both NSTEMI and STEMI patients (2),(3). The correct interpretation of subgroups is to use interaction p-value to determine consistency of benefit in subgroups and the benefit was consistent in both NSTEMI and STEMI in MATRIX. Taken together, the available trials strongly demonstrate that TRI is superior to femoral approach in ACS patients regardless of presentation. Regarding the effect of center volume, the beneficial effect of radial approach was more pronounced at centers with high radial volume (i.e. a greater proportion of procedures performed via radial access) in the RIVAL and MATRIX trials. The positive interaction term by center volume demonstrates that the primary effect may be affected to some degree by the subgroup – in other words, the results of the MATRIX trial stand

alone and the finding in the subgroups of center radial volume demonstrate that centers that perform a high proportion of TRI realize its greater benefits – a finding that is completely consistent with the volume-outcome relationship for cardiovascular procedures. There was no significant difference in the use of glycoprotein IIb/IIIa inhibitors (GPI) between the radial and femoral groups in the MATRIX trial, and the benefit of radial access was irrespective of GPI use. Finally, a commonly held belief among interventional cardiologists is that VCDs reduce bleeding, and LeMay and colleagues repeat this misconception in their review. A meta-analysis of randomized trials showed that VCDs were no better than manual compression at preventing vascular complications and may increase the risk of groin hematoma and pseudoaneurysm (4). The ISAR-CLOSURE trial showed that VCDs were noninferior, not superior, to manual compression with respect to 30-day vascular complications (5). The added cost of VCDs are not justified when their outcomes are either no better or worse than manual compression.

The randomized trial data clearly support the use of radial access in patients with acute coronary syndromes undergoing angiography or intervention. Given the robustness of the data, performing more randomized trials, like the SAFARI STEMI trial, which is one-third the size of the MATRIX trial and is most likely underpowered for the primary outcome, is unlikely to be informative. Instead, effort should be directed at increasing the adoption of radial approach and ensuring proficiency with the procedure using “best practices” as the foundation (6).

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