

Letter: Refining the prediction of side branch occlusion following percutaneous coronary intervention in bifurcation lesions



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We read with great interest the paper by Lee et al regarding prediction of side branch (SB) occlusion following percutaneous coronary intervention (PCI) for bifurcation lesions using a risk score derived from coronary computed tomography angiography (coronary CTA)¹. While we recognise the unmet need for optimised tools to facilitate the individualised guidance of PCI in bifurcation lesions, we have some concerns regarding the methodological approach used in this study.

First, the prevalence of SB compromise in the paper by Lee et al is higher than hitherto reported. While the authors report an SB occlusion rate of 16% (42/260), the combined occurrence of SB occlusion in the RESOLVE derivation and validation cohorts was 9.9% (340/3,421), and it was 7.0% (28/400) in our recent paper on the CTA-derived RESOLVE score^{2,3}. We believe this discrepancy might be explained by a more liberal definition of SB occlusion in the study by Lee et al (inclusive of the drop in Thrombolysis In Myocardial Infarction flow grade after main vessel stenting as well as ballooning). In addition, the current study did not adopt the definition of SB significance recommended by the European Bifurcation Club but rather included all

SBs with a visually estimated diameter of ≥ 2 mm. Such application of a strictly anatomical criterion may impact on the true rates of the complication in small cohorts and consequently bias the true characteristics of globally significant SBs that were compromised, thereby limiting the clinical utility of the proposed risk score. Providing sensitivity analysis using previously established endpoints and definitions would enhance comparability between the discussed risk scores^{2,3}.

Second, the results of the study by Lee et al should be interpreted with caution regarding the risk of bias related to the analysis of plaque composition on coronary CTA. Although the proposed CT bifurcation score requires dichotomous assessment of the presence of calcified and low-attenuation plaques, neither workflow nor reference for their qualitative detection was proposed. Considering the wide number of definitions developed over a span of many years (particularly for calcified plaque), the question of the reproducibility of the risk score presented by Lee et al is still left open.

Finally, the authors report simultaneous use of coronary segmentation software that could overestimate the detectability of the respective components – especially low-attenuation plaques.

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Despite the promising results produced with this technology for serial measurements, different factors such as contrast type, acquisition protocol or kernel reconstruction might potentially influence analysis in the multicentre setting.

The above-mentioned concerns may be important when interpreting the results of the study by Lee et al.

Conflict of interest statement

The authors have no conflicts of interest to declare.

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