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Letter to the Editor

Letter to the editor regarding "Clinical outcomes of modified partial eversion carotid endarterectomy: 27-month follow-up"

Abdullah Burak Balci

İstanbul Bakırköy Dr. Sadi Konuk Training and Research Hospital, Department of Cardiovascular Surgery, İstanbul, Türkiye

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DEAR EDITOR

We read with interest the article by Pinar et al. reporting the mid-term clinical outcomes of modified partial eversion carotid endarterectomy [1]. The authors present a neat single-center cohort undergoing an interesting technical variation performed by a single surgical team with low perioperative complication rates and an average clamp time of 16 minutes.

While these early results are encouraging, several methodological and analytical points warrant further discussion.

First, the primary endpoint was restricted to overall survival. While mortality is a major benchmark, the surgical durability of carotid endarterectomy is primarily reflected in stroke prevention, freedom from transient ischemic attack, ipsilateral restenosis, and reintervention rates, aligning with major European Society for Vascular Surgery (ESVS) guidelines [2]. Because these stroke- and carotid-specific surveillance endpoints were not systematically tracked, the current data establish short-term procedural safety rather than the long-term anatomical or neurological efficacy of MPE-CEA. Furthermore, given that all arteriotomies were closed primarily without patch angioplasty, systematic postoperative duplex ultrasound data would have been essential to evaluate early and late restenosis risks.

Second, chronic kidney disease (CKD) was identified as an independent predictor of overall mortality. Although this association is clinically plausible and consistent with broader vascular surgical literature [3], only five patients had CKD in this cohort, four of whom died. This extremely small event subset yielded a remarkably wide confidence interval (HR: 10.446; 95% CI: 1.257–86.777), indicating substantial uncertainty around the true effect size. In addition, the manuscript lacks specific baseline definitions for CKD, such as eGFR cutoffs, disease staging, or dialysis status.

Third, the multivariable Cox proportional hazards model included three covariates despite recording only seven death events during follow-up. Incorporating three predictors for seven events raises a clear risk of model overfitting and parameter instability, as statistical guidance generally recommends a minimum of 10 to 15 events per variable [4]. Variable selection

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Corresponding Author: Abdullah Burak Balci, İstanbul Bakırköy Dr. Sadi Konuk Training and Research Hospital, Department of Cardiovascular Surgery, İstanbul, Türkiye
Email: burakmed188bal@gmail.com

based solely on univariate significance further exacerbates this risk. Clarifying whether proportional hazards assumptions were evaluated or if model overfitting was guarded against would assist in interpreting these findings.

Fourth, the reported 5-year Kaplan–Meier survival estimate requires careful reading. With a mean follow-up of 27 months, the number of patients remaining at risk beyond 3 to 4 years was likely minimal. Providing the number of patients at risk beneath the survival curves and reporting median follow-up times with interquartile ranges would give readers a clearer picture of late survival reliability. Lastly, a minor numerical discrepancy appears in Table 1 [1]: the survival group lists 33 symptomatic patients as 48%, which does not mathematically correspond to 33 out of 48 patients (68.8%).

In conclusion, Pinar et al. demonstrate that MPE-CEA can be performed safely with low immediate morbidity. However, the absence of a control group, lack of routine duplex surveillance, and limited neurological follow-up restrict firm conclusions regarding comparative superiority or long-term durability. Future prospective studies with standardized renal staging and objective duplex-based vascular outcomes will better clarify the exact place of this technical modification in carotid surgery.

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REPLY BY PINAR ET AL.

We thank the authors for their careful reading of our article and for their constructive comments.

We agree that long-term evaluation after carotid endarterectomy should ideally include outcomes such as ipsilateral stroke, transient ischemic attack, restenosis, and reintervention. In our study, however, the primary endpoint was all-cause mortality, and systematic long-term data on these outcomes were not available for all patients. Therefore, we did not intend to draw conclusions regarding the long-term anatomical durability of MPE-CEA or its superiority over other carotid endarterectomy techniques. These limitations were already acknowledged in the Discussion and Limitations sections of our article.

We also appreciate the comments regarding chronic kidney disease. CKD was defined according to KDIGO criteria [3], although this was not explicitly stated in the Methods section, and we acknowledge this as a reporting omission. We agree that the small number of patients with CKD and the limited number of mortality events reduce the precision of the estimated effect. A larger cohort with more events would likely provide a more reliable estimate of the association between CKD and long-term mortality. Therefore, this finding should be interpreted with caution.

We also agree with the concern regarding the multivariable Cox regression analysis. Given the limited number of mortality events, the possibility of model overfitting and instability cannot be excluded. The low number of events and the resulting wide confidence intervals were already acknowledged among the limitations of our study. Larger studies with a greater number of outcome events would allow more reliable multivariable analyses.

Regarding the 5-year Kaplan–Meier survival estimate, we agree that the limited number of patients remaining at risk at later time points reduces the reliability of this estimate. This issue was specifically stated in the Results section of our article, where we noted that the 5-year survival estimate should be interpreted cautiously. We also agree that providing the number of patients at risk beneath the survival curve would have improved the presentation of the survival data.

Finally, we thank the authors for identifying the numerical discrepancy in Table 1 [1]. The number of symptomatic patients in the survival group was 33 of 48 patients, corresponding to 68.8%, rather than 48%. This was a typographical error and does not affect the analyses or conclusions of the study.

Overall, we believe that the points raised in the letter are relevant and largely reinforce the limitations already acknowledged in our study, particularly the retrospective single-center design,

small sample size, limited number of mortality events, absence of a comparison group, and lack of systematic long-term duplex and neurological follow-up. Our aim was not to demonstrate the superiority of MPE-CEA over other techniques, but to report the clinical outcomes observed in our cohort and to evaluate factors associated with overall survival. Importantly, published data on modified partial eversion carotid endarterectomy remain limited, particularly regarding longer-term clinical outcomes. Therefore, despite its limitations, we believe that our study adds useful clinical data to the existing literature on this technique. We thank the authors again for their constructive comments and believe that these points may help guide future studies with larger cohorts and more comprehensive follow-up.

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