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Endarterectomy Closure.”<sup>1</sup> We agree with most of their comments. However, we did include a meta-analysis by Texakalidis et al.<sup>2</sup> They had performed a meta-analysis of randomized trials comparing bovine pericardium and other patch materials for carotid endarterectomy (CEA), which showed that the incidence of 30-day stroke, myocardial infarction, wound infection, death, cranial nerve injury, carotid artery thrombosis, and death were comparable. The long-term stroke and restenosis rate were also similar between venous and synthetic patches, with no differences in 30-day stroke, death, transient ischemic attack, carotid artery thrombosis, or long-term restenosis detected between Dacron and synthetic PTFE patches. It is feasible that the differences to which the authors referred can be explained by the different study designs and did not distinguish between various Dacron materials (old design Dacron [eg, Hemashield] vs Ultra Finesse Dacron), which were found in our previous study.<sup>3-5</sup> It is true that most authorities, at least in the United States, have recently tended to use either pericardial patches or eversion CEA, which were believed to have similar early and late outcomes. The authors themselves prefer CEA with patch closure using the Acu-seal PTFE patch and eversion endarterectomy. However, studying the data we analyzed, it was clear that no significant differences were present between the various patches, especially if old Dacron patch (Hemashield patch) were excluded.

Thank you for the opportunity to reply to the letter to the editor.

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## Abdominal aortic aneurysm repair in the context of COVID-19



We would like to share ideas on the publication “Reassessing the operative threshold for abdominal aortic aneurysm repair in the context of COVID-19” by McGuinness et al, who noted that “The decision to delay operative repair of AAA should consider both patient age and local COVID-19 prevalence in addition to aneurysm size.”<sup>1</sup> Aneurysm is an important problem, and it usually requires an emergency management if there is a rupture. A standard management should be provided regardless of COVID-19 outbreak status.

It is an interesting clinical question whether COVID-19 can affect the fate of vascular aneurysm or not. In a recent report from China, the vascular surgery outcomes in patients having ruptured intracranial aneurysm with and without COVID-9 are not different.<sup>2</sup> Regarding management of elective aortic aneurysm repairs during COVID-19, there should also be no change in the indications. Regardless of COVID-19 situation, regular surgical procedure planning is recommended. There should be no postponement due to COVID-19 situation because a delay might result in a deadly rupture.<sup>3</sup> Nevertheless, it is necessary to follow standard universal infection control principles. All cases should be strictly managed regardless of COVID-19 status. For vascular aneurysm management during COVID-19 outbreak, a strict safety protocol, infection control, and shortening of surgical period are recommended.<sup>4</sup>

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### Statistical limitations to knowing the odds of heart failure for patients with traumatic arteriovenous fistulas



I read the article by Wenzl et al<sup>1</sup> "A systematic review and individual patient data meta-analysis of heart failure as a rare complication of traumatic arteriovenous fistulas" with great interest. Understanding the odds of patients developing severe complications is a worthy endeavor.

Several limitations, some discussed to a certain extent in their article, limit the generalizability and reliability of the specific reported odds ratios. As the authors noted, their meta-analysis was performed on 15 case series and 177 case reports. However, the marked differences between the patients chosen for their unusual characteristics to include in a case report and most patients encountered clinically substantially limit the generalizability of the specific statistics. Also, the nonstandard statistical techniques used in their study limit the certainty of the reported results. The authors did not state what specific clinical or demographic characteristics they adjusted for nor how they had statistically adjusted for them.

When reporting an odds ratio of 3.25 ( $P = .015$ ) for the involvement of large feeding arteries being associated with heart failure, the authors arbitrarily designated the four largest arteries as large without specifying why the four were chosen. They classified the external iliac artery as a large artery and the common femoral artery as not a large artery. This decision increased the reported significance of the findings but is not a trustworthy statistical technique. They also wrote that "an increased feeding artery diameter significantly increased the risk of HF [heart failure] in the entire study group (odds ratio [OR], 1.96; 95% CI, 1.37-2.8,  $P < .001$ )" without specifying any length of measurement to associate with that odds ratio.

The authors also reported that 6 years of delay to presentation had an odds ratio of 1.30 for heart failure ( $P = .026$ ) but did not specify why the unusual duration of 6 years was chosen when a typical period such as 5 years might not have resulted in significance.

These nonstandard techniques, combined with the substantial limitations to extrapolating the study's statistical model to patients encountered in clinical practice, given the unique patient population included in the case series and case reports, make these odds ratios clinically unreliable.

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### Single versus multiple vessel revascularization of infrapopliteal arteries



We congratulate Hater et al<sup>1</sup> on their comprehensive article, describing their findings on single-vs multiple-vessel below-the-knee revascularization in patients with chronic limb-threatening ischemia. We were interested to read that in 527 limbs there was no difference found between the two groups for freedom from amputation ( $P = .109$ ), reintervention ( $P = .13$ ), and overall survival (73% in both groups), with a follow-up of  $19 \pm 18$  months. We also noted the high proportion of diabetic patients within the cohort ( $P = .79$ ) and the authors' success at multiple-vessel revascularization.

We were struck by the differences between the authors' findings and those of our own group. Our retrospective review included 250 limbs that underwent infrapopliteal angioplasty over an 8-year period (January 2009 to December 2016). We found that combined multivessel reperfusion resulted in significant improvements over simple indirect reperfusion in amputation-free survival ( $P = .039$ ), reintervention and amputation-free survival ( $P = .005$ ), and wound healing ( $P = .047$ ),<sup>2</sup> even after adjustment for confounders. We found that outcomes were similar between multivessel and simple indirect reperfusion only when examining the diabetic cohort.

We were surprised to find that the angiosome model was not considered for such a large cohort of patients, given the ongoing discussions in the literature surrounding its impact on limb salvage. A systematic review and meta-analysis by Bosanquet et al<sup>3</sup> examined direct vs indirect angiosomal revascularization of infrapopliteal arteries, including 15 cohort studies and 1868 limbs. This meta-analysis found that direct reperfusion resulted in significantly improved wound healing (odds ratio: 0.40, 95% confidence interval: 0.29-0.54,  $P \leq .00001$ ) and limb salvage rates (odds ratio: 0.24, 95% confidence interval: 0.13-0.45,  $P \leq .0001$ ) compared with indirect reperfusion, but had no effect on reintervention rates ( $P = .27$ ) or mortality rates ( $P = .24$ ).<sup>3</sup>

The patient cohort, indications, and end points between our two studies are similar, so it is very interesting that the results are so different. We would be interested to learn