

LETTERS TO THE EDITOR

Regarding “Replanting the inferior mesenteric artery during infrarenal aortic aneurysm repair: influence on postoperative colon ischemia”

I hoped to find good level 1 evidence in the first randomized trial on the influence of replanting the inferior mesenteric artery (IMA) or ligation during open (semi)elective AAA repair on postoperative colon ischemia in the study by Senekowitsch et al (*J Vasc Surg* 2006;43:689-94). However, after having read the article, I still don't have an answer.

The primary end point in this trial was colon ischemia assessed with sigmoidoscopy, and the secondary end point was mortality. Although the authors acknowledge that the study was underpowered to detect a significant difference in ischemic colitis, it might be that the study design and analysis also contributed to this result.

An intention-to-treat analysis shows no differences in ischemic colitis and mortality in both study groups (Table II). Then the authors jump to a subgroup analysis. Of the 71 patients assigned to IMA replanting, only 42 had a patent IMA, and patency in 25 of the remaining 29 patients could be restored by thrombectomy or endarterectomy. In 25 of the 86 patients assigned to ligation, the IMA was occluded. These patients were excluded from a detailed analysis of ischemic colitis. Thus, the authors actually compare an attempt to optimize circulation of the colon in 67 patients vs maximum compromise of this circulation in 61 patients. Yet, no differences could be established in the incidence of ischemic colitis or mortality. In addition, data on the extent of ischemic colitis in five of nine asymptomatic patients are not provided in the results.

Finally, the authors try to identify risk factors for ischemic colitis in the cohort of 128 (and not the original 157) patients by calculating relative risks for different factors. Although the risks are properly calculated, they are not correctly summarized in Table V.

A post hoc multivariate analysis could also have helped identify independent predictors of ischemic colitis or mortality. The authors suggest that older patients with increased operative blood loss might benefit from IMA replantation because this procedure does no harm. This advice is not supported by the data in this interesting study.

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Reply

We thank Dr Koelemay for his interest in our article and also for pointing out some relevant aspects of our study.

Assessing the primary cause of ischemic colitis after aortic reconstructions is, as you also pointed out, not always an easy task, as it is multifactorial in origin. We tried to minimize potential confounders by excluding all patients but those with at least one patent hypogastric artery and without chronic colon disease or previous colon resection. Also, clinically relevant ischemic colitis is relatively rare, and differences between groups were (as demonstrated in the past) not large. Therefore, histology was our only possible end point, arguably not always clinically relevant, yet—as we believe—most sensitive for the purpose of the study.

It is correct that the intention-to-treat analysis as well as the analysis of the subgroup with patent inferior mesenteric artery (IMA) did not yield significant differences between the groups.

The reason for mainly discussing the subgroup with patent IMA is also quite apparent: An already occluded vessel cannot have any influence on outcome if ligated or not. We initially encompassed all patients into the analysis; however, it was an explicit demand by the reviewers and editors of the *Journal of Vascular Surgery* to mainly include patients with a patent IMA, and we do fully agree that the later patients are the relevant ones for final evaluation. A multivariate analysis was also performed but did not yield further insight into causes for postoperative ischemic colitis.

When analyzing all 157 patients regarding risk factors, no different results were seen: age, sex and blood loss were the parameters differing between patients with and without ischemic colitis. We believe that this result is actually of great relevance as it demonstrates that we are mainly talking about a hemodynamic problem in open surgery and not—as frequently believed—an embolic one, also underlined by the occurrence of ischemic colitis in patients with an occluded IMA. Histologic results of all patients are listed in the first paragraph of the “Results” section.

Finally, we do agree that our data do not fully support the suggestion to replant the IMA in older patients and in those with high intraoperative blood loss. Yet, the fact that we did not encounter a single complication, neither intraoperatively nor postoperatively, by replanting the IMA and that revascularization usually means increased perfusion pressure and thus optimizing critical flow justifies the suggestion. Even more so, as we could not produce final evidence against IMA replant in patients with at least one patent hypogastric artery, even though it did not prove to be beneficial in the whole study population or the subgroup of patients with patent IMA.

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Regarding “Diffuse phlegmonous phlebitis after endovenous laser treatment of the great saphenous vein”

We read the case report by Dunst and colleagues (*J Vasc Surg* 2006;43:1056-8) with much interest. Tens of thousand cases of endovenous laser ablation (EVL) have been done with no infection reported.^{1,2} Therefore, one may suspect that appropriate guidelines for performing EVL were not followed.³⁻⁵ These deviations may have contributed to this serious infection.

1. The entry point of the laser (presumably percutaneous) was too close to the ulcer, ie, 10 cm; moreover, it appears that another incision was made between the entry point and ulcer to ligate perforators.
2. No tumescent anesthesia was used.
3. Prophylactic antibiotic was not used, in the presence of an active ulcer, where additional procedures were performed.

We recently published our results of the first 1250 EVLs,² among which were 52 patients with active ulcers. All the cases were