ment. Clearly, aneurysms can occur in any biologic material and in the future surveillance protocols will surely reveal a more accurate picture of its prevalence.

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24/41/97446

Reply

We read with interest the letter from Dr Dardik regarding our article “Infrainguinal aneurysm formation in arterialized autologous saphenous vein grafts.”

We highly appreciated the comments of Dr Dardik, and we agree that an isolated graft aneurysm will likely be the consequence of a local graft defect, although a degenerative or metabolic origin cannot be excluded. However, in diffuse graft dilatation, a local defect seems etiologically highly improbable.

The suggestions of Dr Dardik lead us to review the 19 described cases of aneurysmatic grafts once again, and we observed that five of the 19 grafts had been performed to repair a popliteal artery aneurysm and that in three cases a concomitant abdominal aneurysm was found. All but one of these eight patients later showed a diffuse graft dilatation that necessitated the replacement of the entire graft. In contrast, in all of the patients in whom the vein graft had been performed because of occlusive disease, an isolated graft aneurysm rather than diffuse dilatation was noted and only segmental graft replacement was needed. This would confirm a propensity of aneurysms to develop both in arteries and in arterialized veins in some patients.

If we postulate further that endogenous factors (eg, genetic) rather than exogenous noxious substances (eg, tobacco) lead to aneurysms, I am not surprised that, in the case presented by Dr Dardik, the autologous vein graft was aneurysmatic in contrast to the homologous umbilical vein.

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24/41/97445

Regarding “A comparison of carotid angioplasty with stenting versus endarterectomy with regional anesthesia”

To the Editors:

An interesting paper by Jordan et al (J Vasc Surg 1998;28:397-403) contains a few weaknesses. Some of these weaknesses are clearly identified by the authors, and some are discussed by Dr Clagett in his commentary after the paper. I would like to address some weaknesses that are mainly caused by the absence of any input by anesthesiologists into this study.

First, the methods of anesthesia are described in fewer than 10 words. Which anesthetics were used? Which concentrations? What regional block was performed: deep, superficial, or both cervical blocks?

Second, in the methods section, the authors stated, “Hypotension and bradycardia were counted as complications if the conditions were treated with additional intravenous fluids, inotropic agents, or atropine.” If anesthesiologists had been consulted in the preparation of this manuscript, they probably would have suggested having different, better justified endpoints, namely, a certain degree of arterial hypotension and bradycardia. The reason is that different anesthesiologists and different physicians, nonanesthesiologists, would start treating a condition at different values of blood pressure and heart rate. The use of more objective endpoints would potentially make this part of the results more accurate.

Third, in the following sentence, the authors stated, “Other events, such as neck or groin hematomas…, were classified as requiring additional monitoring.” The connection between hematomas and monitoring does not make much sense. If an anesthesiologist was consulted in the preparation of this manuscript, this point would have been clarified.

Fourth, in the discussion section, the authors stated that “One can argue that [local or regional anesthesia] may reduce additional complications…..” The authors, however, did not address this question. Neither of the surgical procedures that were performed with local or regional anesthesia was compared with similar procedures done with regional anesthesia. If anesthesiologists were consulted, they would have suggested having a matched control of patients for general anesthesia. This is easy to do in a retrospective study.

Fifth, there are a few remarks throughout the manuscript that are not justified. In the middle of the discussion section, for example, the authors mention the patient population that was excluded from the study. They state, “There are also those patients who are to be at extreme high risk for a general anesthetic…..” I have been an anesthesiologist for 40 years and do not remember a single such patient. We see patients who are at very high risk for surgical procedures, and we see patients with contraindications for regional or local anesthesia, but I am not aware of any contraindications to general anesthesia. If an anesthesiologist were consulted, I am sure that this distinguished team of investigators would have been enlightened.

These brief notes have only one goal, which is to draw the attention of your surgical readership to the usefulness of cooperation among colleagues with whom they work closely and who are experts in their fields. Such collaboration would improve the quality of research we are con-