LETTERS TO THE EDITORS

Regarding “A comparative evaluation of polytetrafluoroethylene, umbilical vein, and saphenous vein bypass grafts for femoral-popliteal above-knee revascularization: a prospective randomized Department of Veterans Affairs cooperative study”

To the Editors:

Johnson et al. report the fourth published series of patients requiring lower limb revascularization and undergoing a protocol of prospective randomization to saphenous vein, umbilical vein, or PTFE. Eickoff et al. reported on 104 below-knee popliteal of prospective randomization to saphenous vein, umbilical vein, and PTFE. Alders and von Vroothen reported 96 above-knee popliteal bypasses, and McCollum et al. on 191 patients with an above-knee: below-knee ratio of 2:1. All of these data were accumulated and, with the exception of the delayed report by Johnson et al., reported in the 1980s. Several comments are in order:

1. Though these studies are dated and in many areas, flawed, they are, nonetheless, incredibly, the only ones carried out with more than one alternative to the saphenous vein, randomized, prospective, and performed at multiple surgical sites. This is in contradiction to the manuscript by Burger et al., which typifies the usual reporting of lower limb graft material reviews—a single graft compared with autologous vein, the usual inferior results obtained with a prosthetic and the standard conclusion that, despite differences in patency, use of the prosthetic is “reasonable” and “acceptable.” The Invited Comment by J. Mills, which should be required reading for all vascular surgeons, points to the deficiencies of statistical analysis in Burger’s paper and others based on inadequate power.

2. The Meadox product used in all these studies is no longer manufactured. The currently available UV graft is manufactured by BioVascular Inc, Minn. There are available data showing absence of aneurysmal degradation with the latter product as well as a significant reduction in the early thrombotic events as documented by Johnson et al. with the Meadox product. This is based on improved quality control during manufacture, reduction of residual aldehyde moities in graft and storage media, and appreciation of the surgical skills required for implantation. Our first decade of experience with the UV graft was reported in 1988, not in 1998 (reference 20 in Johnson et al.). Additional experience and even better results with the improved UV graft were reported in 1995 and 1996.

3. Current evaluations of graft materials should include intraoperative completion duplex sonography and be complimented, as required, with completion angiography. This is the only way to establish the different materials and surgeons on an even keel.

4. The ultimate test for comparative function of materials is in the crural position. No such studies have been performed in a multihospital, randomized prospective manner and with more than one alternative material to the saphenous vein. The Advisory Council for Vascular Surgery to the American College of Surgeons has recently authorized the formulation of such a protocol. Its execution will depend on availability of funding from agencies, foundations, and industry. Hopefully, this will occur and provide some needed guidance in a field now clouded by personal bias and the industrial complex.

Herbert Dardik, MD
Englewood Hospital and Medical Center
Englewood, NJ

REFERENCES


24/41/112315

Reply

We welcome comments from the vascular community about our recently published article on the comparative performance of saphenous vein, PTFE, and HUV bypasses that were implanted between 1983 and 1988 and followed until October 1991. Dr Dardik is correct that the HUV bypasses were provided by Meadox and not by BioVascular. We apologize for the incorrect citation of his 1988 article (wrong year but correct volume of Journal of Vascular Surgery).

We concur with Dr Mill’s and Dr Dardik’s comments that the Burger report has inadequate “power” and inappropriately suggests a similar vein and PTFE bypass performance (P = .065) at 104 weeks’ follow-up (81% primary patency for vein as compared with 67% for PTFE). Note that in our report, the 2-year assisted primary patency was 80.7% in the vein group as compared with 69.4% in the PTFE group, results that were very similar to those of Burger but with more patients and longer follow up were significantly different (P = .03).

The major question raised by Dr Dardik’s letter is whether a new but similar randomized study needs to be performed in the 21st century with special postoperative duplex surveillance for patency and aneurysmal formation. I certainly endorse such an
effort and look forward to the long-term results, which will probably not be available for 5 to 7 years. Until then, one should consider many factors, including the results of previously reported, well-designed, randomized studies, when choosing a bypass conduit.

Willard C. Johnson, MD
Kelvin K. Lee, PhD
Palo Alto, Veterans Affairs Medical Center
Palo Alto, Calif

REFERENCES

24/41/112316

Regarding “Ambulatory venous pressure revisited”
To the Editors:
Neglen and Raju simultaneously measured ambulatory venous pressure in the popliteal and dorsal foot veins. They stated that the pressure drop in the dorsal foot vein was more marked than in the popliteal vein. They concluded that ambulatory dorsal venous pressure was not always accurate in detecting changes in the pressure of the tibial and popliteal veins. Although the behavior of the dorsal foot venous pressure may be normal, deep venous pressure may decrease to a lesser degree or even increase. Their statement merits some comment.
The measurements of Neglen and Raju showed, in reality, that a pressure difference occurred between the popliteal and the dorsal foot veins during activation of the muscle venous pump. Höjensgard and Stürup in 19521 and Arnoldi in 19662 first reported that the ambulatory pressure in the posterior tibial vein decreased considerably during ambulation, whereas it did not decline in the popliteal vein. They only noted this fact and did not point out its implication for the venous circulation of the lower extremity. In fact, the ambulatory pressure gradient occurring between the femoral/popliteal vein and the veins beneath the knee level plays an important role in the venous hemodynamics. This issue is discussed in my recent paper.3 The pressure gradient explains why the blood flows downward in the insufficient saphenous vein and inward through the calf perforators during ambulation.4 It explains, further, why the reflux can only take place in an insufficient vein connecting the femoral, popliteal, or iliac vein with one of the deep veins of the lower leg. Moreover, the pressure gradient may be the trigger factor initiating neovascularization. When high ligation of the saphenofemoral junction without stripping of the insufficient saphenous stem is performed, the insufficient saphenous trunk in the thigh remains patent in most cases. Venous pressure measurements have shown that the low ambulatory pressure extends from the deep veins of the lower leg into the blind saphenous trunk in the thigh.3 This results in a pressure gradient between the femoral vein and the blind saphenous trunk and may initiate neovascularization, because the pressure gradient is an important hemodynamic impetus to the formation of collateral circulation.
Several authors have proved that the pressures in the superficial and deep veins of the lower leg are similar or almost identical.1, 2, 5-7 These veins form conjoined vessels due to numerous communicating veins. The opinion expressed by Neglen and Raju that the pressure changes in the superficial veins do not reflect those in the deep veins of the lower leg is not substantiated. With regard to the previously mentioned ambulatory pressure gradient, it was not surprising that Neglen and Raju found an ambulatory pressure difference between the popliteal and dorsal foot veins.

Cestmir Recek, MD
Vienna, Austria

REFERENCES

24/41/112299

Reply
We have read with interest the comments of Dr Recek regarding our article “Ambulatory venous pressure revisited (J Vasc Surg 2000;31:1206-13). His comments include a criticism of our interpretation of the data as presented in the paper as well as a synopsis of his own hypothesis regarding the origin of saphenous reflux and neovascularization. The latter subjects were not covered in our article, but we are able to respond to specific criticisms of the material presented in our manuscript.
The major thrust of his criticism appears to be that we have only described a “difference” in pressures between the dorsal vein and the popliteal vein, a fact already known (Hojensgard and Stürup 1952, Arnoldi 1966), and thus nothing new. Dr Recek further asserts that, contrary to our interpretation, “several authors have proved that the pressures in the superficial and deep veins of the lower leg are similar or almost identical” because of equilibration from the presence of numerous communicating veins. This criticism appears to be based on an inaccurate and/or incomplete reading of our data (Table I, page 1209) and our interpretation of it, as detailed in the article. We identified three categories of patients in whom the popliteal pressure respectively decreased markedly (group A), decreased marginally (group B), and increased (Group C) in response to calf exercise. Yet in all three groups the dorsal vein pressure decreased markedly in simi-