

to overread the degree of stenosis by noninvasive studies. At the time of the study, the technique for cervical evaluation was two-dimensional time-of-flight. We now perform three-dimensional time-of-flight and agree with Dr. Hartnell that this is a more useful and accurate technique.

Of note, the MRA scans were performed with a "gradient echo pulse sequence," not a "gradient echocardiographic pulse sequence," as stated in the article.

It was not the intention of our article to totally exclude MRA from the clinical decisionmaking process for carotid endarterectomy. If a patient comes to us with clear-cut carotid territory symptoms (or for that matter, asymptomatic disease) with a technically good duplex ultrasound scan from our ACAS-certified vascular laboratory showing 80% to 99% stenosis, then we would perform surgery on that patient on the basis of duplex ultrasound alone. If either the symptoms are not clearly carotid territory or the duplex ultrasound scan does not clearly show severe stenosis, then we would proceed with MRA, now using three-dimensional time-of-flight in the neck, or in some instances proceed directly to conventional arteriography.

Harold J. Welch, MD  
William C. Mackey, MD  
Samuel M. Wolpert, MD

Divisions of Vascular Surgery and Neuroradiology  
New England Medical Center  
750 Washington St.  
Boston, MA 02111

24/41/80565

### Regarding "Complications of iliac artery stent deployment"

*To the Editors:*

I disagree with the premise of Ballard et al. (1996;24:545-55) that complications in percutaneous stent placement are underreported and that high technical success rates are the result of "adjunctive endovascular maneuvers" that should be considered "complications." The abstract claims that iliac artery stent placement was associated with a 19.4% incidence of procedure-related complications in 98 limbs. The manuscript describes two iliac artery ruptures, two distal embolizations, and one stent infection, which are legitimate complications. However, this report also describes seven "dissections" associated with hemodynamically significant gradients that required additional stent placement, three "dissections" that were not flow-limiting and did not require additional stent placement, and one stent "embolization" that was retrieved percutaneously.

Because "dissection" is necessary in percutaneous angioplasty and occurs with every successful procedure,<sup>1</sup> and because gradients were presumably present before stent placement, it is incorrect to consider many of these observations "complications." Even the "embolized" Palmaz stent that was retrieved and an additional stent deployed can be disputed as a "complication." *Stedman's Medical Dictionary* defines "complication" as "a morbid process or

event occurring during a disease which is not an essential part of the disease...."<sup>2</sup> Most of the events that the authors term "complications" were not associated with any morbidity, but relate to technical features of the procedure, analogous to placement of additional sutures in a leaky anastomosis.

As a reference standard for defining complications, I would suggest reviewing experience with the surgical alternative, which the authors conclude would be suitable for randomized comparison. Review of published surgical experience with aortofemoral bypass grafting procedures for aortoiliac insufficiency that have been published since 1993 reveals six large series with a total of 1270 patients.<sup>3-8</sup> The weighted surgical mortality rate was 4%, and the major complications rate was 21%. Complications included myocardial infarction, congestive heart failure, multisystem organ failure, stroke, spinal cord ischemia, intestinal infarction, aortoenteric fistula, acute renal failure, respiratory, and so forth. These complications are similar to those described in practice guidelines published previously in the *Journal of Vascular Surgery*.<sup>9</sup> The only remotely comparable complications in Ballard's article are three stent occlusions, two ruptures, one infection, and two distal embolizations, for a major complication rate of 8%. They reported no 30-day deaths. These results are consistent with those of previous reports of aortoiliac stenting.<sup>10-13</sup> The authors' call for randomization to stent placement or aortofemoral bypass grafting is not supported by their data or by previous published experience.

It would be unfortunate if patients were presented inflated "complication" rates such as these during discussions about treatment strategies. An important point that will hopefully not be missed when this report is cited is that technically successful results were achieved in 97% of patients, surgical intervention was required in only 3%, and the 30-day mortality rate was 0%. Why would the authors consider this unworthy of mention in the discussion or in the conclusion of the abstract? It is interesting to note that despite concluding that iliac artery stent placement is "...an invasive procedure that has associated limb-threatening and life-threatening complications," midway through the trial they began performing them as outpatient procedures. How do the authors explain this paradox?

Timothy P. Murphy, MD

Director, Division of Vascular and Interventional Radiology  
Rhode Island Hospital  
593 Eddy St.  
Providence, RI 02903

### REFERENCES

1. Castadeda WR, Formanek A, Tadavarthy M, Vladaver Z, Edwards JE, Zollkofer C, et al. The mechanism of balloon angioplasty. *Radiology* 1980;135:566-71.
2. Complication. *Stedman's Medical Dictionary*. 24th ed. Baltimore: Williams & Wilkins, 1982:306.
3. Hans SS. Concurrent audit of early outcome for 1,617 consecutive arterial reconstructions. *Surg Gynecol Obstet* 1993;176:382-6.

- Littooy FN, Steffan G, Steinam S, et al. An 11-year experience with aortofemoral bypass grafting. *Cardiovasc Surg* 1993;1:232-8.
- Schneider JR, Besso SR, Walsh DB, Zwolak RM, Cronenwett JL. Femorofemoral versus aortobifemoral bypass: outcome and hemodynamic results. *J Vasc Surg* 1994;19:43-57.
- Huber TS, Harward TRS, Flynn TC, Albright JL, Seeger JM. Operative mortality rates after elective infrarenal aortic reconstructions. *J Vasc Surg* 1995;22:287-94.
- Erdoes LS, Bernhard VM, Berman SS. Aortofemoral graft occlusion: strategy and timing of reoperation. *Cardiovasc Surg* 1995;3:277-83.
- Passman MA, Taylor LM Jr, Moneta GL, Edwards JM, Yeager RA, McConnell DB, et al. Comparison of axillofemoral and aortofemoral bypass for aortoiliac occlusive disease. *J Vasc Surg* 1996;23:263-71.
- DeWeese JA, Leather R, Porter J. Practice guidelines: lower extremity revascularization. *J Vasc Surg* 1993;18:280-94.
- Vorwerk D, Guenther RW, Schurmann K, et al. Primary stent placement for chronic iliac artery occlusions: follow-up results in 103 patients. *Radiology* 1995;194:745-9.
- Martin EC, Katzen BT, Benenati JF, et al. Multicenter trial of the Wallstent in the iliac and femoral arteries. *J Vasc Interv Radiol* 1995;6:843-50.
- Murphy TP, Webb MS, Lambiase RE, et al. Percutaneous revascularization of complex iliac artery stenoses and occlusions with use of Wallstents: three-year experience. *J Vasc Interv Radiol* 1996;7:21-7.
- Strecker EP, Boos IB, Hagen B. Flexible tantalum stents for the treatment of iliac artery lesions: long-term patency, complications, and risk factors. *Radiology* 1996;199:641-7.

24/41/80518

### Reply

*To the Editors:*

We appreciate the thoughtful and well-researched letter from Dr. Murphy. His reply is consistent with the outlook of most interventional radiologists as they report their experiences.

A major point made by our report is that the format used by interventional radiologists is inconsistent with vascular surgical reporting standards. Dr. Murphy demonstrates the differing practices very well in his letter. For example, disputing the fact of an embolized stent as a complication is a clear deviation from vascular practice. Also, deployment of an additional \$600 to \$900 stent in the management of hemodynamically significant iatrogenic dissections remote from or adjacent to the target lesion cannot be compared with placement of an additional suture in an anastomosis. In a recently presented update of our iliac stent experience,<sup>1</sup> we found the relative risk of a complication to be increased when multiple stents are used (relative risk, 2.65;  $p = 0.05$ ) during the procedure. Furthermore, the relative risk of subsequent iliac artery thrombosis was heavily influenced by the incidence of a procedure-related complication (relative risk, 15.33;  $p < 0.0001$ ).

Dr. Murphy correctly makes the point that surgical intervention is more morbid than percutaneous angioplasty. We have no disagreement with this fact and instead

make a plea for uniform reporting of complications. As interventionalists and vascular surgeons work increasingly close together, it is crucial that they speak a common language. Surgeons are trained to be meticulous in reporting complications and in discussing them openly. Successful management of an untoward event as described by Dr. Murphy is a tribute to the skill of the interventional radiologist but does not erase the fact of the complication. Lastly, although our experience demonstrates that outpatient iliac artery stent deployment can be done, that has not decreased our appreciation for the potential problems that can occur with the procedure.

As Dr. Murphy's letter provides strong evidence for the main point made in our article, we are grateful to him for taking the time to respond.

*Jeffrey L. Ballard, MD*

Division of Vascular Surgery  
Loma Linda Medical Center  
11234 Anderson St., Room 2586A  
Loma Linda, CA 92354

### REFERENCE

- Ballard JL. Complications of iliac artery stent deployment and their management. Presented at the 23rd Annual Symposium on Current Critical Problems, New Horizons and Techniques in Vascular and Endovascular Surgery, New York, Nov. 21-24, 1996.

24/41/80519

### Regarding "Upper dorsal thoracoscopic sympathectomy for palmar hyperhidrosis: Improved intermediate-term results"

*To the Editors:*

Since 1990 we have performed more than 200 thoracoscopic sympathectomy procedures (in 100 patients) for palmar-axillary hyperhidrosis or Raynaud's phenomenon.

Contrary to the technique of Kopelman et al. (1996; 24:194-9), we have always used double-lumen intubation for ipsilateral lung collapse. Single-lumen intubation and CO<sub>2</sub> insufflation entails the risk of tension pneumothorax and dramatic hemodynamic changes as a result of elevated intrathoracic pressures and is to be avoided, in our opinion.

We have never seen postoperative atelectasis because we always ask the anesthesiologist to manually ventilate with positive end-expiratory pressure at the moment of instrument withdrawal. Only in one patient with bullous emphysema was postoperative thoracic drainage required, and all procedures were performed as planned.

We operate with the patient in a semisupine position, both sides consecutively, through two stab-wound incisions anterior on the chest in the second intercostal space. We are surprised by the high rate of complications (Horner's syndrome, brachialgia) described by Dr. Kopelman et al., which we think are a result of their surgical technique. We have always performed transpleural coagulation of T2-T3-T4 ganglia, avoiding any dissection of or traction on the sympathetic chain. Performing this technique, we have