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Commentary regarding “Lower-extremity endovascular interventions for Medicare beneficiaries: Comparative effectiveness as a function of provider specialty” by Zafar et al. J Vasc Interv Radiol 2012;23:3-9

In the preparation of this commentary, four separate individuals with extensive publications, in particular, with use of the Medicare database, independently reviewed the article in question,¹ and readers of JVS will note that it was reviewed in the August 2012 Abstracts section of the *Journal*. I note that the study in the above-referenced article was supported by a grant from the Society of Interventional Radiology (SIR), and that the corresponding author was, at the time of publication of the article, the SIR President. Furthermore, the same individual continued to malign vascular surgeons with respect to their endovascular competence in his SIR presidential address, stating that “it is impossible . . . to be a master of such disparate skills as open revascularization and IR.”² It is clear then that the article in question is but a vehicle to promulgate this prejudiced and self-serving point of view. The unfounded accusation, stated in the article with respect to monetary concerns motivating the practice of vascular surgeons (VS), is repugnant to be included in a scientific publication. To wit, the introductory paragraph states the authors’ message, which is that VS offer endovascular procedures related to economic greed. This theme is continued in the Discussion section where the authors contend that vascular surgeons offering both open and endovascular procedures represents an inherent conflict of interest, encompassing some sinister plot that VS are perpetuating against Medicare beneficiaries to capture more revenue. This accusation represents a perverse aberration of the facts in the practice of vascular interventions. Since VS are the only specialists offering both open and endovascular options to patients, it must be apparent that VS make treatment recommenda-

tions without prejudice or bias to any particular mode of intervention since they can perform both modalities.

As in other vascular territories, treatment of leg occlusive disease has largely shifted from open to endovascular procedures, and Interventional Radiologists (IR) “market share” for such procedures declined from 67% in 1996 to less than 20% in 2006.³ Patients are best served when VS, who provide comprehensive care and longitudinal follow-up, also perform their intervention, irrespective of its nature. It is certainly true that the Society for Vascular Surgery (SVS) and others engineered a retraining of the VS workforce over the past 15 years. It is also true that in terms of endovascular training, the paradigm has shifted completely. VS trainees have had mandated endovascular training with case number requirements for a decade now, and their experience in this realm now far exceeds that of IR fellows.

The very structure of the study and its end points are illogical. The selection of end points such as transfusion, use of intensive care services, and length of stay are often irrelevant (in hospitalized patients) to an endovascular procedure per se; rather, they reflect the overall complexity and/or complicating patient comorbidities that may dominate the clinical picture. Nor can the Medicare database used in this study discern the temporal relationship of end points such as intensive care unit stay to the vascular intervention. These are the realities of clinical practice that only those involved in actual patient care can appreciate. In consideration of the mortality end point, the authors repeatedly call attention to the “19% higher mortality with vascular surgeons” (data not shown), but they note this difference was not significant. In addition, the selection of a subsequent revascularization or amputation as a surrogate for procedural quality is seriously flawed and introduces the single glaring flaw in this article, viz the failure to include specifics of the *indication for vascular intervention* (ie, whether the indication for intervention was claudication or limb-threatening ischemia). These data are indeed available in the Medicare database that the authors used, but we are told that inclusion of such data in regression models were “less predictive and had a low R-square values.” The trans-

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lation, obviously, is that when adjusting for vascular disease severity, the authors' preconceived message was no longer supported by the data. The authors' attempt to manipulate the data with illogical surrogates for procedural expertise may be in response to prior work, using the Nationwide Inpatient Sample database that demonstrated both procedural mortality and iatrogenic arterial injury to be significantly higher for IR and cardiologists as compared to VS.⁴ Indeed, this report also utilized appropriate risk adjusted multivariate analyses, which is inherently lacking in the *Journal of Vascular and Interventional Radiology* article. Furthermore, it is well documented that VS are significantly more likely to treat patients with rest pain and tissue loss (limb-threatening ischemia) as compared to claudicants.⁵ It is also documented and intuitively logical that virtually all of the end points that the authors consider, including mortality, length of stay, amputation rates, and overall resource consumption, are significantly higher in patients treated for limb-threatening ischemia as opposed to claudication.⁶ The analysis was adjusted only for age, sex, race, admission type, and a general comorbidity measure (Elixhauser method), which accounts for 30 variables including irrelevant factors such as weight loss, hypothyroidism, and paralysis but not for the indication for the procedure. Thus, when used as a total score, this method includes a great deal of "noise," making it largely irrelevant to treatment outcomes in patients with PAD. Such indices were developed to be used at administrative levels in comparing hospital or health systems rather than individual patient outcomes.

Multicollinearity is another problem that can lead to biased estimates of the parameter standard errors. This should have been tested for and fixed when it exists. Apparently, this was not done during the analysis of the data in this study. In addition, the authors made little attempt to match cases across providers. The standard way is to use propensity score matching. These scores should be derived using risk models to predict provider type; this might have allowed for a comparison of different provider types' outcomes when compared based on similar patients. This was also not done. Therefore, it is rather surprising that the authors (and reviewers) of this article appear to have ignored the fact that a correlation is not the same as a cause. Even if it had used the appropriate matching methods described above, this is not a study that can establish a causal relationship between provider type and outcome

since it is retrospective and utilized correlational data. As noted above, patients do indeed differ across provider types⁵; therefore, the conclusion that VS delivered worse outcomes to Medicare patients reaches beyond what this type of study is capable of demonstrating, particularly in the absence of appropriate risk adjustment.

Finally, the use of the end point of repeat revascularization or *any* amputation as the surrogate for quality of the index procedure is patently absurd. A digital or forefoot amputation is often performed subsequent to a lower extremity revascularization in patients with tissue loss. Furthermore, secondary interventions after infrainguinal endovascular procedures are commonly required to maintain secondary (reported ranges, 15-30%) patency.⁷

This article should be recognized as a self-serving slander against VS, doubtlessly motivated by or in response to the major shift in medical specialty provider demographics for lower extremity endovascular interventions.³ The lack of scientific and analytical rigor completely undermines the validity of its conclusions.

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