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Letters

TO THE EDITOR

Endovascular Stenting of Superior Mesenteric Artery Branches

Friend or Foe

We read with great interest the paper by Colkesen et al. (1). Thrombus occluding the superior mesenteric artery (SMA) distal branch was confirmed by angiogram (Figure 1B). A balloon-expandable stent was placed after catheter aspiration due to flow limiting lesion remained distally. However, we would like to elaborate on the stenting of SMA distal branch.

First, multiple investigators have reported the effectiveness of infusion thrombolytic/antispasmodic agents in the treatment of the thrombus or spasm of SMA (2–4). The flow-limiting lesion after catheter aspiration of this case was mostly due to remnant thrombus or spasm of the SMA, and the authors should infuse thrombolytic/antispasmodic agents via a microcatheter before deciding to place a stent. Second, clinical success depends on resolution of abdominal pain, and stent placement is unnecessary if the abdominal pain resolved after catheter aspiration. Besides, the distal branch beyond the occlusion can be seen during angiography due to the development of collateral arteries (Figure 1B). Third, although endovascular stent placement provided immediate symptomatic improvement, stent placement has many potential drawbacks, including stent restenosis and obliteration of side branches of the SMA.

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REPLY: Endovascular Stenting of Superior Mesenteric Artery Branches

Friend or Foe

We appreciate the valuable commentary on our paper (1) by Dr. Jia and colleagues and the time they spent reading and appraising our paper.

Large clinical trials for endovascular treatment (ET) and systematic comparison of ET modalities (i.e., transcatheter lytic therapy, mechanical thromboembolectomy, balloon angioplasty, and stenting) are not yet available in acute mesenteric ischemia. Hence, the European Society of Cardiology and American College of Cardiology/American Heart Association guidelines recommend ET at Level of Evidence: C without emphasizing priority or superiority of one modality to the other (2,3).

Results of 28 published articles reporting on acute mesenteric ischemia of 234 patients treated by ET support the previously mentioned statement. Thrombolytic infusion was performed in 43% of patients (with adjunctive angioplasty or stent placement in 20%), mechanical thromboembolectomy in 12% (with adjunctive angioplasty or stent placement in 12%), and angioplasty or stent placement in 36% as a primary treatment method. Determining the choice of intervention depended on clinical status of the patient and anatomical suitability (4). In a similar manner, Jia et al. (5) suggested that aspiration should be an initial treatment, adjunctive local thrombolysis should be performed if aspiration fails, and stenting is a treatment choice if both aspiration and adjunctive local thrombolysis fail. Jia et al. (5) recommended that thrombolytic or antispasmodic agents should have been infused before the decision of stent

placement. Due to uncertainty in history of recent gastrointestinal bleeding, we decided to start with the mechanical thromboembolectomy in our case (1).

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Scientific Data and Transparency of Conflict of Interest Are Important, Not Biased Editorial Without Facts

The tone of the editorial comment by White and Reilly (1) resembles the current U.S. political environment, which is characterized by assumptions and allegations without any hard facts or scientific arguments.

The journal editors were wise to publish these comments; if they had not done so, the authors might have made an even larger story out of this. With our comments in reply, the readers may now make up their own minds. Our original contribution was submitted to *JACC: Cardiovascular Interventions* and underwent a rigorous scientific review process, with several reviewers and with an editor carefully checking our manuscript, data, and conclusions. Indeed several back-and-forth edited versions took place before final publication of our article (2). All authors made it very clear what kind of potential conflicts existed according to current standard practice. Nothing was hidden. To label the article "biased" (2) without any facts to support their allegation rather reflects the biased view of Drs. White and Reilly.

The 3 employees were coauthors of the publication due to their substantial contributions. They invented the technology, developed the technology, tested it in preclinical animal studies, contributed to the clinical protocol, and trained the physicians in the procedure. Two of the employee authors, Drs. Gertner and Dawood, are experienced interventional clinicians and researchers, and the third, Dr. Anderson, has been working the field of therapeutic ultrasound for over 4 decades. They did not contribute to data collection, analysis, or interpretation of the data.

The conclusion of the editorial (1) is simply reiterating our last sentence (2). Already in August 2014, the WAVE IV (Sham Controlled Study of Renal Denervation for Subjects With Uncontrolled Hypertension) study started (NCT02029885) and results of an interim analysis will soon be released. We prefer to spend our time conducting and analyzing this randomized, double-blind, sham-controlled denervation study in resistant hypertension rather than defending against largely biased comments.

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