Authors have nothing to disclose with regard to commercial support.

involvement was undocumented in the 5 patients with immunoglobulin light chains-type amyloidosis, the literature would support that 50% of these patients also have such involvement.³ Our study cohort was identified retrospectively by collating 2 separate databases. We feel that this represents our best-available data.

Another criticism is that there were probably patients at our institution who were declined intervention because of “advanced amyloidosis.”¹ Given the nature of our retrospective study, there was no way for us to identify such patients. We agree that our patient cohort was a selected group (see the Discussion in our article), but we feel that declining intervention on patients with “advanced amyloidosis” represents just good surgical judgment.²

Scully and colleagues also suggest that more than 98% of our cases were either missed or excluded from intervention.¹ They base their assumption on an estimated prevalence of 6% to 16% for cardiac amyloidosis in patients with severe aortic valve stenosis. We agree that our prevalence is an underestimation (see Discussion).² We would like to point out that the authors’ estimated prevalence comes from only 3 studies, each with comparable small numbers of patients with cardiac amyloidosis (ie, 6, 14, and 24).

The final criticism from Scully and colleagues is that they advocate caution with respect to our reported outcomes.¹ We had no cases of operative stroke or dialysis and no operative or 1-year mortality in 11 patients with transthyretin/prealbumin-type amyloidosis (n = 6) and immunoglobulin light chains-type amyloidosis (n = 5).² We updated our vital status follow-up for this response, and our Kaplan–Meier estimated 2-year mortality stands now at $16.7 \pm 10.8\%$. Our estimated mortality is consistent with what was reported in the prospective, randomized, intermediate-risk aortic valve replacement Placement of Aortic Transcatheter Valves (PARTNER) 2A trial.⁴

Scully and colleagues¹ go on to report “red flags” for occult cardiac amyloidosis in elderly patients with aortic stenosis. Several of those “red flags” (eg, advanced age, male sex, and low-flow, low-gradient aortic valve stenosis) can be found in the patients in the low-flow severe aortic stenosis PARTNER trial.⁵ In that study, importantly, survival was improved with transcatheter aortic valve replacement compared with medical management and was similar with transcatheter and surgical aortic valve replacement.

Patient selection is critical when offering aortic valve replacement to patients with amyloidosis. However, based on the evidence presented, we feel that aortic valve replacement should not be denied solely on the basis of underlying amyloidosis.

Amit P. Java, MD^a

Kevin L. Greason, MD^a

Angela Dispenzieri, MD^b

Martha Grogan, MD^c

Joseph J. Maleszewski, MD^d

Richard C. Daly, MD^a

Mackram F. Eleid, MD^c

Hartzell V. Schaff, MD^a

Departments of ^aCardiovascular Surgery

^bInternal Medicine

^cCardiovascular Diseases

^dLaboratory Medicine and Pathology

Mayo Clinic

Rochester, Minn

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ALPHA-STAT VERSUS pH STAT ACID BASE BALANCE FOR AORTIC ARCH SURGERY: THE DEBATE CONTINUES

Reply to the Editor:

In his letter to the Editor regarding the recent article by Damberg and colleagues¹ and my accompanying editorial commentary,² Jonas³ offers several criticisms and suggestions regarding the management of adult patients who require intervals of hypothermic circulatory arrest (HCA) during operations on the thoracic aorta that involve the aortic arch. His major criticism relates to use of the alpha-stat strategy for acid-base management during these procedures, and he discusses why he considers the pH-stat strategy to be preferable to the alpha-stat strategy in this setting. This recommendation is based primarily on a randomized, controlled clinical trial in infants reported by his group (du Plessis and colleagues⁴) in 1997, which showed that use of the pH-stat strategy during procedures that required deep hypothermic cardiopulmonary bypass, which included patients requiring deep HCA, was

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associated with improved perioperative outcomes relative to the alpha-stat strategy.

The debate regarding the optimal strategy for acid-base management during operations on the thoracic aorta involving use of HCA as the primary method for brain protection in adults is ongoing and unresolved.⁵ A major reason for the continued resistance to widespread adoption of the pH-stat strategy in adult patients, particularly the elderly, is concern regarding the potential risk of microembolization and macroembolization resulting from the “luxury perfusion” that occurs with the pH-stat technique.^{6,7} In a previous study by the Elefteriades group (Ziganshin and colleagues⁸) of straight deep HCA for brain protection during aortic arch surgery with the alpha-stat strategy, stroke occurred in 8 (1.6%) of 490 patients. Brain imaging studies determined the nature of the stroke to be embolic in 5 of the 8 patients (62.5%). Concern about the risk of embolization in adults during these procedures is appropriate.

With regard to the admonition by Jonas³ to include cognitive assessment in trials to assess optimal acid-base strategies, at least 4 studies, including 1 randomized trial, have assessed neurocognitive function in patients in whom straight HCA was used as the principal method of brain protection in combination with the alpha-stat strategy, and all found this function to be preserved.⁹⁻¹² The randomized trial assessed neurocognitive function after HCA, and also after 2 other methods of brain protection (retrograde and antegrade perfusion), and failed to show added neurocognitive benefits with these alternative techniques.¹⁰ Although neurocognitive impairment was noted 3 to 6 days postoperatively in 96% of patients in the 3 arms of the study, complete recovery was noted in all patients by 6 months. It is of interest that in a follow-up study of the infants in the randomized trial reported by Jonas’s group referred to previously,⁴ the type of acid-base management strategy (alpha-stat vs pH-stat) did not have a consistent effect on neurodevelopmental outcome at 1 year of age or at 2 to 4 years of age.¹³ In further defense of the alpha-stat strategy, in a poll of surgeons from 10 of some of the largest aortic surgery centers in the United States (including my own and that of Elefteriades), the alpha-stat strategy is used in all but one of the centers.

I agree with Jonas that extreme hemodilution should be avoided during operations with HCA. In my practice, we maintain the hematocrit at a level of 24% or greater during the cooling and rewarming phases of the procedure.

With the trend toward increasing use of antegrade or retrograde cerebral perfusion with moderate hypothermia as the primary methods for brain protection during operations

involving the thoracic aorta, it is doubtful that randomized trials comparing alpha-stat and pH-stat strategies or varying hematocrit levels in patients in whom straight HCA is used as the primary mode of brain protection will ever be conducted. There appears to be no advantage in attempting to increase the safe duration of circulatory arrest with hypothermia as the sole method of brain protection. Because the optimal method of brain protection remains to be determined, however, there is a clear need for large prospective randomized trials to compare the current widely used techniques, and these trials should incorporate assessment of neurocognitive function into the protocols.

Nicholas T. Kouchoukos, MD

Division of Cardiovascular and Thoracic Surgery
Heart Center
Missouri Baptist Medical Center
BJC HealthCare
St Louis, Mo

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