the intensive care unit with a high incidence of delayed sternal closure. Open sternums do translate into a high risk of infection in tropical countries² and may adversely affect the outcome. Also with a single-stage strategy, extracorporeal membrane oxygenation support may not always be available if there is difficulty weaning these patients from CPB, particularly after an extensive 2-stage procedure in a neonate. Under these circumstances, if the anatomy of the CoA is suitable for repair and there is no need for arch repair, a 2-stage approach can often be justified, particularly in the neonatal age group in whom the risk of CPB and total circulatory arrest related complications is higher. These patients can have the VSD closure in a safe manner at a later stage, which is also technically simpler in an older patient.

In patients beyond the neonatal period with uncomplicated CoA, we prefer the single-stage approach described by Kanter and colleagues,³ in which the CoA is first repaired via a posterolateral thoracotomy, the patient is turned supine, and the VSD is closed using standard CPB. The advantages of this approach are its simplicity, better CoA management (debatable), less CPB times, and avoidance of circulatory arrest. However, this strategy is not applicable to patients with arch hypoplasia and has the disadvantage of 2 incisions.

In most of the public hospitals in India and other developing countries, an initial repair of the CoA costs approximately \$500 and a subsequent second stage can be accomplished at less than \$1500, but if we were to adopt a 1-stage approach, we may incur a substantial increase in costs because of a higher morbidity and longer intensive care unit stay. Despite the claimed advantages of a single-stage approach, we continue to advocate the 2-stage approach in developing countries, particularly in those centers where "proficiency" has not been achieved in managing these critically ill small neo-

nates. The authors are to be congratulated on successfully achieving this.

Sachin Talwar, MCh
Shiv Kumar Choudhary, MCh
Balram Airan, MCh
Cardiothoracic Center
All India Institute of Medical Sciences
New Delhi, India

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Reply to the Editor:

As Talwar and colleagues state in their Letter to the Editor, we acknowledged the obvious and unavoidable era bias in our article. The 3 patients who underwent repair of coarctation (CoA) of the aorta *alone*, using a median sternotomy incision with cardiopulmonary bypass, had significant proximal aortic arch hypoplasia that precluded satisfactory repair through a left posterolateral thoracotomy incision. In the era during which those patients presented, we were not performing single-stage repair of CoA with ventricular septal defect (VSD).

We agree that the socioeconomic context in which the patient with CoA-VSD presents should be considered when choosing the surgical strategy, namely, single- versus 2-stage repair. Similarly, institutional resources and surgeon-specific variables should also be factored into this decision. We have documented, in our own institutional experience, that the increased need for delayed sternal closure has now been neutralized¹ and that the risk of mediastinitis with delayed sternal closure,

when used, is minimal,² but the same may not be true in other centers.

Nonetheless, the advantages of single-stage repair that we have clearly documented, such as earlier age at complete repair, as well as the hypothetical advantages that we have postulated, such as better neurodevelopmental outcomes related to earlier complete repair, may, in the end, transcend the economic advantages of 2-stage repair. Therefore, the single-stage repair of CoA-VSD could, in the future, become a goal worth pursuing in all centers regardless of socioeconomic barriers. With further research on neurodevelopmental outcomes, the advantages of this technique may become important enough to serve as a stimulus for the development of resources to accommodate its successful and improved implementation.

Henry L. Walters, III, MD
Department of Cardiovascular Surgery
Children's Hospital of Michigan
Detroit Medical Center
Wayne State University School of
Medicine
Detroit, Mich

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SOLID AND GASEOUS CEREBRAL MICROEMBOLIZATION AFTER BIOLOGIC AND MECHANICAL AORTIC VALVE REPLACEMENT: INVESTIGATION WITH MULTIRANGE AND MULTIFREQUENCY TRANSCRANIAL DOPPLER ULTRASOUND

To the Editor:

We read with interest the recent article by Guerrieri Wolf and colleagues¹

describing their experience of a transcranial Doppler system (Embo-Dop; DWL Elektronische Systeme GmbH, Singen, Germany) to discriminate between gaseous and particulate emboli after aortic valve replacement. The main finding of their study was that solid microemboli occurred more frequently after mechanical aortic valve replacement compared with biological valve replacement at 3 months after surgery. The inability to distinguish the difference between solid and gaseous microemboli has limited the use of standard transcranial Doppler when both coexist, for example, during open carotid endarterectomy. As the authors state, this particular Doppler technique was first assessed by Brucher and Russell² in a 2-part study comprising both in vitro and in vivo models. The in vivo part used 45 patients with carotid artery stenosis (39 symptomatic), producing a relatively low number of 32 embolic signals, of which 30 were classified as solid.

The assumption in validation studies is that all patients with carotid artery stenosis produce only solid emboli. One arm of a clinical validation study performed by Markus and Punter³ used a cohort of patients with symptomatic carotid artery stenosis to generate 145 presumed solid embolic signals. Of these 145 signals, the Embo-Dop system classified 72 as gaseous emboli. This gave a sensitivity of 50.3% and specificity of 96.5% for detecting solid embolic signals. Markus and Punter concluded that although the Embo-Dop system allowed better discrimination than simple intensity threshold alone, it was not accurate enough for clinical or research studies. Unless the specificity is approximately 100%, a significant number will be classified wrongly as potentially significant solid emboli. We believe that Embo-Dop has to be used with caution in distinguishing gaseous from solid emboli.

The authors comment on no antiplatelet therapy after mechanical versus biological valve replacement. Our

group conducted a UK survey of consultant practice and literature review of this subject.⁴ We concluded that the incidence of thromboembolism after aortic bioprosthesis is highest in the first 3 months but then diminishes. Whether this is related to endothelialization of prosthetic material or specifically to antiplatelet therapy is unknown. Therefore, their observation that the proportion of particulate emboli at 3 months was in the biological valve replacement cohort receiving aspirin but not in the mechanical group receiving only warfarin could indeed be explained by the antiplatelet function of aspirin.

Justin Nowell, MRCS
Marjan Jahangiri, FRCS (CTh)
St George's Hospital
London, United Kingdom

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Reply to the Editor:

We thank Nowell and Jahangiri for their interest in our recent article¹ using a novel transcranial Doppler system (Embo-Dop; DWL Elektronische Systeme GmbH, Singen, Germany) to investigate the number and nature of cerebral microemboli in recipients of biological and mechanical aortic valve prostheses. The 2 important issues highlighted by the authors in their

Letter to the Editor were, in fact, both discussed specifically in our article.

The first issue is the potentially low sensitivity and specificity of the Embo-Dop system as suggested in the clinical validation study by Markus and Punter.² With specific regard to that study, we wrote in the discussion section of our article: "More recently, Markus and Punter, while confirming the superior discrimination achievable with this new Embo-Dop transcranial Doppler system compared with conventional transcranial Doppler ultrasound, reported it to have a sensitivity of only 50% but a specificity of 96% for detecting solid microemboli in vivo. If confirmed, that finding will only serve to emphasize the already significantly greater solid microembolic load detected with mechanical rather than biological prostheses." In other words, because the sensitivity of the Embo-Dop system is much lower than its specificity in detecting solid microemboli, this will result in an underestimation of the solid cerebral microembolic load.

The second issue raised by Nowell and Jahangiri is that the reduction of solid microemboli over time in the biological prosthesis recipients, but not in the mechanical prosthesis recipients, could be explained by the use of aspirin in the first group. Again, we discussed that issue in our article stating, "Although biological recipients were all receiving aspirin, mechanical recipients received warfarin without any antiplatelet agents. This could explain the reduction in the proportion of particulate microemboli over time in the biological valve group but not in the mechanical group." We subsequently suggested, "However, the results could also be affected by the use of aspirin in the biologic valve group, which would explain the reduction of microemboli detected over time in group B (biological) but not in group M (mechanical). Indeed, using aspirin instead of warfarin after a biological aortic valve implantation is the current practice among the