for our patients is whether the main “advantage” of transcatheter device closure, namely, its minimally invasive nature, is really worth taking the risk of having a life-threatening complication at a later date, requiring emergency surgery with eventual surgical closure of the septal defect. In contrast, primary surgical closure of an ASD/PFO through a minithoracotomy/ministernotomy is a safe and effective minimally invasive procedure, without the risk of erosion.

It is possible that some patients may have had only minor complications after transcatheter device closure, without the severe clinical complications presented in our case reports. Furthermore, how many centers are really reporting all the postimplantation complications? How many patients are being followed up to assess long-term complications?

The other question we should ask ourselves is, “Are we really only seeing the tip of the iceberg?” Moreover, is even the tip of the iceberg really much bigger than what is being presented?

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Reply to the Editor:

We appreciate the conscientious input by Cherian, Kalangos, and

Cikirikcioglu regarding the risk of erosion associated with Amplatzer septal occluder devices (AGA Medical Corporation, Golden Valley, Minn). We likewise acknowledge that we do not know the true timeline of device erosions. This uncertainty may create some unease among cardiologists and surgeons, but it should not necessarily dissuade us from catheter-based treatment of atrial septal defects (ASDs). Certainly, one may speculate about icebergs, but there is little evidence of those here. The fact remains that device erosion is rare; current postmarket study of the Amplatzer septal occluder device placement may provide some insight into the true incidence of erosions.

Although the letter raises important questions to consider before selecting a transcatheter or surgical approach to treatment of ASDs, we disagree with the implication that erosion is an inherent risk of transcatheter ASD closure. The 2 devices mentioned in the letter (BioSTAR and Solysafe) both have stiff arms, which may perforate and have been removed from the US market. In addition, the Gore Helex device (W. L. Gore & Associates, Inc, Flagstaff, Ariz) has not been associated with any erosions and may represent a safe alternative to surgical intervention.

What we can all agree on is that discussions such as this one are vital to developing safer, less invasive treatment strategies for patients with congenital heart disease.

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EN BLOC REMOVAL OF THE BRAIN AND SPINAL CORD FOR PERFUSION STUDIES

To the Editor:

We were a little puzzled during our reading of Al-Ali and colleagues'

recent article.¹ They mentioned that the brain and spinal cord were photographed and then carefully removed en bloc. Because the epidural space between spinal cord and vertebrae is small, spinal cord removed en bloc was not easy even if the spinal cords were fixed. Which methods and equipment were applied by Al-Ali and colleagues? In our experience, the vertebral column and cranium was decalcified first. Then craniotomy and laminectomy were easily performed to expose the brain and spinal cord in situ. The brain and spinal cord could then be removed en bloc.

In the study of Al-Ali and colleagues,¹ the length of filling in the anterior and posterior spinal arteries was also measured and then converted into a percentage of the total length of the cord. Sometimes, however, spinal arteries, especially the posterior spinal artery, are absent or partially absent (Figure 1). What did they do in such cases?

Al-Ali and colleagues reported measuring the length of the spinal cord from the brainstem to the conus medullaris.¹ In our experience, accurately identifying the initial point of the cauda equina is very hard without incision of the dura mater. How did Al-Ali and colleagues accurately distinguish between the conus medullaris and the cauda equina?

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