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doi:10.1016/j.jtcvs.2006.07.022

Bifid cardiac apex: A rare morphologic structure To the Editor:

I read with great interest the case reported by Sayin and associates¹ in the February 2006 issue of the *Journal*. In this article, the authors presented an exceedingly uncommon pathologic condition and provided new insights about the extremes of cardiac morphogenesis. Although the described case is interesting, there are some concerns about the surgical technique. I would like to make the following comments.

The authors reported that the preoperative oxygen saturation was 92% and, by surgical intervention, the arterial oxygen saturation reached 96% postoperatively. As mentioned by the authors, the right ventricular volume was less than normal and the arterial desaturation was probably related to hypoplasia of the right ventricle.

First, the preoperative and postoperative hemodynamic data were lacking, and

the reader could not obtain the necessary information from the article about the degree of right ventricular and tricuspid valve hypoplasia. Additionally, the long-term effects of this accessory chamber on right ventricular contractile function are not known. An explanation for why a one and a half ventricle repair was not tried was not convincingly given. Finally, the authors must comment on the possible consequences of partial closure of an atrial septal defect on an already elevated right ventricular pressure.

Another issue with the technique is the persistence of a diverticulum-like chamber communicating with the left ventricle. The magnetic resonance imaging indicates that this chamber is large enough. I wonder why the authors did not consider this blind sac as a potential source of postoperative embolism and arrhythmias.

Because there are scanty data in the literature, such rare anomalies and their surgical corrections should be considered as important opportunities for determining the most accurate surgical approach. The description of a rare congenital anomaly is one aspect of this article, but the applied technique may lead to some interpretive errors because of the lack of necessary data.

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doi:10.1016/j.jtcvs.2006.07.027

Reply to the Editor:

We thank Dr Basaran and appreciate his kind comments and suggestions regarding our article.¹ First of all, we have to stress that the purpose of our article was mainly to describe this rare cardiac morphologic condition—bifid cardiac apex. Although the patient was examined by several cardiologists in the preoperative period, none of them was able to diagnose the pathologic condition precisely. During the diagnostic workup, magnetic resonance imaging was also performed; however, the bifid apex and the third chamber could not be identified. As we mentioned in the article, the

preoperative echocardiogram and intraoperative data were not consistent. We had no data about the preoperative right ventricular volume and tricuspid valve. Therefore, intraoperatively, we decided to close the large atrial septal defect partially, without knowing of the existence of the third chamber. We absolutely agree that the long-term effects of this accessory chamber on right ventricular contractile function are not well known. Also, the intermediate and long-term results of one and a half ventricle repair for these kinds of patients are controversial owing to the evidence of pulmonary arteriovenous fistulas on follow-up. However, we disagree with the underestimation of the blind sac as a potential source of postoperative embolism and arrhythmias, because the exact pathologic condition could not be diagnosed preoperatively.

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Reference

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doi:10.1016/j.jtcvs.2006.08.009

Off-pump pulmonary valve placement To the Editor:

I read with great interest the article by Berdat and coworkers on off-pump pulmonary valve placement.¹ The authors present the results in 4 patients who underwent perventricular (transventricular) placement of a stented Shelhigh valve (Shelhigh, Inc, Union, NJ). The first clinical application of this procedure was published recently by Schreiber and colleagues.² I would like to congratulate the authors, who successfully used the perventricular technique for off-pump placement of the pulmonary valve.

The perventricular technique was initially used to close complex muscular ventricular septal defects, and the first report was published in *The Journal of Thoracic and Cardiovascular Surgery* by my colleagues and me.³ This technique has also been used to close perimembranous ventricular septal defects.⁴

The initial experiment with the Shelhigh valve in a self-expandable conduit was performed in sheep by us and the results published as a book chapter in *Transcatheter Valve Repair* by Hijazi, Bonhoeffer, Feldman, and Ruiz.⁵ In this animal experiment, the Shelhigh valve that had been placed in the self-expanding Cook Z stent (Cook Inc, Bloomington, Ind) was placed with the help of a delivery gun into the right ventricular outflow tract of sheep. The approach was periventricular and no cardiopulmonary bypass was used. The clinical application of this approach followed this animal experiment.

I was astonished that none of our animal work with the Shelhigh valve or the periventricular approach was acknowledged by Berdat and coworkers in the case report.

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doi:10.1016/j.jtcvs.2006.06.038

Reply to the Editor:

We greatly welcome the valuable comments from our colleague Dr Amin and his important contribution to the development of this technique.¹ Due to space restrictions for published articles, we could not go into

details regarding the development and historical background of this new device. We therefore greatly appreciate that this information is still presented to the readership. We would like to emphasize, however, that our group was the first to clinically implant the Shelhigh injectable pulmonary valve, although Schreiber and colleagues² were the first to publish their experience.

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doi:10.1016/j.jtcvs.2006.08.008

Inflow occlusion in the era of modern cardiac surgery

To the Editor:

We read with interest the article titled "Inflow Occlusion Pulmonary Embolectomy in the Modern Era of Cardiac Surgery" by Bobadilla and associates.¹ The following points are not clear from the contents of the article.

First, how was the core temperature lowered? Was it lowered with a cooling blanket or just by dropping the temperature of the operating theater, and how long did it take for the temperature to drop to 32°C?

Second, for what duration was the inflow occluded?

Absence of structural or functional damage to the brain has been noted during circulatory arrest. Probability of safe circulatory arrest according to degree of hypothermia is shown in Table 1.

There are a number of situations wherein familiarity with inflow occlusion technique may be of use, such as pulmonary valvotomy, aortic valvotomy, and atrial septectomy. Patients who have pulmonary atresia with intact septum can undergo relief of

TABLE 1. Probability of safe circulatory arrest according to hyperthermia

Temperature (°C)	Duration of safe circulatory arrest (min)
37	3
28	10
18	45-60

Revised from Kouchoukos NT, Blackstone EH, Doty DB, Hanley FL, Karp RB. Hypothermia, circulatory arrest and cardiopulmonary bypass. In: Kirklin/Barratt-Boyes Cardiac Surgery, 3rd ed. Philadelphia: Churchill Livingstone; 2003. p. 80 (Table 2.9A). Reprinted with permission from Elsevier, Inc.

right ventricular outflow tract obstruction with a patch, pulmonary valvotomy, or a Brock procedure. Other procedures that have been performed safely by this technique are tricuspid valvectomy, emergency perforation of a ventricular septal patch, obtaining full thickness myocardial biopsy, and repair of penetrating wounds of the heart.²

Undoubtedly, this technique represents a considerable cost savings over the same procedures performed with cardiopulmonary bypass. In the current economic era of limited health care budget, any surgical technique that provides equivalent results is to be preferred over more expensive alternatives.

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Reply to the Editor:

We graciously thank the authors of this letter to the editor for supplying further