equalize the pressure over the entire surface of the clip.

We have applied this technique for 2 years now in 48 patients to correct P2 prolapse, bileaflet prolapse, and even Barlow disease.

In the case of endocarditis, we prefer to use resective techniques rather than the one described here (Figure 1-3).

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doi:10.1016/j.jtcvs.2007.01.032

Intraoperative ablation of atrial fibrillation using bipolar output of surgical radiofrequency generator (diathermy) and reusable bipolar forceps

To the Editor:

My colleagues and I began using bipolar radiofrequency (RF) ablation in August 1996 and published the first clinical study in the *European Journal of Cardiothoracic Surgery*.¹ We used the standard bayonet-shaped bipolar forceps with a 7-mm tip to draw ablation lines, which replaced most of the incisions of the Cox maze III procedure. The larger experience with this technique was published in *Heart, Lung and Circulation*.²

A number of bipolar RF ablation devices have been used experimentally and clinically. The results of these studies have been published in this *Journal*. One such study is that by Gaynor and associates.³ They tried to do exactly what we described in our publication in an indexed journal. Similarly, Gillinov and coworkers⁴ have used bipolar RF with different lesion sets. In addition, our letter to Editor⁵ clearly defines the advantage of bipolar RF over unipolar RF in avoiding collateral damage. In the abstract to the article by Benussi and associates,⁶ the "Objective" section states, "Bipolar radiofrequency proved highly effective in the animal model, but clinical experience is still initial."

I am at a loss to understand the reasons for the omission, in this Internet era, of reference to our original work published much earlier. I am absolutely sure that this omission was not intentional, yet I am led to believe it is a scientific lapse.

Therefore, I believe it would be appropriate to bring to the notice of the readers that intraoperative ablation of atrial fibrillation with bipolar RF is possible with a good success rate without additional costs of special RF equipment and disposable bipolar clamps. The technique is described in detail in reference 2. It is the purpose of this letter to highlight the fact that the standard RF generator with bipolar output can be used with the bayonet-shaped bipolar forceps to effect ablation lines on the atria and achieve a success similar to that of modified maze procedures. Our ongoing work, begun 10 years, ago emphasizes that our approach is highly successful, simple, and eminently cost-effective.

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doi:10.1016/j.jtcvs.2006.11.073

Reply to the Editor:

Dr Patwardhan complains that two seminal papers describing his pioneering experience in surgical ablation with bipolar coagulation^{1,2} were not given due consideration in articles published by us³ and by others.

We recognize that Patwardhan's intuitions opened the way to the modern scenario of surgical ablation. Nevertheless, only 2 of 74 articles among PubMed-listed clinical papers focusing on atrial fibrillation (AF) ablation with bipolar radiofrequency (RF) (by matching AF with bipolar RF) quote Patwardhan's works.

There are different possible reasons for this reiterated omission:

First of all, Patwardhan and colleagues reported using a standard electrocautery in the bipolar mode to ablate the atrial myocardium. This is not perceived by us (and probably by most surgeons) as being in accordance with some fundamental safety issues: Power-based RF can lead to charring and to excessive endocardial trauma, potentially leading to an increased risk of thromboembolism. Furthermore, also due to the absence of any feedback controlling energy delivery, electrocoagulation can cause excessive tissue weakening, possibly leading to bleeding. Re-exploration for bleeding occurred in 20% of survivors in Patwardhan's initial series.¹ Temperature and impedance feedback, featured by commercially available bipolar devices, are actually meant to address such problems.

It is then not irrelevant that bipolar electrocautery, despite sharing the same physical principles of commercially available RF ablation devices, is not CE- or Food and Drug Administration-marked for cardiac ablation. As a result, most surgeons, including our group, would rather avoid the "off-label" use of a bipolar electrocautery and indulge in the use of more recognized commercially available alternatives.

Furthermore, Patwardhan's spot-by-spot ablation without tissue feedback can admittedly lead to the creation of incomplete ablation lines, rendering the ablation procedure not reproducible and clinical result possibly suboptimal.

It is finally worth mentioning that Patwardhan's electrocautery ablation is an open-heart procedure. Electrocautery forceps, in fact, despite being bipolar, do not share the epicardial clamping feature that

makes modern bipolar devices more appealing and, to our advice, the real breakthrough in the arena of AF surgery. Such a characteristic is actually the greatest aspect of modern bipolar devices since, by clearing out the convective cooling of circulating blood on the target area, it allows an effective and reproducible ablation of a double layer of atrial wall epicardially, on the beating heart.

This notwithstanding, we respectfully acknowledge Patwardhan's contribution to innovation in this field. We express our empathy with his feelings at not being quoted in the context of bipolar RF. It is the same feeling we experience every time an author writes about epicardial ablation (be it bipolar or not) without mentioning our initiating clinical report⁴ or when we realize that most authors reporting on surgical ablation—including Patwardhan in his later experience²—describe administering amiodarone prophylactically, but nobody seems to be aware that such pharmacologic strategy was first proposed for AF surgery by our group.^{4,5}

We tend to blame the omission on a poor “conceptual copyright” protection policy on our side rather than suspecting the scientific accuracy of highly respected colleagues with whom we share the passion for finding new solutions to challenging clinical situations.

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Stefano Benussi reports consulting fees from Atlicure and Estech and lecture fees from St Jude Medical, Medtronic, and Cryocath.

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Immediate surgery in aortic dissection with cerebral malperfusion

To the Editor:

We congratulate Estrera and colleagues¹ and Urbanski and coworkers² on their impressive results with immediate aortic surgery for acute type A dissection with cerebral malperfusion. However, we would like to add specific observations, according to our experience with this complex issue.³

Estrera and associates¹ report a case series of 16 patients with acute aortic dissection and stroke, outlining a correlation between the time interval before aortic repair and outcome. Specifically, neurologic status never worsened, whereas recovery or improvement was observed in 80% of patients operated on within 10 hours. Surgery was denied in 1 patient with “neurologic devastation,” defined as coma with a Glasgow score of 5 or less. This definition appears somewhat arbitrary and refers to a single patient in this series, which is insufficient to draw specific recommendations. We previously reported on 5 comatose patients selected for immediate repair on the basis of hemodynamic stability, preserved pupillary reactivity, and coma duration of 12 hours or less, with encouraging results.³ Median Glasgow coma score was 5.5, whereas all patients showed an eye response score of 1. Similarly, all documented strokes in our series were right-sided, which likely indicates the same underlying pathophysiology, namely, malperfusion with dynamic flow patterns and prevalent compression of the innominate artery true lumen. In this respect, we pointed out the unsuitability of the Glasgow coma score, originally developed for the evaluation of neurologic trauma, to stratify these patients. Importantly, prompt aortic repair has been successfully reported with a score of 3,⁴ whereas neurologic recovery has been reported with extra-anatomic revascularization and delayed aortic repair more than 2 decades ago.⁵ Furthermore, the Rankin score also failed to correlate with outcome in this series, suggest-

ing that pathophysiologic mechanisms are different than those involved in acute occlusion causing traditional stroke.

Urbanski and colleagues² introduced the left common carotid as a systemic perfusion route for arterial return, with excellent results in 100 patients, including 27 with acute dissection, 4 of whom had cerebral malperfusion. In view of the prevalence of right-sided arch vessel malperfusion in the case of an acutely dissected aorta and cerebral injury, this approach seems particularly appealing in this context, especially if cannulation via a prosthetic vascular graft is established distant from the arch, thus avoiding dissected segments. Furthermore, this strategy reduces embolic hazards and greatly simplifies the implementation of selective antegrade brain perfusion. However, there is also a rationale for additional innominate artery perfusion after intraluminal arch inspection, which requires only an insignificant period of total circulatory arrest. In our opinion, we would hesitate to not provide selective antegrade perfusion to the most severely ischemic territory.

In summary, available reports indicate the importance of a nondelayed attitude for a satisfactory outcome in selected patients with acute type A aortic dissection and cerebral malperfusion. Current scores appear unsuitable to stratify the severity of neurologic injury in this specific context and should be viewed cautiously when the intent is to define irreversible “neurologic devastation,” particularly in stable patients with preserved brain stem reflexes.

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