

ment that the article breaches an important ethical standard regarding informed consent but that the information gained from this study addresses an important question, thus justifying its publication. Previous randomized trials [2, 3] evaluating the role of postoperative radiotherapy have suffered from a lack of adequate numbers. However, we take exception to the conclusions of the study on several counts. In a trial where the intervention is postoperative, we do not understand why the randomization was not performed after the operation. Postoperative randomization would have ensured that stratification into the 2 groups could have been performed while taking into consideration the pTNM staging. Failure to do so has resulted in a skewed distribution of patients, with significantly more lymph node-positive patients being randomized into the surgery plus radiotherapy arm. With lymph nodal positivity being a significant independent predictor of poor outcome, the effect of radiotherapy is expected to be diluted. This could have been easily prevented by stratifying patients on the basis of pTNM status after esophagectomy.

We also do not understand why 54 patients were excluded from the surgery plus radiotherapy arm because of low dosage of radiotherapy, poor health, leukopenia, and radiation reactions. We believe that patients should have been analyzed on an intention-to-treat basis (especially because most of the reasons for exclusion were related to the intervention itself) and included in the final statistical analysis. We wonder whether there still would be a significant difference between the 2 arms in patients with T3 tumors if these 54 patients were also included in the statistical analysis. Despite these methodologic and statistical flaws, the study still carries an important take-home message that postoperative radiotherapy may yield better locoregional control and, probably, overall survival in T3 cancers of the esophagus. The effect of postoperative radiotherapy in lymph node-positive patients probably did not reach statistical significance because of the relatively small numbers. We suggest that a prospective, multicenter trial of patients with T3 and N1 esophageal cancer, randomized after operation to receive or not to receive adjuvant radiotherapy, should be performed to resolve this issue. This trial should have a sample size adequate to detect a 10% improvement in survival, and randomization should be performed after operation and stratified on the basis of pTNM status.

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Efficacy of the “Box” Lesion Pattern in the Treatment of Atrial Fibrillation in the Presence of Mitral Valve Disease

To the Editor:

The article by Kalil and associates [1] reports their experience using surgical isolation of the pulmonary veins to treat chronic atrial fibrillation in the presence of mitral valve disease in a small cohort of patients. The authors described a simplification of the surgical technique used by Cox and colleagues [2] by creating a simple circumferential incision around the ostia of all the pulmonary veins and excluding the left atrial appendage. No additional lesions are made in the mitral annulus or the left atrial appendage.

Kalil and associates reported sinus rhythm in 14 of 15 patients at 6 months, results that compare favorably with those reported by Cox and co-authors [3] (98% of patients in normal sinus rhythm), and observed postoperative atrial flutter in only 1 patient. They concluded, that their technique is easy and does not require the technology or the expertise associated with many of the currently available ablation systems.

Although the technique described in this report represents a simplification of Cox's original maze operation, it has some limitations. The “cut-and-sew” technique can be cumbersome in many patients, does not eliminate the risk of suture line bleeding, and still prolongs cross-clamp and cardiopulmonary bypass times. For these reasons, the advantages of this technique, both in terms of clinical outcome and hospital costs, might be more apparent if the authors had compared their patients with a cohort undergoing an alternative approach. Another important limitation is the fact that although a cut-and-sew approach is clearly less expensive than a lesion set created with one of a number of available ablative energy sources, it is not feasible in the off-pump beating heart setting (a requirement for any technique that is to be part of a minimally invasive operation for atrial fibrillation).

Despite these limitations, this study demonstrates that the pulmonary vein-encircling (“box”) lesion alone can be effective in restoring sinus rhythm in the presence of structural heart disease; conversely, it suggests that the mitral annulus-“connecting lesion” may not be absolutely necessary. To the extent that the more promising of the minimally invasive approaches currently under development combine the convenience of ablative devices with the simplicity of a “box” lesion pattern, this study makes an important contribution.

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Reply

To the Editor:

We acknowledge the expert comments of Bisleri and colleagues regarding our technique for pulmonary vein isolation. We now have operated on 38 patients in chronic atrial fibrillation (AF) secondary to mitral valve disease, and the results remain similar to those in the report [1]. This has become our procedure of choice for chronic AF. The authors are correct in stating that this procedure can be difficult in some patients, especially when cutting the left part of the atrium. Some sort of ablation in that area, if possible, could help overcome the problem, but one must bear in mind that a complete transmural lesion is needed and that this can be guaranteed only by the cut-and-sew technique. Suture line bleeding is always a risk, but it has not been greater in our series than with mitral valve operation alone. Cross-clamp time is increased by 20 to 30 minutes and should decrease as we gain experience.

We have not compared this technique with others except our previous experience with the Cox maze operation and mitral

valve operation alone. A prospective, randomized series comparing these three operations was conducted recently (unpublished observations).

We agree completely that this small series demonstrates that chronic AF can be effectively treated with simple electrical exclusion of all pulmonary veins from the rest of the heart. This fact may facilitate the development of minimally invasive or videothoracoscopic procedures that combine the convenience of ablative devices and the simplicity of this lesion pattern for pulmonary vein isolation.

We thank our colleagues for their interest in surgical procedures for AF.

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Reference

- Kalil RAK, Lima GG, Leiria TLL, et al. Simple surgical isolation of pulmonary veins for treating secondary atrial fibrillation in mitral valve disease. *Ann Thorac Surg* 2002;73:1169–73.

CORRECTION

Rangappa S, Fen C, Lee EH, Bongso A, Wei ESK. Transformation of adult mesenchymal stem cells isolated from the fatty tissue into cardiomyocytes. *Ann Thorac Surg* 2003;75:775–9.

In the above-referenced article, which appeared in the March 2003 issue of *The Annals*, the last author's name should read: Eugene Kwang Wei Sim, instead of Eugene Sim Kwang Wei. *The Annals* regrets this error.