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pacemaker implantation (PPM). Jouan and colleagues² are clear when defending prophylactic tricuspid annuloplasty to counteract late severe functional tricuspid regurgitation (TR) and an extended observation period after postoperative conduction abnormalities aimed at decreasing the incidence of PPM. Although in his reply published in the *Journal*,³ Jouan agrees on our remarks on the importance of informing patients on the eventual risk of PPM after prophylactic tricuspid annuloplasty at the time of mitral intervention, he is also strong on producing information of the risk of developing functional TR that carries mortality over time.

The contribution of Jouan and colleagues² and our comments¹ are instrumental in highlighting what is known today. First, tricuspid valve surgery is a challenge (Figure 1). It is likely that our understanding of the disease and its different forms is still suboptimal, considering that accumulated information is based on retrospective studies covering long periods of time⁴ or observational studies with large differences in sample sizes and variability in inclusion criteria.² Second, we have restricted room to expand our knowledge, because methodologically sound studies are not easy to design and execute when one considers logistics and finance among other issues.

Prophylaxis in tricuspid surgery is meant to be a matter of discussion. We may need a solid and unquestionable definition in this setting. The data from Jouan and colleagues² come from an observational study with still-limited follow-up of less than 60 months. Although their results are more than remarkable, there may be need for

additional input in the future. There is more recent controversy, as data from Shinn and colleagues⁵ from the Mayo Clinic support no influence of the method of annuloplasty used on recurrence of TR over time in their 15-year experience, therefore challenging a number of studies.⁴ In this case, however, a number of methodology issues have been addressed in a related commentary by Antunes.⁶ This is not uncommon in contributions dealing with the tricuspid valve because of all of the aforementioned reasons. Lack of standardization is still significant, and many answers to our question may come only after studies that we know are difficult to implement. Consistently neglecting the tricuspid valve is still significant, also.⁷

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<http://dx.doi.org/10.1016/j.jtcvs.2016.08.049>

FIGURE 1. Tricuspid valve suture annuloplasty.

DOES N2 FROZEN SECTION MAKE SENSE IN cN0 NON-SMALL CELL LUNG CANCER PATIENTS?

To the Editor:

We have carefully read the article by Yang and colleagues¹ about the outcomes after lobectomy when unexpected pN2 are found. Quite surprising, they concluded that survival was comparable with cN2 patients who underwent induction therapy and subsequent lobectomy. In particular, the survival of surprise N2 patients was similar to that of postinduction cN2 patients if adjuvant

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chemotherapy, with or without radiation therapy, could be administered after surgery. If adjuvant therapy was not provided, outcomes were significantly worse for the resected, surprise N2 patients. The good news suggested by the work of Yang and colleagues is that surprise N2 patients might be helped by surgery even if induction therapy has been “accidentally” dismissed; however, many biases are present in the study.

In our opinion, the most important one is that the authors were not able to distinguish patients with suspected N2 who underwent induction therapy with successful downstaging from those who continued to have N2 disease. Despite the claim by some that the prognosis of cN2 induction/surgery patients is not influenced by N2 downstaging if complete nodal dissection can be achieved,² there is strong evidence that surgery is not successful equally in those with persistent versus nonpersistent N2 disease.

Moreover, we have concerns about the authors’ suggested role of intraoperative nodal frozen section before lobectomy. They suggested that N2 frozen section is useless because its result does not change the prognosis if lobectomy and following adjuvant therapy can be performed; however, because they were not able to distinguish between single or multiple station c/pN2, they also added that N2 frozen section could be performed when multiple stations N2 disease is suspected. In fact, the role of surgery is controversial when N2 involvement is widespread.

Our first question is thus: How can a surgeon intraoperatively decide that a patient, who may be fit for a lobectomy, is or is not eligible for postresection adjuvant therapy? The second question is: When should surgeons intraoperatively suspect multiple N2 disease and thus do a complete lymphadenectomy with an immediate frozen section? If preoperative computed tomography, positron emission tomography scan, and endobronchial ultrasonography biopsy already have been performed adequately to exclude N2 disease, what would prompt a frozen section on a lymphadenectomy specimen?

In our opinion, it currently is possible to obtain an accurate preoperative nodal staging that is so reliable that the occasion of macroscopic disease at surgery is very unlikely. Therefore, intraoperative preresection nodal frozen section is probably useful only in few rare cases in which suspected multiple N2 disease could not be proved by endobronchial ultrasonography.

Despite these doubts, the paper is interesting, especially because this work appears to strengthen the role of surgery

and adjuvant therapy in case of locally advanced non–small cell lung cancer.

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**FROZEN SECTION
OF N2 NODES IS
INVALUABLE
WHENEVER
UNEXPECTED**

**SUSPICIOUS OPERATIVE FINDINGS ARE
ENCOUNTERED**

Reply to the Editor:

We appreciate the insight on our study expressed by Baisi and colleagues.¹ Their comments are a reflection of the complexity of how to optimally manage patients with non–small cell lung cancer and N2 disease. We disagree with their statement that we suggest that intraoperative frozen section is useless. On the contrary, we feel strongly that intraoperative frozen section is invaluable in many cases of non–small cell lung cancer and in fact is one of the tools that increases the utility of surgery over other modalities. In our study, we only commented that routine frozen section of N2 nodes during lobectomy is not necessary in all patients who do not have clinically suspicious nodes presurgery or do not have grossly abnormal nodes found during surgery.

With regard to their question of how a surgeon can evaluate whether a patient could tolerate adjuvant therapy, we feel this is simply an extension and re-evaluation of all the clinical patient characteristics (eg, age, pulmonary function, comorbidities, activity and functional levels, frailty, etc) that the surgeon must have evaluated extensively during assessment of whether the patient was an appropriate surgical candidate in the first place. Baisi and colleagues also had a question regarding when surgeons should intraoperatively suspect multiple N2 disease and perform a complete