Authors have nothing to disclose with regard to commercial support.

cautiously, these data characterize comparable survival with SBRT and surgery in medically operable populations. To our knowledge, the only randomized data comparing SBRT and surgery come from the STARS/ROSEL pooled analysis.¹³ This study found no differences in local, regional, or distant recurrences, but the 3-year OS was superior with SBRT (95% vs 79% with lobectomy, $P = .037$). However, additional prospective studies are clearly needed.

While further randomized data are awaited, thoracic oncology physicians invested in this discussion may find comfort in the fact that they are in good company. Breast oncology was once a heated battleground for debates over mastectomy versus breast conservation, until randomized trials established the viability of both approaches.¹⁶ Likewise, urologic oncology witnessed decades of contention and untold numbers of retrospective analyses championing OS advantages with prostatectomy over radiotherapy, until randomized data showed their equivalence.¹⁷ Thoracic surgeons, like urologists and breast surgeons, are right to aspire to provide the highest quality treatment modality for their patients. Yet, at the same time, patients with cancer deserve careful interpretation of retrospective analyses influenced by selection and indication biases, as well as collegial multidisciplinary discourse promoting the completion of prospective trials. Characterizations of effective, well-tolerated treatment modalities as a “slippery slope”¹⁸ may betray a lack of perspective on historical progress in cancer management across disease sites.

Chad G. Rusthoven, MD

Bernard L. Jones, PhD

Brian D. Kavanagh, MD

Department of Radiation Oncology

University of Colorado School of Medicine

Aurora, Colo

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WHEN IN DOUBT, ABSTAIN

Reply to the Editor:

The contribution to the *Journal* by Rusthoven and colleagues¹ on stereotactic body radiation therapy (SBRT) focuses on several controversial issues, which are analyzed with the magnifying lens of the radiation oncologist. In fact, the conclusions are, at best, unsupported by current evidence, although they do pave the way to the design of a much-awaited randomized clinical trial of SBRT versus surgery in patients with early-stage lung cancer.

The first controversial issue is the concept of operability. The threshold for operability should be defined according to

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current national and international guidelines, thus avoiding individual interpretations not in line with the available evidence in terms of risk assessment and leading to discordant clinical approaches.² As an example, in a highly quoted article,³ some vaguely defined criteria of “pulmonary and cardiac insufficiency” appear among the indications for SBRT without providing cut-off values for ruling out surgery. In their classic study, Guckenberger and colleagues⁴ described the selection processes for SBRT in 5 international institutions, which were characterized by a large variability in the pulmonary function test cut-offs.⁴ In that article,⁴ median forced expiratory volume for 1 second, forced expiratory volume for 1 second expressed as a percentage of the forced vital capacity, diffusing capacity of the lung for carbon monoxide, and percentage of predicted diffusing capacity of the lung for carbon monoxide were 1.4 L, 63%, 10.9 mL/min/mm Hg, and 52%, respectively⁴—values that are deemed acceptable for operative candidates for pulmonary resection in a current thoracic surgical practice.⁵ In addition, when outcomes from patients with lung cancer who had marginal pulmonary function entered in the Radiation Therapy Oncology Group 0236 and American College of Surgeons Oncology Group Z4032 trials were analyzed with propensity matching, no difference in major morbidity between the surgical and the SBRT arms was observed, even though almost 30% of the clinical stage I disease proved more advanced at final pathologic analysis.⁶ In this setting, a new ground for discussion will be provided by the recently published American Society for Radiation Oncology, guidelines that emphasize how thoracic surgeons should decide on operability in a view to offer a pulmonary resection associated with a mortality lower than 1.5%.⁷

The observation brought forward by Rusthoven and colleagues¹ that a selection bias could partially justify the results in the recent study by Yerokun and associates⁸ may indeed be well grounded. Radiation oncologists and thoracic surgeons must devise a randomized trial, however, and this trial needs to be based on the evaluation of comparable pathologic TNM stages and 5-year overall and recurrence-free survivals.⁹ In fact, if we were to compare the overall 5-year survivals of patients medically fit for operation subjected to sublobar resection for stage IA lung cancer in Japan,¹⁰ the gap between the 96% figure recorded in the surgical study and the 65% observed in the SBRT study performed in Japan by Nagata and colleagues¹¹ could be considered too wide to support

SBRT lightheartedly as an alternative to surgery. In this setting, the reported 5-year overall survival for cT1 disease in the American College of Surgeons Oncology Group Z0030 study was 72%,¹² a value that compares favorably with the 3-year overall survival of 77% reported in both the JCOG 0403 trial and the Radiation Therapy Oncology Group 0618 trial.¹

In the ideal randomized, controlled trial, the outcome analysis should focus on SBRT dosage adaptation to the tumor localization as well as sublobar resections, because they could represent two sides of the same coin: the need for modulation of the ablative treatment as a response to the patient-related compromised condition opposing the desired extent of resection.¹³ In addition, an agreement on the concept of posttreatment local failure should be sought, thereby avoiding the inconsistencies that make any meaningful comparison vain.¹³ In this setting, I do not agree with Rusthoven and colleagues,¹ who found it useful to compare data from a randomized, controlled trial such as the American College of Surgeons Oncology Group Z4032 study with a retrospective study that was based on 65% of SBRT-treated patients without histologic confirmation and on local, regional, and locoregional modalities of recurrence.³ In addition, although a recurrence in the residual lung or at the staple line can be easily detected, a local recurrence after SBRT may be difficult to distinguish clearly from SBRT-related inflammatory changes.¹⁴

A final issue is the evaluation of the results of previously conducted (and failed) SBRT randomized, controlled trials and related theoretic byproducts.¹⁵ Beyond any partisan interpretation, these trials have not contributed to generating the body of evidence necessary to claim superiority, or even equipoise, for SBRT relative to surgery for patients with early-stage lung cancer who are operative candidates.^{16,17} From now on, while structuring a randomized trial that is based on the firm and convinced collaboration between surgeons and radiation oncologists, any future contributions from either side that fails to draw a line and pave the way to building a reliable and undisputed bulk of evidence should not be considered for publication. Under the current circumstances, we should behave as the old motto goes: “When in doubt, abstain.”

Gaetano Rocco, MD, FRCSEd
Division of Thoracic Surgery
Thoracic Department
Istituto Nazionale Tumori
IRCCS Fondazione G. Pascale
Naples, Italy

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