Response

To the Editor:

We thank Dr Senthil and colleagues for their interest in our CHEST article.1 We agree that the work by Haasbeek et al2 should have been cited in our article. Although the median follow-up of 16.5 months is rather short, the results are impressive with no local failures and only two of 15 ≤ grade 3 lung-associated toxicities. We agree that stereotactic radiotherapy (SRT) is a less invasive procedure in comparison with cryoablation, particularly if it is done without the implantation of fiducial markers.

In terms of safety, pneumothorax after cryoablation can be sufficiently managed by course observation, by chest tube insertion, or, if persistent, by medical pleurodesis. However, in single-lung patients, delayed pneumothorax may have serious consequences if access to medical facilities is limited. We have previously reported that the incidence of delayed pneumothorax after cryoablation occurred in 30 of 193 patients (16%), mostly at 3 days after cryoablation still during admission. However, the incidence could occur as late as 20 days after cryoablation, and eight of these patients were readmitted after being discharged. Therefore, if the incidence of pulmonary toxicities after SRT is actually as low as generally reported (≤ grade 2, 5%~10%), we agree that SRT should be offered as a primary treatment option even in single-lung patients.

However, one concern remains. To date, SRT for lung cancer has been evaluated mostly in inoperable patients, many with COPD. A recent report suggests that radiation pneumonitis may be milder in patients with COPD in comparison to patients with normal lung function.3 Another study shows that the decline in pulmonary function after SRT was less in patients with COPD in comparison to patients who underwent SRT primarily because of cardiac comorbidities.4 In a study of SRT in medically operable patients with lung cancer, grade 3 pulmonary toxicity was noted in only one patient (1.1%), but this was a retrospective study, and 36 of 87 patients in this study had underlying chronic lung diseases.5 The majority of patients in the study by Haasbeek et al were also patients with COPD. Therefore, we consider that the incidence of pulmonary toxicities after SRT in patients with normal lung function is still unclear. Ongoing prospective studies on SRT in patients with operable lung cancer with normal lung function will provide answers to this issue.

Yotaro Izumi, MD
Yoshikane Yamauchi, MD
Osamu Kawaguchi, MD
Hiroaki Nomori, MD
Tokyo, Japan

Natriuretic Peptides and Mortality in Community-Acquired Pneumonia

To the Editor:

We read with great interest the study by Nowak et al1 in a recent issue of CHEST (April 2012) on natriuretic peptides in patients with community-acquired pneumonia. The findings extend two earlier reports on the prognostic significance of these peptides by the same group.2 3 It would be helpful to know to what extent these cohorts overlapped. In particular, the two most recent studies recruited patients from community-acquired pneumonia admitted to the same hospital between 2003 and 2005. The most recent publication1 extended the recruitment period to include April 2006 to March 2007, but it is not clear whether some patients were in both cohorts or how many new patients were added in the extra recruitment time for the recent study.

If there is overlap between these two cohorts, we would also be grateful if the authors could clarify two further points. First, the initial cohort was part of a randomized controlled trial where patients were managed empirically or according to serial procalcitonin levels,4 whereas the current report states that all patients were managed according to standard guidelines. Secondly, Nowak et al5 state that the treating clinicians were blinded to natriuretic peptide levels, whereas the earlier report stated that B-type natriuretic peptide levels were available to the treating clinicians and may, therefore, have influenced treatment. We would be grateful if the authors could clarify these issues because we believe that it would help us to interpret the findings.

Catherine L. Chang, MD
Robert J. Hancox, MD
Hamilton, New Zealand

REFERENCES

REFERENCES


Response

To the Editor:

We thank Drs Chang and Hancox for their interest in our work on natriuretic peptides in community-acquired pneumonia.1,4 The current report extends our previous observations and tries to highlight findings that (1) apply to the whole family of natriuretic peptides in general and (2) provide a direct comparison of the clinical potential of all three commercially available natriuretic peptides (B-type natriuretic peptide [BNP], N-terminal pro-B-type natriuretic peptide, and midregional pro-atrial natriuretic peptide). In this analysis, we used the data from all patients who presented to our institution with community-acquired pneumonia in whom we had the measurements of all three natriuretic peptides available. As correctly mentioned by Drs Chang and Hancox, some of these patients were in earlier cohorts that dealt exclusively with BNP. About one-third of the patients were included in the recruitment period from April 2006 to March 2007.

Drs Chang and Hancox are also correct in highlighting that in some patients, physicians used procalcitonin levels as additional information for the tailoring of the duration of antibiotic treatment. Regarding blinding, physicians were blinded to N-terminal pro-B-type natriuretic peptide and midregional pro-atrial natriuretic peptide levels in all patients. These levels were measured from frozen samples long after the discharge of patients. The blinding regarding BNP levels was not uniform. In about one-half of the patients, BNP levels were measured in a blinded fashion; in the other half, BNP levels would have been available to physicians via the electronic patient record.

Christian Müller, MD
Basel, Switzerland
Albina Nowak, MD
Zurich, Switzerland

Affiliations: From the Department of Internal Medicine (Dr Müller), University Hospital Basel; and the University Hospital Zurich (Dr Nowak).

Financial/nonfinancial disclosures: The authors have reported to CHEST the following conflicts of interest: Dr Müller has received research support and speakers’ honoraria from Abbott Laboratories, Alere, BRAHMS, Nanosphere Inc, Hoffman-La Roche Inc, and Siemens AG. Dr Nowak has reported that no potential conflicts of interest exist with any companies/organizations whose products or services may be discussed in this article.

Correspondence to: Christian Müller, MD, University Hospital Basel, Department of Internal Medicine, Petersgraben 4, Basel, 4031, Switzerland; e-mail: chmueller@ubhs.ch

Preventing VTE in Outpatients With Cancer

To the Editor:

We applaud the innovations pioneered by the American College of Chest Physicians evidence-based clinical practice guidelines on antithrombotic therapy and prevention of thrombosis (February 2012). However, we are concerned about the resulting recommendations regarding cancer-associated thrombosis, a significant contributor to the public health burden of VTE and an area of particular interest to us.

Regarding prevention of VTE in nonsurgical patients,1 the panel suggested that outpatients with solid tumors with additional risk factors for VTE should receive prophylactic-dose low-molecular-weight heparin or low-dose unfractionated heparin (recommendation 4.2.2). Additional risk factors cited by the panel include hormonal therapy and angiogenesis inhibitors. In our opinion, this recommendation (even as a grade 2B) does not reflect an appropriate interpretation of the results of recent studies on cancer thromboprophylaxis and risk assessment. If these recommendations were to be followed, tens of thousands of women with breast cancer or men with prostate cancer on hormonal therapy for extended periods would receive low-molecular-weight heparin or low-dose unfractionated heparin. The rate of VTE in these patients, however, is much lower than other cancer subgroups, and there are no studies showing a benefit of thromboprophylaxis.2 Similarly, the linkage between antiangiogenic agents, such as bevacizumab, and VTE has not been consistently demonstrated in pooled analyses.3 (Although thalidomide- and lenalidomide-based regimens are associated with VTE, these are used primarily in myeloma and not solid tumors.) Recommendation 4.2.2 further fails to cite important risk factors, such as site of cancer, and a risk assessment model for chemotherapy-associated VTE that has been externally validated in multiple studies; both are the basis for recent and ongoing prophylaxis studies.4

The Institute of Medicine’s 2011 report, *Clinical Practice Guidelines We Can Trust,* emphasized guideline development group composition and external review.5 Inclusion of oncology content experts on the panel or as external reviewers could have provided a different interpretation of the data and more...