significant differences in smoking cessation. To show a significant difference of 5% between groups of smokers with AL and NLF with $p = 0.05$ and $\beta = 0.2$, the number of smokers in each group should be 603.

We suggested in the “Discussion” section that the fact that more smokers with moderate/severe disease as compared to those with mild disease quit smoking might result from more symptoms observed in more advanced disease. We have now looked into our database for more details: 66% of smokers with AL vs 68% of smokers with NLF produced sputum (not significant), and 71% vs 60%, respectively, complained of cough ($p < 0.05$). There was also an increasing trend in the phlegm production in more severe disease, but we have not found significant differences in symptoms after stratifying the patients with AL according to disease severity.

Our results (10.1% of patients with COPD and 8.4% of those with NLF who remained nonsmoking after 12 months) compare favorably with the results of the Italian study (6.5% of smokers offered spirometric testing and counseling quit as compared to 4.5% of control subjects offered minimal intervention), and the Norwegian study in male subjects with low lung function resulting in a 5.6% quit rate at 12 months after sending a personalized letter explaining the results of spirometric testing with advice to stop smoking, as compared to 3.5% in control subjects ($p < 0.01$) who were not informed about their lung function.

We have also found, after additional random telephone screening of smokers who did not attend the follow-up visit, that only smokers with airflow limitation quit smoking (an additional four patients who stopped smoking). Also, smokers with the diagnosis of AL were more successful in reducing the number of cigarettes smoked (five fewer cigarettes per day, $p < 0.05$), as compared to smokers without AL (two fewer cigarettes per day, not significant). We believe therefore that the diagnosis of AL motivated smokers to try to quit.

Every effort should be made to make people stop smoking. This is especially true for smokers at risk and with early diagnosis of COPD. This issue is now being discussed, in trying to assess the role of spirometric testing in motivating smokers to quit.4–6

We agree with Dr. Kaminsky and Dr. Marcy that larger studies are needed, including a control group of smokers given stop-smoking advice without spirometry, to assess the cost-effectiveness of spirometry as part of a smoking cessation program. But as Krahn and Chapman have quoted, “even modest quit rates attributable to screening spirometry may result in highly favorable cost effectiveness ratios.”

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**References**


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**Early Mobilization in Pneumonia**

To the Editor:

Mundy et al (September 2003) performed a very well-designed, group randomized, controlled trial investigating the benefits of early mobilization in patients admitted to the hospital with community acquired-pneumonia. They reported that early mobilization resulted in improved outcomes, specifically a 1.1-day decrease in hospital length of stay with a concomitant savings of approximately $1,000 per patient in the intervention group. I want to believe the improvement was due to the intervention, as it would provide a simple but powerful tool to improve my patient’s outcomes. But, alas, I don’t believe it.

If one examines Table 2 of this study carefully, one sees that 61% of the control (usual care) group received early mobilization and 73% of the intervention group received early mobilization, for an absolute difference of only 12% or approximately one eighth of the patient population. This means that if the intervention alone was responsible for the outcome differences noted among the entire patient population, then the magnitude of improvement due to the intervention would have to be eight times the improvement noted among the population as a whole. In other words, we would have to believe that early mobilization resulted in an 8.8-day decline in the length of stay and a cost savings of $8,800.

The above scenario is unlikely for two reasons. It is not biologically plausible that mobilization could decrease the length of stay more than the average length of stay for community-acquired pneumonia. Furthermore, if the results were this compelling, I would have expected them to have been presented.

What then could explain the results that were noted, if they were not due to the intervention? Several were appropriately noted by the authors and in the accompanying editorial. One possible explanation not noted is that the intervention group received their initial antibiotics a mean of 1.2 h quicker than the usual care group. Although not statistically significant, this may have been clinically significant, as a shorter time to initial antibiotics has been associated with improved patient outcomes.

In conclusion, I believe that early mobilization is likely to benefit some patients with pneumonia; however, I do not believe that this study can be used as evidence of that benefit. Perhaps, if further analysis of the data from this study demonstrated that patients who actually received the intervention did better than those who did not, we would have stronger evidence in support of early mobilization.

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To the Editor:

We appreciate Dr. Metersky’s interest in our article. While studies such as this one can never define causality, we have identified a striking association between early mobilization and length of hospital stay.

As noted in our article and the accompanying editorial by Dr. Wunderink, the association between early mobilization and length of hospital stay for patients hospitalized with community-acquired pneumonia (CAP) may be attributable to several factors. It is worth noting that Dr. Metersky’s suggestion that the timing of antibiotics reflected an explanation is a misread of the article. The intervention group received their initial antibiotics a mean of 1.2 h later, not sooner, than the usual care group. Thus, the effect of early mobilization may be even stronger if evaluated in future studies. In prior work by Meehan et al., initiation of antibiotics within 8 h of arrival was associated with lower CAP mortality in elderly patients; no associations were made for timing of antibiotics with length of hospital stay.

The absolute difference in early mobilization for the two groups was 12% (73% vs 61% for the intervention and usual care groups, respectively). The additional calculations made by Dr. Metersky appear to assume a linear relationship for levels of pneumonia severity of illness and compliance with the intervention.

To the best of our knowledge, there is no evidence to support this assumption. Future studies may be able to assess this as a hypothesis, along with other intriguing questions such as a potential dose effect of mobilization. Overall, we appreciate the spotlight on this provocative study and, as in the article, re-emphasize that we do not recommend that this provides sufficient evidence for early mobilization to become a standard of CAP care.

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REFERENCES

Diagnosing Neuroleptic Malignant Syndrome

To the Editor:

I read with great interest the article by Tsai et al (October 2003) about a 68-year-old man with depression who presented with fever, mental status changes, and rigidity that was subsequently diagnosed as neuroleptic malignant syndrome (NMS). The authors suggest that the coincidental increase in the dose of venlafaxine, a dual serotonin and noradrenergic reuptake inhibitor, may have induced the NMS. One important distinction that was not mentioned by the authors is the possibility of acute serotonin syndrome (SS).

SS results from the overstimulation of 5-HT1A receptors by selective serotonin reuptake inhibitors, tricyclic antidepressants, monoamine oxidase inhibitors, or other serotonergic agents. Clinically, NMS and SS share many features, suggesting different spectrums of a similar disorder. Both syndromes may present with varying degrees of fever, altered mental status, and neuromuscular abnormalities, including leukocytosis, elevated creatinine kinase levels, transaminis, and low serum bicarbonate levels. Distinctions between the two diagnoses are often difficult to make, having large clinical overlap. However, some authors have suggested that patients with NMS demonstrate higher fevers and more pronounced extrapyramidal effects, while SS patients have lower fevers, myoclonus, and GI dysfunction. However, some authors have suggested that patients with NMS demonstrate higher fevers and more pronounced extrapyramidal effects, while SS patients have lower fevers, myoclonus, and GI dysfunction. SS secondary to venlafaxine therapy has been well-described in the medical literature. Clearly, the inclusion of SS in the differential diagnosis of this patient is warranted and may suggest an alternate diagnosis. Fortunately, the treatment for both NMS and SS consists of removing the offending agent and providing supportive care. As stated by the authors, there may be a role for both dantrolene and bromocriptine in the treatment of these conditions.

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