Association Between Baseline Airflow Obstruction and Rate of Decline in Lung Function

To the Editor:

We read with interest the study by Mohamed Hoesein et al1 (see page 1530), in which they nicely proved that rate of lung function decline is maximum in male heavy smokers with mild or no airflow limitation as documented on spirometry. However, the criteria for categorizing all enrolled patients into COPD have not been specified. In view of the lack of spirometric evidence of airway obstruction and reversibility, it is plausible that some of the subjects may actually be suffering from other airway diseases, notably asthma, which usually do not result in progressive lung function decline. Segregation of such patients by serially testing spirometry any time during the course of 3 years could have increased the specificity of the study. Moreover, COPD is also known to occur in nonsmokers, predisposed by other risk factors like air pollution,2 genetic predisposition,3 and so forth. Inclusion of nonsmokers might have given a better picture of the association between rate of decline in lung function and stage of COPD.

Acute exacerbations of COPD also have a deleterious effect on lung functions, irrespective of stage. These, along with the COPD treatment, have been shown to alter the rate of decline in expiratory flow rates.45 It would be useful for readers if the authors could shed light on the clinical course of subjects, such as exacerbation frequency and treatment given during the study period. Both of these confounding factors, if not considered, might have affected the results.

COPD is a progressive disease with significant morbidity and mortality. Pending discussion, the results of this study carry huge clinical implications, emphasizing the need to devise new ways to efficiently diagnose and target patients at risk for and with early COPD.

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REFERENCES


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Response

To the Editor:

We thank Drs Aggarwal and colleagues for their valuable questions about and remarks on our article in CHEST.1 To start with, we would like to emphasize that the included subjects were heavy smokers drawn from the general population: Non-smoking-related diseases would show a study prevalence equal to that in the population. Asthma would, therefore, be present in the same (low) population percentage in all the groups we formed. As mentioned in the “Materials and Methods” section, the three groups were based on the baseline prebronchodilator FEV1/FVC: either >70%, ≤70% and greater than the lower limit of normal, or less than or equal to the lower limit of normal. Because asthma was neither an inclusion nor an exclusion criterion, asthmatic subjects were fully randomized over the three groups formed. The effect on lung function decline was therefore negligible.

The aim of the current study was to relate baseline FEV1/FVC to lung function decline in heavy smokers. We agree that COPD is also known to occur in nonsmokers; nonetheless, the majority of COPD in the Western World is caused by tobacco smoke and only a small minority by other causes. The subjects in this study originated from the concise Utrecht and Groningen areas in The Netherlands, and the air pollution burdens for the included subjects are very comparable. No large contrasts in terms of heavily

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Shifting the Focus of ICU Staffing Research to the Community

To the Editor:

In an issue of CHEST (May 2012), I read with great interest the article on 24-h intensivist staffing in teaching hospitals by Kerlin and Halperni because it was published at the same time as a large retrospective study that did not reveal a mortality benefit with night intensivist staffing at high-intensity ICUs. Kerlin and Halperni make strong arguments in support of prospective randomized trials on the topic of ICU staffing. As well, I recently questioned the need for 24-h intensivist coverage on the basis of my anecdotal experience in the community setting.3 In the article, the authors focused on the risks and benefits of 24-h attending staff as these relate to medical education and the care of patients in an academic setting. They also briefly discussed the potential for the unintended consequences of decreased staffing and compromising care at regional hospitals, widening existing chasms of care. I emphasize that this is not a theoretical problem—it is real and ever growing in the community, where the shortage of trained intensivists is already painfully felt. For this reason, this topic should be examined in earnest, as advocated by the authors, and the focus should shift to the community setting. I believe that there is a pressing need to explore the value of night intensivists to community ICUs, more so than studying that of a night ICU attending physician working with trainees vs trainees alone to academic ICUs.

Most community hospitals in my metropolitan region have shifted from a 12- or 16-h workday to 24-h coverage. As Kerlin and Halperni mentioned, the benefits of this move seemingly have strong face validity and the support of observational studies. However, a significant consequence of this shift is that a small regional pool of community intensivists is now spread thin to cover nights. This, in turn, has led to lean and fractured day coverage and exacerbated a provider tug-of-war between facilities where full-time equivalent (FTE) and moonlighting physicians recruited to fill voids created by night staffing at one facility leave voids at another. For example, we recently recruited an FTE physician from one facility, leaving that facility, which had recently transitioned to 24-h coverage, more short staffed. A nearby hospital just transitioned to 24-h staffing and recruited one of our moonlighter physicians, necessitating our FTE physicians to work more nights. These voids are challenging to fill given the ongoing shortage of trained intensivists and may offset the purported benefits of 24-h coverage.

Nonacademic ICUs account for more than one-half of the ICUs in the United States. Despite this, most ICU staffing research to date has involved academic centers, leaving a paucity of evidence to guide community ICU staffing. Research on staffing in academia has focused on the question of whether 24-h attending physician coverage is necessary. This has little bearing on community ICUs because these typically are staffed with one intensivist, not a team of trainees led by an attending physician. The main question for community ICUs is whether the presence of a night intensivist is at all necessary if there is high-intensity staffing during the day. Alternatives to 24-h intensivist staffing include night intensivist phone coverage, remote video monitoring, physician extender coverage, expanded hospitalist coverage, or any combination of these. These alternatives need to be explored rigorously because the demand for trained intensivists continues to outstrip supply. If viable, these alternatives may help to alleviate the increasing staffing strain in the community.

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