Underutilization of Spirometry for the Diagnosis of COPD

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Regarding the comments on safety, since their article[2] did not include a discussion on safety, obviously our comments are of a general nature, as was stated up front in our editorial. Our concerns emanate from long experience in the field of pediatric bronchoscopy and our sense that we do not have the tools to define what "important complications" are. We also lack the benchmarks to assess their severity or a platform to easily report them. The single large study[5] that analyzed complications in bronchoscopy reported "At least one complication" at 6.9%. At Children's Hospital Boston, as part of a quality control project, we monitored complications immediately at the end of each bronchoscopy and found that we had an overall complication rate of 6%. While the design was prospective, we belatedly recognized that we failed to include contact with the families following the procedure and thus may have missed late complications such as hemoptysis and fever. The data were used for internal control and have not yet been submitted for publication. The recognition that complications are underappreciated is the likely reason that the government of the Netherlands is now mandating that hospitals develop mechanisms and benchmarks to report complications of their various procedures.

In a previous review on bronchoscopy,[6] one of the authors of the article and letter wrote: "As it is an invasive procedure, the following should be asked: what question am I trying to answer by bronchoscopy? Will the answer justify the risks of the procedure?" We are in full agreement with this approach and would only comment that when an invasive element to an already invasive procedure is added, one should bear in mind the adage *primum non nocere*. We are advocating caution when adding a further invasive procedure to our routine bronchoscopy/BAL unless it carries benefits that remain to be defined or serves legitimate research purposes.

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To the Editor:

The interesting article in *CHEST* (August 2007) by Han et al[1] and the relative editorial comment[2] stressed the problem of COPD diagnosis, which by definition must be confirmed by spirometry, but which in clinical practice is based only on clinical grounds in a large proportion of cases. The negative consequences, such as the overadministration of therapies, with the chance of doing more harm than good in terms of costs and number of adverse events, have been correctly underlined. The authors reported no difference between the primary care and the specialist setting, while other published articles have reported that specialists are more likely to use spirometry.

The Clinical Effectiveness Unit of the Public Local Health Authority of the province of Reggio Emilia (Italy) [population of about 500,000] conducted an audit study in its four general hospital and one rehabilitation hospital to verify whether or not the diagnosis of COPD was correctly supported by spirometry. Taking a 2-year period into account (from 2005 to 2006), 379 clinical records were selected with a first diagnosis of COPD coded according to the *International Classification of Diseases* (ninth revision) as 49120, 49121, or 51881 (respiratory failure), with 49120 or 49121 as a second diagnosis. The mean percentage of COPD diagnoses supported by spirometry was 19.3%; interestingly enough, however, there were large variations according to the organization of hospital units. In our only Pulmonary Rehabilitation Unit, clinical records documented the use of spirometry in 46.1% of cases, while in the pulmonary services within internal medicine departments documentation of the use of spirometry was present in 14.4% of cases (range, 9.2 to 26.0%). Since our hospitals refer respiratory problems to pulmonologists of similar experience, it is evident that the organization and operational context may play a significant role, as demonstrated in the literature in relation to survival and length of hospital stay.[3,4]

In the editorial comment, our article (*ie*, Lusuardi et al[5]) was kindly quoted as an example of the potential overutilization of office spirometry in COPD diagnosis. Actually, the real conclusions of the study were rather disappointing with regard to the regular use of spirometry in primary care, and one detail from the final conclusions of the study should be underlined: the use of a questionnaire was comparable to office spirometry in identifying the patients with the highest probability of a COPD or asthma diagnosis, which is exactly in line with the recommendations of Enright and Quanjer.[2]

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Response

To the Editor:

We thank Drs. Lusuardi and Orlandini for their comment on our recent article,[1] in which they highlight the poor overall usage of spirometry seen in both the primary care and specialty settings in our study. They also report data that they have collected suggesting spirometry utilization varies by practice setting, with patients being cared for in a pulmonary rehabilitation unit having received significantly more documented spirometry than patients being cared for on pulmonary services within internal medicine departments. As we point out in our article, our data may have been biased by the fact that pulmonologists were not separated from other specialists. Several other reports[2,3] have suggested higher spirometry utilization among pulmonologists. We also appreciate the comments regarding the letter writers' own study[4] that failed to find a significant advantage to office spirometry in the general practice setting to improve the diagnosis of asthma and COPD, highlighting the need to identify appropriate patients for testing and for general practitioners and pulmonologists to work together.

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Noninvasive Ventilation for Patients With Neuromuscular Disease and Acute Respiratory Failure

To the Editor:

In our opinion, the very interesting article in *CHEST* (August 2007) by Garpestad et al[1] successfully contributed to a better understanding of noninvasive ventilation (NIV). However, we missed one potentially important indication for NIV, part time or continuous ventilatory support during an episode of acute respiratory failure (ARF) in patients with neuromuscular disease (NMD). Although the few studies on these patients[2–4] have been designed without a randomized control group that utilized tracheostomy ventilation (TV), all of them have underlined the effectiveness of NIV on the basis of two consistent outcomes: preventing endotracheal intubation; and avoiding mortality during these episodes. The lack of studies with a control group may have allowed Garpestad et al[1] to exclude patients with NMD as candidates for NIV in acute settings; but, keeping in mind that most of these patients rejected TV, in our opinion a protocol in which randomization might suppose the death of some of the patients included is both ethically and technically unfeasible. In a previous study[4] of patients who were unable to breath, we found that three of the four patients who previously had rejected TV, but not continuous NIV, survived an episode of ARF.

On obtaining the informed patient's agreement to receive this treatment and in the absence of severe bulbar involvement,[4] continuous NIV during ARF in NMD must be performed in a specific designated area with an available cohort of staff who have the appropriate experience. Matters to be considered are the appropriateness of the ventilation devices, the possibility of combining nasal or oronasal with mouthpiece interfaces, and the effectiveness of noninvasive aids to clear the patient's airway secretions.

Patients should be carefully monitored and, if NIV fails, those who previously have accepted TV should be intubated without delay. In the patients who reject TV, all of the futile procedures (including NIV) should be interrupted, and adequate palliative care should be instituted immediately.

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