they praised the clinical usefulness of endobronchial ultrasound (EBUS), its benefit beyond the clinical scope of patients, and its lowering of the cost burden on the health-care system.

The correspondence cites weaknesses we identified in our discussion. We agree that the value of EBUS to our institution should not be generalized and even suggested that using our dollar amount may translate to financial loss in certain institutions. We offered assessment steps (outlined in Table 3 in the article) and feel that by doing their homework, some institutions will discover they are better off not adopting a new procedure. We also designed the study to be conservative in estimates of downstream revenue by excluding patients already in the system, capturing actual collections, and only including collections related to the reason for EBUS. Furthermore, our collections are likely less in relation to other practices because our payer mix includes a substantial percentage of unfunded or underfunded patients. Our institution is a tertiary care center with pulmonologists trained in EBUS, but our catchment area contains hospitals with mediastinoscopy, endoscopic ultrasound fine needle aspiration, and traditional tranbronchial needle aspiration, and EBUS and medical oncologists, thoracic surgeons, and radiation oncologists.

Opportunity costs are a concern for physicians devoting time away from higher-paying endeavors, and, for that reason, downstream revenue is a consideration for them as well. Although this article focused on downstream revenue to a health-care system, future evaluations may include downstream revenue to the individual.

A discount method was not needed, because we limited collections to 1 year after the procedure (month by month would be desired when there are tight margins and seasonal variation of cases). Furthermore, a discount rate is not practical given the variety of services received by these patients, each with its own discount rate based on cost of capital.

We are disappointed with the assertion of Dr Fadul and colleagues that our analysis may not be free of “real or perceived conflicts of interest” without offering a reason. Potential conflicts of interest permeate the medical literature, but we remind them that this is a peer-reviewed manuscript and throughout the review process the issue of conflict never arose. Like them, we disclosed all conflicts of interest in the published article. The study was supported by an unrestricted industry grant, and the company had no influence whatsoever over the design, analysis, or manuscript preparation. We believe the readership of CHEST is savvy enough to decide whether to accept the results based on the disclosed funding source.

Obviously, our institution’s monetary gain is not applicable to all hospitals, but the concepts presented should be considered and may prevent ill- advised expenditures. The only complication to our understanding of obesity hypoventilation syndrome, an increasingly prevalent condition, the study confirms the data from uncontrolled studies and reaffirms the current clinical consensus by demonstrating the clinical improvements that occur when managing patients with obesity hypoventilation syndrome with noninvasive ventilation. The study control group involved lifestyle advice and was also associated with significant improvements in some clinical parameters. It should be noted that sleep-disordered breathing has been shown to improve with lifestyle modification and exercise training, and, thus, the control group was not a sham of active treatment but employed a different modality of treatment. This may explain, in part, the nonsignificant findings of the results.

Previous data and the current study have been unable to demonstrate a reduction in weight or improvement in physical activity with the treatment of sleep-disordered breathing. However, the heightened cardiovascular risk in this patient group and the benefits of even modest risk modification indicate this is a vital area of management. In recent work published by our own group, we showed that improvements in objectively assessed physical activity in this patient population occurred following treatment with noninvasive ventilation are correlated with reduction in weight. However, as with the current article, we were unable to demonstrate that anthropometric changes led to improvements in glycemic control or serum lipids. Evaluation of the addition of specific training and lifestyle modifications to conventional medical therapy are required to advance this challenging area of respiratory and sleep medicine.

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Financial/nonfinancial disclosures: The authors have reported to CHEST the following conflicts of interest: Dr Silvestri received an Olympus grant for this study and other research projects, and an Allegro Diagnostics Corp research grant. Dr Pastis received a consultant fee from Olympus America.

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Obesity Hypoventilation Syndrome
The Need For a Multifaceted Approach to Treatment

To the Editor:

The article by Borel et al in CHEST (March 2012) is a timely addition to our understanding of obesity hypoventilation syndrome, an increasingly prevalent condition. The study confirms the data from uncontrolled studies and reaffirms the current clinical consensus by demonstrating the clinical improvements that occur when managing patients with obesity hypoventilation syndrome with noninvasive ventilation. The study control group involved lifestyle advice and was also associated with significant improvements in some clinical parameters. It should be noted that sleep-disordered breathing has been shown to improve with lifestyle modification and exercise training, and, thus, the control group was not a sham of active treatment but employed a different modality of treatment. This may explain, in part, the nonsignificant findings of the results.

Previous data and the current study have been unable to demonstrate a reduction in weight or improvement in physical activity with the treatment of sleep-disordered breathing. However, the heightened cardiovascular risk in this patient group and the benefits of even modest risk modification indicate this is a vital area of management. In recent work published by our own group, we showed that improvements in objectively assessed physical activity in this patient population occurred following treatment with noninvasive ventilation are correlated with reduction in weight. However, as with the current article, we were unable to demonstrate that anthropometric changes led to improvements in glycemic control or serum lipids. Evaluation of the addition of specific training and lifestyle modifications to conventional medical therapy are required to advance this challenging area of respiratory and sleep medicine.

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Funding/Support: The study group has received unrestricted educational grants from ResMed, Philips Respironics, Fisher & Paykel Healthcare, and B&D Electromedical LLP. The authors acknowledge financial support from the Department of Health via...
the National Institute for Health Research (NIHR) comprehensive Biomedical Research Centre award to Guy’s & St. Thomas’ NHS Foundation Trust in partnership with King’s College London and King’s College Hospital NHS Foundation Trust and the NIHR Respiratory Disease Biomedical Research Unit at the Royal Brompton and Harefield NHS Foundation Trust and Imperial College London.

Financial/nonfinancial disclosures: The authors have reported to CHEST the following conflicts of interest: Dr. Murphy has received expenses for travel to conferences from Philips Respironics. Dr. Hart has received fees for lecturing from Philips Respironics and Fisher & Paykel Healthcare. Dr. Polkey has received fees for lecturing from Philips Respironics.

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ACKNOWLEDGMENTS

Role of sponsors: The sponsors had no role in the design of the study; the collection and analysis of the data, or in the preparation of the manuscript.

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Response

To the Editor:

We appreciate the interest of Dr. Murphy and colleagues in our article in CHEST on a randomized control study investigating the efficacy of noninvasive ventilation (NIV) on respiratory, sleep, cardiovascular, metabolic, and inflammatory outcomes in obesity hypoventilation syndrome (OHS). We acknowledge that our control group was not submitted to sham-NIV, but had only lifestyle counseling. Indeed, any participation in a clinical research study is likely to improve a patient’s lifestyle. Therefore, patients in both groups may have changed their lifestyle (eg, increased their physical activity). In any case, this cannot mask a specific effect of NIV on cardiovascular and metabolic parameters.

Our study demonstrated that short-term NIV dramatically improves sleep and blood gases but does not alter inflammation, metabolic, or cardiovascular markers. This suggests a need to address these comorbidities by offering combined treatment modalities. Programs aimed at reducing the detrimental consequences of obesity systematically target an increase in physical activity and a reduction in sedentary behaviors. In their recent work in patients with OHS, Murphy and colleagues reported objective improvement in physical activity after 3 months of nocturnal NIV. This elegant study, including patients both in a stable state and in post-acute respiratory failure, compared two ventilatory modes with a randomized control design. Therefore, all patients were exposed to NIV treatment. Thus, it is disputable whether weight loss and physical activity improvements were related to NIV per se or to lifestyle changes associated with inclusion in a clinical research protocol. Moreover, patients placed on NIV during post-acute respiratory failure were likely to have low physical activity at baseline, whereas a dramatic improvement in activity was expected after 3 months of recovery.

Actually, obesity itself promotes limited physical activity and sedentary behavior partly because of exercise-related dyspnea. NIV during exercise training in rehabilitation programs might enhance exercise capacity by reducing the respiratory load in subjects who are morbidly obese. Additionally, we have demonstrated that the training of respiratory muscles in subjects who are obese improves dyspnea and exercise capacity. We are currently evaluating these tools in rehabilitation programs aimed at reducing cardiometabolic risks in obese subjects, in addition to nocturnal NIV. NIV initiation could, thus, be the appropriate starting time for such integrated programs, although the best modalities to improve motivation and adherence have yet to be determined. Future studies should not only evaluate the efficacy of combining nocturnal NIV and rehabilitation programs, but also determine the rate of drop-outs and the cost-effectiveness of such combined strategies in OHS treatment.

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Financial/nonfinancial disclosures: The authors have reported to CHEST that no potential conflicts of interest exist with any companies/organizations whose products or services may be discussed in this article.

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