To the Editor:

We thank Dr. Ng and his colleagues for their valuable comments on our article.1 Our study aimed to answer two questions. First, does a first-night effect exist in childhood sleep, and second, is single-night polysomnography adequate in assessing children with sleep-related disordered breathing? We agree that the available population was biased toward obese children, and we fully acknowledged that limitation in our discussion. This limitation may have some bearing to the second question our study tried to answer, but we do not think the presence of the first-night effect would have been affected by the body mass index of the subjects.

The diagnostic cutoff for childhood obstructive sleep apnoea (OSA) is still very much a controversial issue. Dr. Ng and colleagues argued that the apnoea-hypopnea index (AHI) would be a better diagnostic parameter for diagnosing OSA. They suggested that we use a cutoff of AHI > 1.5, and they based their suggestion on a letter to the editor by Witmen et al.2,3 Thus, only IL-8 blockade itself may not have a great influence on disease activity. We are also concerned about the relatively low circulatory levels of the cytokine (ie, <50 pg/mL) in the subjects examined. Alternatively, these data may indicate that IL-8 does not contribute to the symptoms of COPD in stable stages of the disease. Instead, we want to be informed of any previous studies in the literature in which the possible correlations between IL-8 and an exacerbated phase of COPD have been demonstrated.

In the “Results” section, the differences in the transition dyspnea index total score between the fully human monodonal IgG2 antibody directed against interleukin-8 (ABX-IL-8)-treated group and the placebo group showed significance only at week 2 in their follow-up period. The results suggest that an IL-8 neutralizing strategy may be effective only for the short-term use of a chemoattractant of effector cells (neutrophils) in patients with COPD. Since COPD is one of the representative chronic disorders, the results by Mahler and coworkers cannot satisfy the patients and physicians.

Finally, we think the administration of ABX-IL-8 by inhalation should lead to better, more optimistic outcomes for subjects. The majority of reports have shown elevated IL-8 levels in the sputum samples of patients with COPD. However, as we have confirmed in murine models, the elevated levels of the local expression of proinflammatory cytokines, such as IL-1β, and chemokines, such as keratinocyte chemotactant, macrophage inflammatory protein-1α, and macrophage chemoattractant protein-1, during lung inflammation are not always paralleled by systemic circulatory levels of the molecules (unpublished obser-
appropriate to evaluate biological agents in COPD. Administration of ABX-IL8 may be beneficial (as suggested by Dr. Inoue and colleagues, there are many unanswered questions about the role and the importance of the numerous mediators of inflammation in COPD. This study was performed to assess the appropriate dose of ABX-IL8, to consider whether inhaled infusions of monoclonal antibody recognizing interleukin 8 (ABX-IL8) were safe and well tolerated compared with placebo. These safety data are evidence to assess new therapies for patients with COPD. As noted in the subtitle and in the “Discussion,” this was a pilot study. We agree that recruitment of subjects and performance of placebo-controlled trials in patients with symptomatic COPD are both difficult and challenging. However, such randomized controlled trials are critical for providing the scientific evidence to assess new therapies for patients with COPD.

We appreciate the interest expressed by Dr. Inoue and colleagues in our study evaluating monoclonal therapy recognizing interleukin (IL)-8 in patients with COPD. We agree that, first, we are not as well trained as we would like to be, and, second, that those who are upstream from us are even less likely to be as attuned to these issues and are more uncomfortable in dealing with them. And above and beyond these issues are the economic disincentives that provide pressure against doing the right thing, even when the medical team is well-trained.

We strongly support the advocacy by Marik and Wood for better education in Palliative and End-of-life Care (The EPEC Project) is a training program that was developed by the American Medical Association with support from the Robert Wood Johnson Foundation to bridge the gap in training in end-of-life care that the vast majority of physicians have experienced. It was designed as an intensive 2-day educational experience to bring competence to physicians in all of the major aspects of end-of-life care. It does not turn physicians into palliative care experts any more than advanced cardiac life support turns us into cardiologists, but it does provide a solid foundation and basic competencies in end-of-life care, and it allows us to teach and learn with primary care physicians, specialists, nurses, clergy, social workers, and others. It is a powerful course, one that can lead to tears of recognition or uneasiness when we recognize ourselves in others eyes, and to joy when we rediscover many of the profoundly human reasons why we are so privileged to be physicians.

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