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To the Editor:

We are grateful for the interest and comments by Neri et al1 on our article.2 and we appreciate the opportunity to respond. We agree with the authors that the finding of an association between fractures and higher bone mineral density (BMD) is not clinically useful. The importance of this finding is more to emphasize the point that the association between T scores and fracture risk is unclear in patients with cystic fibrosis (CF). It may be erroneous to assume that the data from postmenopausal women applies to those with CF. At our institution, the dual energy x-ray absorptiometry report states the fracture risk based on the T score, and that this may be misinterpreted, especially for caregivers who are unfamiliar with CF-related bone disease. Based on this report, bisphosphonates may be prescribed for CF patients with osteoporosis; however, the ability of these drugs to decrease fracture risk has not been studied. Bisphosphonates seem to increase BMD in CF patients.3,4 but whether this translates into fracture prevention is unclear. To initiate the long-term use of a drug in a population of young premenopausal patients, which potentially may have effects on fetal development, must be considered carefully.

Neri et al1 recognized the importance of developing a tool to evaluate the risk of fractures in CF patients, which, we agree, is much needed. Using World Health Organization criteria,2 36% of the subjects in our no-fracture group had osteopenia and 9% had osteoporosis. Within the fracture group, 25% had osteopenia and none had osteoporosis. However, it is unclear how knowing this breakdown helps clinicians assess fracture risk, given the paradoxical findings of our study. The more important question is whether or not a different modality for evaluating BMD in CF can identify those patients who are at high risk for fracture based on an abnormal test result. DEXA may not be the appropriate tool for this population. Hopefully, future studies will help to clarify these issues.

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The authors have reported to the ACCP that no significant conflicts of interest exist with any companies/organizations whose products or services may be discussed in this article.

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Cost Analysis of Intensive Glycemic Control in Critically Ill Adult Patients

To the Editor:

We congratulate Krinsley and Jones1,2 for their impressive improvement in patient survival, which was attributed to tight glucose regulation. Surprisingly, the intensification of therapy led to decreased costs, mainly due to shorter ICU and hospital stays. Fortunately, this finding has already been confirmed.3 However, they found1,2 a greater improvement in mortality than did van den Berghe et al4 in a key study. Moreover, in patients who were more comparable with the patients studied by Krinsley,1 and Krinsley and Jones,2 they found no significant improvement in mortality in the intention-to-treat analysis, although a tighter upper limit of 110 mg/dL was used instead of 140 mg/dL.5

We wonder whether other factors might have influenced the findings. In a before-and-after study, subtle changes in policy or patient characteristics can easily influence outcome without apparent changes in APACHE (acute physiology and chronic health evaluation) II score or diagnostic groups. The obviously very comprehensive database should allow further analyses. It might be worthwhile to check for factors known to influence patient outcome. We would suggest checking the following: medical staff availability; nursing staff availability; nurse/patient ratio; bed occupancy ratio; rate of referred patients; availability of daily rounds; hospital admission and discharge protocols; the introduction of ventilatory protocols; and the introduction of early goal-directed therapy. This might moderate the effect of glucose regulation, putting it more in line with the results of van den Berghe et al.6 Otherwise, the effect on mortality of a single measure seems incredibly high.

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ERRATUM

In the September 2005 issue, in the article by Szefler et al, “Ciclesonide, a Novel Inhaled Steroid, Does Not Affect Hypothalamic-Pituitary-Adrenal Axis Function in Patients With Moderate-to-Severe Persistent Asthma (Chest 2005; 128:1104–1114),” the wording of the Abstract in the print edition, lines 10-12 is incorrect. It should read: “FP 1760 produced a statistically significant suppression in mean serum cortisol AUC0-24h compared to PBO (p = 0.0009; 95% confidence interval [CI] – 117.5 to – 32.1).” The pdf version online is correct and that version should be used.