scanty, suction catheter tips were cultured by a roll-plate method to improve the sensitivity of sampling in the lower respiratory tract.

Bacteriologic methodology is also of fundamental importance. Bonten et al state that "routine bacteriologic procedures" were used in the laboratory. Does this mean that the common practice of selecting the predominant or most numerous colony types from the primary isolation agar was used? If so, minority bacterial populations and therefore potentially ecologically significant isolates might have been missed. On the other hand, establishing the co-identity of paired isolates from different sites by relying on phenotypic characteristics is inadequate. Nowadays, it is customary to use one of the molecular methods to establish the clonality of paired isolates.

Despite these methodologic problems, Bonten et al report a possible oropharynx/stomach-to-trachea or -lung sequence in patients 8 and 9. Although the stomach appears not to have been the primary source of Gram-negative colonization in this group, the data presented do not support the conclusion implied in the article's title.

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To the Editor:

We thank Dr. Inglis for his critical evaluation of our study, which is directed basically to three methodologic topics: the bacteriologic analysis, the deletion to perform determination of co-identity of paired isolates with molecular methods, and the frequency of collection of specimens.

With regard to the bacteriologic methodology, we analyzed colonization semiquantitatively, using the four-quadrant streak method on sheep blood (with and without cefuroxim 25 µg/mL), CLED, chocolate, Sabaroud, and SBA (Slanetz and Barley) plates. Cultures were inoculated immediately after sampling, even on weekends. All species that could be identified were isolated, and therefore, it is not likely that isolates, even those present in small amounts, have been missed.

Second, we fully agree that molecular biotyping techniques provide more information to establish co-identity of paired isolates. However, based on phenotypic characteristics the stomach was the initial site of colonization in only a few cases. It is obvious that the use of a more discriminative method only could have decreased, but never increased, the number of cases identified.

Finally, the reliability of the sampling frequency is questioned, which consisted of serially obtaining cultures from the stomach, oropharynx, trachea, and rectum on admission and subsequently twice a week. We agree that obtaining cultures more frequently might have provided more detailed information. However, the minimal frequency of collection of specimens necessary to show sequences with optimal reliability has never been determined. Our data proved to be consistent and have been confirmed in another recent study. Interestingly, Dr. Inglis refers to the results of a recently published study (Lancet 1993; 341:911-13), in which gastric cultures were obtained every 8 h, but cultures of tracheal aspirates were obtained every 2 days, whereas cultures of oropharynx were obtained on admission only. In this study, a sequence of colonization from the stomach to the trachea was shown in 11 out of 100 patients. However, in five of these patients colonization was demonstrated simultaneously at both sites on admission. In contrast to the authors' opinion, we do not believe that a sequence from stomach to trachea was shown in these patients. Moreover, very short time differences between isolation of species from stomach and trachea were observed in four other patients. In these cases, a sequence from stomach to trachea may have been interpreted falsely as a result of the different sampling frequencies at both sites. As a result, colonization of the lower respiratory tract with species initially colonizing the stomach could only be demonstrated clearly in 2 out of 100 patients. Moreover, the interpretation of sequences was seriously hampered because analysis of oropharyngeal colonization was not included. Therefore, we still believe that our data provide important information on the pathogenesis of ventilator-associated pneumonia, and we do not support an important role of the stomach herein.

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Another Viewpoint
What Is a Primary Care Physician?

To the Editor:

I read with interest Dr. Block's editorial "What Is a Primary Care Physician?"1 I have been trying to answer this same question in my new role as a faculty member in a family practice residency program in Central Virginia. I agree with Dr. Block that many patients with chronic obstructive lung disease and asthma can be effectively managed by family practitioners or general internists. This is particularly true when the family physician communicates with a pulmonologist when the patient does not respond to specific therapy. As a practicing pulmonologist, however, I do not agree with Dr. Block's assumption that a pulmonologist who adeptly manages diabetic ketoacidosis in the ICU also has expertise in the outpatient management of diabetes mellitus. This assumption formed the basis for Internal Medicine training for many years and can be reformulated as follows: if you can take care of sick patients in the hospital, you can take care of anybody in the office. The management of a patient's diabetes in the office is in many ways as art: how does one switch a patient over to insulin from an oral hypoglycemic? When? What is the most ef-