To the Editor:

We thank Drs Thapamagar and Mallareddy for their thoughtful comments and careful review of our recent study in CHEST.1 As described in “Study Outcomes” in the “Materials and Methods” section of our study, the secondary outcome was defined as the combined end point of all-cause mortality, objectively confirmed nonfatal symptomatic recurrent VTE, or nonfatal major bleeding.

Overall, in our study of 526 patients with acute symptomatic pulmonary embolism, 30-day all-cause mortality was 7.6% (40 of 526; 95% CI, 5.3% to 9.9%), and 21 patients suffered nonfatal symptomatic recurrent VTE or nonfatal major bleeding. Thus, the secondary end point was reached by 11.6% (95% CI, 8.9% to 14.3%) of patients (61 of 526). To present more detailed secondary outcomes across the models’ strata in Table 4, the frequencies for reaching the combined secondary end point were given separately for “death of any cause” and “nonfatal VTE recurrence or major bleeding.”

Overall, and as described in the article, 12 of the 207 patients in the European Society of Cardiology low-risk strata (5.8%; 95% CI, 2.6% to 9.0%) met the secondary outcome. Of those, seven patients (3.4%; 95% CI, 0.9% to 5.8%) died, and five patients (2.4%; 95% CI, 0.3% to 4.5%) suffered nonfatal recurrent VTE or major bleeding. We apologize that the definition of secondary outcomes and the more detailed description of those in Table 4 led to confusion. In conclusion, our study adds to the body of evidence that the simplified Pulmonary Embolism Severity Index successfully identifies low-risk patients presenting with acute pulmonary embolism.

Mareike Lankeit, MD
Mainz, Germany
David Jiménez, MD, PhD
Madrid, Spain

Response

To the Editor:

We read with great interest the article by Barraud et al1 in this issue of CHEST (see page 646) discussing the effects of probiotics on mortality in critically ill adult patients. We congratulate them and applaud their work, but we feel a couple of issues must be addressed.

First, in the “Selection of Studies” section the authors declared that randomized controlled trials were potentially evaluated if they enrolled critically ill adult patients admitted into an ICU and compared the administration of probiotics (and/or prebiotics or synbiotics) and control (placebo or other), and that the articles must also have reported on ICU or hospital mortality. Was that the inclusion/exclusion criteria of their report? If so, why did they not include the trial conducted by Giamarellos-Bourboulis et al?2 The authors should have given a more detailed description of the inclusion/exclusion criteria.

Second, the meta-analysis also evaluated the effects of probiotics on secondary outcomes, including all-cause hospital mortality, incidence of ICU-acquired infection, incidence of diarrhea, duration of mechanical ventilation, and ICU and hospital length of stay. In fact, these results are not conclusive because the examination of the effects of probiotics on these end points was not adequately powered. They were not regarded as the primary outcome but were the only clinically significant end points consistently reported in some of the studies included in this meta-analysis. More data are needed to clarify these questions in essence instead of inform.

Wan-Jie Gu, MSc
Jing-Chen Liu, MD
Nanning, China

Affiliations: From the Department of Anaesthesiology, the First Affiliated Hospital of Guangxi Medical University.

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Correspondence to: Jing-Chen Liu, MD, Department of Anaesthesiology, the First Affiliated Hospital of Guangxi Medical University, 22 Shuangyong Rd, Nanning 530021, Guangxi, China; e-mail: jingchenliu1966@yahoo.com.cn

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Probiotics in Critically Ill Patients

More Data Are Needed

To the Editor:

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