Editor’s Note: Authors are invited to respond to Correspondence that cites their previously published work. Those responses appear after the related letter. In cases where there is no response, the author of the original article declined to respond or did not reply to our invitation.

Prognostic Scores in Pulmonary Embolism

To the Editor:

We reviewed the article published in CHEST (April 2012) by Lankeit et al during our monthly journal club. The authors have done an impressive job of assessing the performance of the two prognostic models for pulmonary embolism in predicting short-term mortality: the European Society of Cardiology (ESC) model and the simplified Pulmonary Embolism Severity Index.

While reviewing this article, we came across a few contradicting data. In the “Results” section, while reporting secondary outcomes, the authors mentioned that secondary end points occurred in 1.8% (95% CI, 0.2-3.9%) of the simplified Pulmonary Embolism Severity Index low-risk strata and 5.8% (95% CI, 2.6-9.0%) of the ESC low-risk strata with a difference of 4.0% points (95% CI, 0.2-7.8).

However, in Table 4, which is the corresponding table for both primary and secondary outcome, the percentage of patients who met secondary end points in low-risk ESC model strata is reported as 2.4% (95% CI, 0.3-4.5), which is different from the percentage reported in the text. To evaluate this discrepancy, we took the liberty of editing Table 4 to make it more illustrative (Table 1). In this table, we can clearly see that if we accept the secondary end point rate for the ESC model as such, the total number of secondary end points is 21, which is the reported total number of secondary end points in the study. But, if we accept the secondary end point rate of 5.8% for low-risk ESC model strata, then the total number of patients meeting this end point for low-risk ESC model strata would be 12 and total number of secondary end point events for the ESC model would be 28 (low-risk strata = 12 and elevated-risk strata = 16) (Table 2), which is incorrect according to the reported events in the study.

In the absence of the raw data, we are not in the position to make the final evaluation of this discrepancy. Therefore, it would be helpful if the authors could elaborate on these data and indicate the effects on secondary end points if it was incorrectly reported.

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Table 1—Thirty-Day Mortality and Nonfatal Adverse Events Based on the sPESI and the ESC Prognostic Model (Reformatted From the Original Lankeit et al Table 4)

<table>
<thead>
<tr>
<th>Model</th>
<th>Study Sample</th>
<th>Patients (N = 526)</th>
<th>Death of Any Cause (n = 40)</th>
<th>Nonfatal Recurrence or Major Bleeding (n = 21)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ESC model</td>
<td>Low risk (n = 207)</td>
<td>39.3 (35.2-43.5)</td>
<td>3.4 (0.9-5.8)</td>
<td>2.4 (0.3-4.5) (n = 5)</td>
</tr>
<tr>
<td>Esc model</td>
<td>Elevated risk (n = 319)</td>
<td>60.7 (56.5-64.8)</td>
<td>10.3 (7.0-13.7)</td>
<td>5.0 (2.6-7.4) (n = 16)</td>
</tr>
<tr>
<td>sPESI</td>
<td>Low risk (n = 165)</td>
<td>31.4 (27.4-35.3)</td>
<td>0</td>
<td>1.8 (0.3-3.9) (n = 3)</td>
</tr>
<tr>
<td>sPESI</td>
<td>High risk (n = 361)</td>
<td>68.6 (64.7-72.6)</td>
<td>11.1 (7.8-14.3)</td>
<td>5.0 (2.7-7.2) (n = 18)</td>
</tr>
</tbody>
</table>

Data given as % (95% CI) unless otherwise indicated. ESC = European Society of Cardiology; sPESI = simplified Pulmonary Embolism Severity Index.

Table 2—Thirty-Day Mortality and Nonfatal Adverse Events Based on the ESC Prognostic Model (Accepting a Secondary End Point Event Rate of 5.8% for Low-risk Strata)

<table>
<thead>
<tr>
<th>ESC Model</th>
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<td></td>
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</tr>
</tbody>
</table>

Resultant secondary end point events = 28

Data given as % (95% CI) unless otherwise indicated. See Table 1 legend for expansion of abbreviations.
To the Editor:

We thank Drs Thapamagar and Mallareddy for their thoughtful comments and careful review of our recent study in CHEST. 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.

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David Jiménez, MD, PhD
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References

Response

To the Editor:

We read with great interest the article by Barraud et al 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. 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.

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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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Probiotics in Critically Ill Patients
More Data Are Needed

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