way. In a recent study by Ibrahim and coworkers, it is likely to contribute to the risk in an additive manner. In the community-acquired pneumonia (CAP) in 388,406 patients admitted with CAP in the United Kingdom, we found that death after PE in those who were normotensive but without RVD and the mortality in that group was 15% in those who did not receive thrombolysis was 7.2 per 1000 person days. We therefore believe that it would be wrong to underestimate the early acute PE-associated mortality risk, even in normotensive patients. We believe this is significant in those with objective evidence on echocardiography or on cardiac biomarkers of RVD. It is this association which is eloquently described in the original article on which our editorial was based, stressing its importance to the literature.

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Competing interests None.

Predicting CAP-related mortality with CRB-65
Ewig et al are to be commended for their very large study of 388,406 patients admitted with community-acquired pneumonia (CAP) in German hospitals from 2005 to 2006. Using the CRB-65 tool (confusion, respiratory rate ≥30 min, low blood pressure (either systolic <90 mm Hg or diastolic ≤60 mm Hg) and age ≥65 years), the authors found 30-day mortality rates of 2.4, 13.4 and 34.4% in those with 0 points, 1–2 points and 3–4 points, respectively. As a result, the authors promote this tool as being accurate for predicting CRB-65-related deaths.

However, while this appears impressive, it is notable that of the >54,700 deaths, only 29.0% were classed as high risk, whereas 68.1% were only intermediate risk and 2.8% were low risk. In addition, many of those patients who died had treatment limitations applied and only 15.7% of the patients who died received ventilatory support. These two points raise the question of how clinically useful this tool really is. If over two-thirds of deaths were classed as having clinically ‘moderate’ CAP, then the tool cannot really be described as being accurate for this purpose. Furthermore, if the vast majority of people who died did so after active treatment was withdrawn, then the identification of such patients does not appear to serve much purpose. It would be more relevant to assess such a tool for its ability to identify those patients in whom every effort is made to save their lives—that is, those admitted to the intensive care unit.

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Authors’ response
We thank Dr Charles for his important comments. He raises the important question of whether CRB-65 is a useful tool to advise treatment limitations. If only 29% of those who finally died were at high risk of death at initial presentation (CRB-65 risk class 3), such a tool may be of limited value in this regard. In fact, we agree that the CRB-65 score (like any other such as the PSI) is not helpful for the decision to apply treatment restrictions. Such restrictions up to fully palliative treatment cannot be based primarily on considerations about the current risk of death but should be the result of a careful evaluation of the clinical state and overall prognosis of the patient, both initially and during follow-up, and such decisions should be decided with the patient or his legal social worker.

In this context, the CRB-65 severity score remains important as part of the initial clinical evaluation of all patients. Treatment restrictions must not follow a hidden agenda.
Predicting CAP-related mortality with CRB-65

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