In the second version of this manuscript, the first 5 pages are introductory material.

The shear parameter does not appear to be an adequate measure of stability and it could be that this is why the scatter and errors remain large. The shear parameter includes roughness and terrain effects and is likely direction sensitive. If the authors really insist that they do not have to compare with stability determined from sonic data (which they have available) then at least they should call this a shear parameter and not stability, or show that it is equivalent.

It is not clear which of the steps in sections 3.1-3.5 have been applied and whether they are needed. What are the physics-based corrections in L-TERRA? Maybe the errors in the large shear case are because the exponent used is wrong? It is unfortunate that the r2 values are so little impacted. The MAE improvement of 0.26 is very difficult to evaluate. Does it mean the difference between the met tower TI (height?) and the WC TI was previously larger than 1.50 so for example it could be that WCTI was 11.5% while the met tower TI is 10% and with TERRA it is now predicted to be 11.26%? Is that a correct interpretation? At the wind farm site the improvement in MAE appears to be 0.28. Are these really significant as stated in the abstract when there is very little or no change in the slope or r2 values?

Section 3.6 is called comparison to previous methods but there is no quantitative comparison. Nor is there a qualitative evaluation of which of the preprocessing steps are necessary and have utility. It is unfortunate because a quantitative comparison here would add value, even if the overall results show rather small improvements from using L-TERRA.

With a more quantitative approach to the processing there could be something here. But based on these results and with the lack of any physical detail of the model, or processing detail that indicates the value of the steps it is unfortunately not a very compelling analysis.