Final response to the following Referee comment

https://doi.org/10.5194/amt-2021-126-RC1, 2021

Anonymous Referee #2

Referee comment on "Options to correct local turbulent flux measurements for large-scale fluxes using a LES-based approach" by Matthias Mauder et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-126-RC1, 2021

The manuscript by Mauder et al evaluates a new method to correct turbulent flux measurements for the widely observed energy balance non-closure. The manuscript addresses an important research question and tests a new approach to solve a long existing problem for measurements of turbulent fluxes. I have a few comments that hopefully can improve the quality of this manuscript.

- Additional statistical analyses could be used to test if the flux corrections result in statistically significant improvements of the energy balance closure. The results qualitatively indicate improvements, but further statistical support of the findings would strengthen this study (e.g., through additional regression uncertainty analysis).

➔ We have chosen the orthogonal Deming regression method in order to account for the uncertainty of both, the x- and the y-variable. We have presented the Pearson's r-coefficient as a measure of goodness for the fits. In addition, we have conducted test on the significance between two correlations and included the results of these tests in the discussion section at the respective text passages.

- Unfortunately, the only site where the correction procedure can be applied directly has already a good energy balance closure, while the two other sites are characterised by a substantially worse closure. It would be helpful if sites with similar energy balance closures could be selected or at least if this issue would be discussed in more detail.

➔ We agree that the energy balance closure is already quite good for the DK-Sor site. We also agree that it would be good to do a similar validation for more sites with different energy balance closure. However, this is not possible within this study. Hence, we have followed the suggestions of the reviewer and extended the discussion in this direction:

> The only test site DK-Sor, where the correction method can be applied directly, already has a relatively good SEB closure to begin with. The good closure can be explained by the rough surface in the surrounding in combination with the relatively high wind velocities that are typical for this region of Denmark. This leads to relatively high values of u_*/w_* , indicating forced convective conditions most of the time. In principle, an additional site with more unstable conditions would be interesting for this study as a complement. However, such sites with high-quality energy-balance data, which also fulfill the criterion of $z_m > 20$ m are scarce. Theoretically, under more strongly unstable conditions, the LES-based correction would be much larger, and also the non-hydrostatic energy transfer might become more relevant (Sun et al., 2021). It is warranted that this correction method is further evaluated, particular for less windy sites with a sufficiently large aerodynamic measurement height, good fetch conditions and high-quality biometeorological measurements.

- Lastly, a new study by Sun et al. (https://doi.org/10.1029/2020JD034219) presenting a new hypothesis for the energy balance non-closure at flux tower sites related to nonhydrostatic energy transfer should be discussed in the manuscript. It would contribute

to a comprehensive discussion of the universality of the correction procedures outlined in the manuscript.

➔ We have included a reference to the paper by Sun et al. (2021) in the discussion section as part of the paragraph in response to the previous comment, see above.