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Review of manuscript by Jahani et al.

Anonymous Referee #1

Referee comment on "Longwave radiative effect of the cloud–aerosol transition zone based on CERES observations" by Babak Jahani et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-421-RC1, 2021

By estimating the effect the zone of clear-to-cloud transition has on longwave radiation, the study addresses an important yet little known issue. By showing that these effects are quite substantial, the results provide information that could be of great interest to the community. The methodology is suitable for the task and the presentation quality is high. Even so, I believe that the manuscript needs some significant improvements. My recommendations for substantial improvements and for some minor refinements are listed below.

Main issues:

1.

It is important to clarify the sentence in Lines 90-91, which says "Indeed, for these pixels neither aerosol nor cloud optical property retrievals exist, yet they are classified as containing a cloud (Lost A), a non-cloud obstruction (Lost B), or were not processed at all in the cloud masking (Lost C)." Specifically, it should be clarified whether the study considers 1 km-size MODIS pixels as "cloudy" or "Lost" (most likely "Lost A") if the MYD06 cloud product does not include a positive retrieved value in the Scientific Data Set (SDS) "Cloud_Optical_Thickness", but includes a positive retrieved value in the SDS named "Cloud_Optical_Thickness_PCL". This occurs for partly cloudy 1 km-size pixels, in which clouds were detected for some, but not all 250 m-size subpixels.

Clarifying this would be important because if such pixels were considered "Lost", CERES footprints containing many small clouds could be included in the transition zone statistics even if their total cloud fraction was well above 10% and their longwave effects came from cloud elements for which the MODIS cloud product did provide cloud property estimates.

2.

Line 250 explains that the low-level transition zone effect of 0.8 W/m2 was calculated using the first four temperature difference (dT) bins in Fig. 4. However, it is not clear why four bins were used, rather than three, five, or more than five. This is a significant issue because Figure 4 suggests that the number of dT bins included into the low-level category can affect the results. It would help to explain why using the first four dT bins is a good choice. For example, could it be linked to a certain altitude range? It would also help to mention how the definition or the extent of the low-level category compares to the definition or extent in Eytan et al. (2020), which provided the radiative effect estimate of 0.75 W/m2 that was close to the 0.8 W/m2 in this paper.

3.

The transition zone statistics include CERES footprints where up to 10% of MODIS pixels have neither aerosol nor cloud data. This criterion is very reasonable, but it allows including footprints where the cloud fraction can reach 10% (or much higher, depending on the treatment of partly cloudy MODIS pixels, as discussed in Point #1 above). Therefore, it could be interesting to discuss whether the transition zone radiative effect shows any statistical relationship to cloud fraction within the CERES footprint. This could be done either for all dT bins combined or for selected dT bins only.

Suggestions for minor refinements:

Line 18: I suggest adding "the" between "onboard" and "Aqua".

Line 28: I suggest replacing "regardless of" by "without considering".

Line 39: I suggest adding "the" between "that" and "transition".

Line 81: I suggest adding "the" between "for" and "identification".

Lines 83-84: It would help to clarify whether the 3 km or 10 km resolution MYD04 aerosol product was used.

Lines 96-97: I suggest rewording “number of ocean MODIS pixels more than or equal to 75% of the expected ≈400 pixels falling within CERES field of view (FOV)” to something like “the number of MODIS ocean pixels equals or exceeds 75% of the ≈400 pixels expected to fall within the CERES field of view (FOV)”.