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Interactive comment on "3D Radiative Heating of Tropical Upper Tropospheric Cloud Systems derived from Synergistic A-Train Observations and Machine Learning" *by* Claudia J. Stubenrauch et al.

Anonymous Referee #1

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3D Radiative Heating of Tropical Upper Tropospheric Cloud Systems derived from Synergistic A-Train Observations and Machine Learning

The authors present a method for proving 3D radiative heating structures using an ANN trained from the AIRS, CloudSat 2b-FLXHR-lidar product, CIRS cloud properties, and reanalysis environmental properties. This is a novel method for expanding the converge of actively-based products in order to describe the effect of upper tropospheric clouds on tropical radiative heating rates and their relation to surface temperature. The ANN method has been applied previously and for this case expands 3D heating rates to a longer data record and increased spatial resolution to allow a more robust analysis on changes in upper tropospheric clouds and MCSs. The authors provide a thorough description of the retrieval and sources of error. It does seem crowded at times with the large amount of material and supplemental material. It would be good to streamline the use of extra material or to partition the addition retrieval/uncertainty aspects into a separate paper and move some supplemental material based on results to the main manuscript. Further, clarifications on some subject matters are needed and significance testing on the last two results sections is needed.

Major Comments

1. Looking at Fig 2 and Fig S4, there do seem to be some further physical explanations. For the LW it makes sense that error would be contained to cloud top in Cbs and Ci or just below cloud base in Ci. Below these regions the LW signal will likely be mostly impacted by the high RH in the tropical atmosphere. The SW signal does demonstrate variability below Ci cloud base ∼400 hPa. This could be errors in representation of Ci optical depth or clouds below the Ci reflecting SW back towards TOA. Multi-layer structures are essential to represent in Ci and thin-Ci in the tropics as the majority of cirrus contain a cloud below them (as in cited Hang et al 2019). Is there a way to capture if the ANN is representing the multi-layer structures below Ci? This is mentioned briefly around Line 445, but did not know if this was quantifiable.

2. Section 3.3. As mentioned in the text, during La Nina changes the location of cloud structures, but ENSO also significantly changes the size and occurrence of MCSs over the tropical oceans due to changes in the environment (e.g. Schumacher et al 2004; Henderson et al 2018; Stephens et al. 2018; Wodzkicki and Rapp 2020). During La Nina the MCSs are usually more isolated and less intense. This will likely have an impact on the observed cirrus cloud fractions. Is there a reason only one end of the ENSO spectrum was considered here? Does this case study limit the sampling of the structures?

Schumacher, C., R. A. Houze, and I. Kraucunas, 2004: The Tropical Dynamical Response to Latent Heating Estimates Derived from the TRMM Precipitation Radar. J. Atmos. Sci., 61, 1341–1358

Henderson, D. S., C. D. Kummerow and W. Berg, 2018: ENSO influence on TRMM tropical oceanic precipitation characteristics and rain rates. J. Climate, 31, 3979–3998

Stephens, G. L., and Coauthors, 2018: Regional intensification of the tropical hydrological cycle during ENSO. Geophys. Res. Lett., 45, 4361–4370

Wodzicki, K. R., and A. D. Rapp, 2020: Variations in Precipitating Convective Feature Populations with ITCZ Width in the Pacific Ocean. J. Climate, 33, 4391–4401

3. To aid the user, how much data needs to be averaged to obtain a representative heating profile? ANNs can give a statistically representative answer, but it might take some averaging to remove the random noise. How much data, spatial and temporal, need to be averaged to remove random error and get an accurate result?

4. Sec 4.2: Using warm regions and cool regions is a good way to initially separate these, but I would be careful with relating differences based on surface temperature. Other main factors, such as local environment and dynamical influences will also need to be considered. For example, MCSs in the West vs East Pacific are quite different in both surface temperature and structure due to thermal forcing in the West Pac and more dynamical forcing in the East Pac due to strong SST gradients. Further, as mentioned above (and in Section 4.3), ENSO can play a large role in the shape of MCSs due to changes in environment and regional dynamics (e.g. Schumacher et al 2004; Henderson et al 2018; Wodzkicki and Rapp 2020). Are the two surface temp (300K vs 302K) categories here consistent in the way MCSs would be initiated? Would isolating the same comparison to a similar region yield similar differences in characteristics?

5.

Section 4.3 I do not think this data record is long enough to make a significant regression analysis. It is OK that these results are here, but a stronger statement on how these results seem to be linked to changes in the ONI+PDO needs to be made and that it could change with a longer record. Do the regressions change if you break up the time periods (e.g. 2003-2012; 2007-2015; 2011-2018)? If significant regressions cannot be found the observed change with surface temperature is more likely due to natural variability. Adler et al (2017) stated that natural variability is too large to make statements on temperature and data periods longer than 30 years are needed.

Adler, R. F., G.Gu, M.Sapiano, J.-J.Wang, and G. J.Huffman, 2017: Global precipitation: Means, variations and trends during the satellite era (1979–2014). Surv. Geophys., 38, 679–699

6. The MCSs are defined using the presences of UT clouds and a convective core. How do you deal with cases were an MCS extends through multiple boxes? How do you ensure that cirrus is not associated with a nearby MCS and in proximity to isolated convection?

7. The usage of supplement material needs to be streamlined somehow. There is a lot of material overall and at some points this feels like two papers that have been pushed together: one outlining the retrieval and performance and another applying the data. There is a lot of back and forth between the manuscript and supplementary material and supplemental figures seem too incorporated into the material. An example of this is the comparison of Fig. 5 and Fig. S7 or the additional information in S12 and S13. The authors compare the shapes of the heating profiles and it requires bouncing back and forth between the Supplemental and normal figures. It is described in text, but it is more useful to see the visual comparisons. Some of the Supplemental figures need to be added to main text if referenced (e.g. S12 or S13). Perhaps discussion on the performance could be added to the supplemental pages and then have the readers sent to supplemental to learn more.

Minor Comments

Line 237: When describing the case sensitivities, it is hard to follow. A table might be easier to visualize.

Line 255: For future analysis, converting to something like sigma vertical coordinates may help mitigate this issue.

Table 2 with MAE: It is hard to understand the magnitude of the error here. What is mean heating compared to the error? Fig S4, gives a slight example, but examples in the text would be useful.

Line 375: “The small cooling around 550 hPa is due to melting” – evidence for this?

Section 4.2 I would remind the readers here that this data is much longer than other vertically resolved datasets.

Figures: Differentiating solid vs broken lines is difficult in the legends.

Line 544: Is T < 210 K cloud top temperature?

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