Atmospheric Measurement Techniques Discussions

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Comment on amt-2022-49

Anonymous Referee #2

Referee comment on "Latent heating profiles from GOES-16 and its impacts on precipitation forecasts" by Yoonjin Lee et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-49-RC1, 2022

Summary: This study presents a new method for estimating latent heating (LH) profiles from geostationary radiances, and compares the result with established methods that use NEXRAD ground-based radar and TRMM and GPM spaceborne radar. The methodology for estimating LH is similar to what is used for TRMM/GPM LH profiles and is based on a database of output from a convection-permitting resolution model. The authors find that the GOES-based LH estimates are similar to those obtained from NEXRAD and GPM, and produce similar (positive) impact on model forecasts when used in model initialization.

General comments: The use of geostationary data to estimate latent heating is interesting, and potentially valuable, as the Geo data provides much more extensive spatial and temporal coverage relative to NEXRAD and GPM. I think this manuscript is publishable, but needs to be supported with quite a bit more explanation of the tools and datasets used, and also should contain additional context and caveats. I would like the authors to consider the following general recommendations.

1. Any LH estimate from remote sensing is by nature indirect - the observation is of the result of a process that involved LH, not of the LH itself. For example, there can only be hydrometeors for the radar to observe after the condensation process has already happened. A change in the cloud top brightness temperature can only happen after the air has arrived at the top of the storm (having already gone through the condensation process). Please comment on this - I think there is a significant unanswered question that relates to the time and space disconnect between an observation of the result of LH and the LH itself.

2. Convection is identified using time sequences of GOES imagery, yet the LH profiles are binned by the magnitude of the cloud top brightness temperature. It seems to me that an interesting and more direct comparison could have been made between the simulated LH and the simulated time-difference brightness temperature. Please discuss.

3. An obvious point of concern in any model-based lookup table is the model construction and configuration. A 3 km horizontal grid spacing is barely convection permitting, and most simulations of deep convection meant for scientific analysis are now conducted at grid spacings of 1 km or less (most often smaller than 250 meters). Studies comparing simulations with sub-1-km grid spacing with those run at ~3 km have consistently shown that updrafts in 3 km grid spacing simulations are too wide and often too strong, and that the latent heating distribution is shifted higher in the coarse resolution runs relative to the fine resolution runs. In addition, studies have also shown that the LH position and magnitude are very sensitive to the details of the cloud microphysical parameterization. I have a number of questions that I would like the authors to address:

- Why did you not run the WRF model at finer grid spacing? Even if this was not computationally feasible, at least one simulation should be run at fine grid spacing and the LH characteristics compared to assess sensitivity.

- What was the sensitivity of the simulated LH to choice of microphysics? I do not expect a detailed study of this, but as with the previous question, one could imagine running companion simulations of the same case, one with Thompson microphysics and another with (for example) Morrison or WSM6. This would at least provide a first order estimate of the sensitivity.

4. There was not enough detail provided about the simulation database itself. I was missing the following, which the authors should provide:

- What were the lengths of the simulations (in time)?

- What were the geographic domains?

- Which data was used for initial and boundary conditions?

- What was the model output time frequency?

- How many vertical layers were used? (this can have as large or larger effect on the convection than the horizontal grid spacing)

- How were the simulations validated? How did the authors ensure they provided a reasonably realistic depiction of storm structure?

- Did the simulations span a range of convective types (size, longevity, mode of organization)?

5. The various LH estimates seem to reflect different sources of information on LH. For example, NEXRAD is sensitive to large hydrometeors and primarily obtains information from the lower portions of the troposphere. As such, one would expect the NEXRAD estimates to be biased toward the lower portion of the storm and miss LH in the middle and upper portions. TRMM/GPM radars operate at a shorter wavelength - they will see more of the smaller hydrometeors higher in the storm and may miss some of the heaviest rainfall due to attenuation). One would thusu expect their information to come from the middle portions of the storm but perhaps miss the very lowest and highest layers due to missing detection of heavy rain and small cloud particles. Geostationary data only sees the change in cloud top properties - it's not clear which portion of the storm produces the change at cloud top, but it is likely weighted toward the middle and upper portion of the storm. I would like to see the authors comment on this, and to perhaps discuss how the three sources might be merged in those instances where all three view the same place and time.

6. There were no caveats listed in the conclusions - one would expect that there are places and times where the GOES data might provide a more reliable estimate of LH and others where these estimates will have larger errors. What are these? Also, there was no mention of future work - what is next? This should also be discussed in the conclusions section.

Specific comments:

1. June 2017 (the case used to assess impact) is within the time frame used to run the WRF simulations that form the database of profiles. In testing a database-based method, it is common to test on a case that lies outside of the training dataset. I wonder what the results would look like if you compared the estimates for a month from 2019?

2. It was clear that there are discrepancies between the NEXRAD and GOES detections of convection. It would be interesting to see statistics on how often these discrepancies occurred.

3. The scatter in the plot comparing GOES vs NEXRAD LH in Fig 8 is very large. It is surprising that the correlation was ~0.8. I wonder if the relationship is more obust for smaller LH values than for larger? I suggest using log-log axes for Fig 8 to better be able to examine the smaller LH values.

4. The phrasing in lines 560-560 on page 16 is confusing - it makes it sound like you are replacing the observed LH with the LH from the model. I think that what you are doing is

inserting the observed LH into the model (replacing the modeled LH), right?

5. Follow-up question - are you inserting the observation-estimated LH *profile*? If so, the NEXRAD profile would be bottom heavy while the GOES profile would be top-heavy, right? This would explain the precipitation differences, I would think... NEXRAD LH would produce warming lower in the troposphere, which should result in a much larger effect on buoyancy, relative to GOES.

6. While the magnitudes are similar between NEXRAD and GOES estimates of LH, the position of the peak in the vertical matters quite a bit for large scale dynamics. How has this discrepancy been addressed in the literature? Is it assumed that NEXRAD is biased low? Is CSH (and by extension GOES) biased high?