Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2022-49

Anonymous Referee #1

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-RC2, 2022

Review for AMT of Lee et al. manuscript entitled "Latent heating profiles from GOES-16 and its impacts on precipitation forecasts."

General Comments:

Latent heating (LH) is an important process-level cloud variable. To address the temporal gap in LH observations (arising due to satellite estimates only being available periodically), the authors develop a new GOES-based LH retrieval. They compare their GOES LH retrievals with existing satellite and NEXRAD estimates, and then demonstrate the impact (on precipitation forecasts) of assimilating the new LH into WRF. The analyses of impact on precipitation forecasts are fairly minimal (skill scores are shown for a few boxes for a few forecast hours), so this part could be thought of as a proof of concept for using GOES LH in WRF.

My major questions are:

- Why is only 1 month of data compared across the products? Can additional months be aggregated with some averaging over connected convective features to remove the substantial noise that shows up in instantaneous pixel comparison plots (e.g., Fig. 8)?
- There is a lot of discussion of this quantity: total LH. As best I can tell, this is not an average convective LH, nor is it a vertical integral. It is a sum of convective LH profiles in a box. I did not understand the purpose of just showing the sum, which is largely a result of summing profiles over different convective area counts (and the convective area counts are product dependent, being different for NEXRAD, CSH and GOES). The average profile structure and convective area differences should be portrayed and analyzed separately. If my understanding of total LH computation is incorrect, then it needs to be clarified through the manuscript.

- The analysis of the impact on precipitation forecasts is pretty minimal, and as a reader, I was not convinced that the abstract text stating "improving the forecast significantly" is warranted quite yet. I would re-phrase the text in parts to suggest that this is more of a proof of concept or demonstration of the potential value of assimilating LH.

I did not comment on grammar, but I emphasize that a comprehensive read-through is needed to correct many sentences for English/grammar structure. The content overall is appropriate for AMT. My comments above and specific comments below probably warrant a recommendation of major revisions, after which the article -- whose topic is important and intersesting -- will be more useful to the community.

Specific Comments:

First line in abstract: It is unusual to say LH is the essential factor driving convective systems. It is also a product of convection. I would reword to say it is an essential factor connected to convective system circulations.

Line 30: convection is definitely not resolved explicitly at a few kilometers. Over a decade has passed since the 2011 paper cited, and this is appreciated even more. Perhaps mesoscale convective effects become resolved, but at this resolution, convection is "permitted."

L72: recommend rewriting first part of sentence to: "These products provide instantaneous heating estimates, but their temporal resolutions are low compared…"

L75-80: Can this sentence by written differently? I'm not sure what is trying to be conveyed as written: "Cloud top information from geostationary data is included when creating cloud analysis during data assimilation (Benjamin et al., 2016), and thus LH retrieved based on cloud top temperature, can be useful in the forecast model by keeping consistency of retrieved LH with the updated cloud analysis."

L320-322: details about converting units is probably unnecessary information in the article: "but provided in different units. LH from GOES-16 and NEXRAD are in K/s to easily match with modeled heating rate, while DPR products are in K/hour. Therefore, LH in K/hour from DPR products are converted to K/s for comparison."

L352-353: what is the reason for interpolating to the WRF grid at this point? WRF will rarely get the convection in the exact right place at the right time as observations due to a different surface relative to reality, so why this is done is not clear to me.

L369-370: changes in buoyancy are related to the vertical derivative of heating, not absolute heating rate at any level. If heating is increasing with altitude, then there is a dampening effect on buoyancy specifically. So, it is not about buoyancy here; convection can be initiated because LH increasing with height induces surface convergence which favors convective initiation (which the authors happened to mention near the introduction near L34). Clarify this statement.

L374-376: some papers using ARM radars (papers led by Die Wang) indicate that 40 dBZ is a good proxy for convection overall. From that perspective, 28 dBZ is too low too.

L450-459: I do not understand Fig. 7. For total LH, are you simply summing up all the LH for each convective pixel? So, if for example there are 100 convective pixels, the total LH is the straight sum across all 100 and not an average? Or is total LH some combination of convective for every product + stratiform from CSH? In the literature, total LH as typically reported as the combination of all convective + stratiform + non-raining + anvil LH. If Fig. 7 "total" (and Table 4) is calculated simply by summing convective LH, then I do not understand the value of this figure. It would largely be reflecting differences in convective area and not LH profiles since the previous figures suggested similarity in LH profiles for GOES and CSH. Instead of summing LH, I would strongly recommend showing the convective area differences for each box and product, and then the reader will be able to infer an overall difference in LH as a combination of both convective area differences as well as profile structure differences.

L510-511: initiating convection certainly depends on structure and not "total" heating. Is there a reference to support that the impact on initiating convection is similar if the total heating is similar? And, again for clarification, what is meant by "total" here – column integrated, or the sum of all individual convective heating profiles?

L512: the text: "and some conditions that are used…are applied before the comparison to be useful for the real application" is very vague. I do not know what this sentence means.

L529-530: there is a lot of scatter in Fig. 8, and I am uncertain again on what is being shown: the vertical integral ("total") or different levels all combined into a scatterplot of the "total" LH shown for convective regions of Fig. 7? Perhaps the authors should think about doing some averaging of convective LH over the identified convective regions for each rainfall system and then aggregating those convective-system averages and comparing? There is almost too much noise to infer anything from Fig. 8, despite the 0.83 correlation.

L565-600: Fig. 9, NEXRAD (9c) shows convection in the yellow box, but it disappears when GOES LH was used. Why? I recommend commenting on this. Also – is convective LH assimilated everywhere or only in the 4 boxes?

L670-672. “Even though one might think… it is actually more than just one brightness value.” This sentence is too informal for a publication, and should be re-written.