Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC3
https://doi.org/10.5194/amt-2021-97-RC3, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on amt-2021-97

Anonymous Referee #2

Referee comment on "Latent heating profiles from GOES-16 and its comparison to heating from NEXRAD and GPM" by Yoonjin Lee et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-97-RC3, 2021

Review for AMT of "Latent heating profiles from GOES-16 and its comparison to heating from NEXRAD and GPM" by Yoonjin Lee et al.

General Comments:

Latent heating (LH) is an important process-level cloud variable. LH retrieval, over the past couple decades, has been largely limited to TRMM, GPM and ground-based radar/NEXRAD (the latter more recently). TRMM and GPM have tropics-wide views, (or near global views for GPM) but also have infrequent revisit times, and thus the temporal resolution of LH retrievals is typically on the order of days. Conversely, ground-based radar retrievals offer increased temporal sampling, though data are not available globally. This paper recognizes the above limitations and aims to develop a retrieval of LH that allows for increased temporal resolution for LH data over a large spatial domain. The retrieval uses GOES input data (and though not stated, the algorithm developed potentially could be applied to multiple geostationary datasets.)

The topic is very appropriate for AMT, and successful LH retrievals will be of use to both the NWP and cloud physics research communities. However, the paper as written requires major revisions largely due to a need for a) more substantial statistical evaluation of the LH retrieval beyond the few cloud snapshot samples discussed; and b) improved clarity of presentation and improved grammar/sentence structure throughout.

Relevant to major revision comment a) above, the authors need to do a much more thorough analysis of the LH retrievals (either via comparison to both NEXRAD and GPM, or just comparison to either NEXRAD or GPM). For example, how do the new LH retrievals compare to GPM CSH over (flat) land vs ocean vs mountains? How about for NEXRAD locations spanning different types of convective regimes? Or, how would results compare as a function of echo top height or surface rainfall rate? There are many ways to slice-and-

dice and/or to design an analysis for comparing LH retrievals. Either way, I do not think comparing a handful of cloud snapshots for one 2 deg domain at one time snapshot is sufficient for this publication. I also do not think a new intercomparison analysis would add so much new text and images so as to warrant rejection as the paper currently exists. Regarding major issue b), I strongly recommend a thorough read-through and correction of the English/grammatical structure. After noticing many issues with sentences in most paragraphs of the manuscript, I decided to not focus on grammar past the Abstract (see Technical Issues section below) and instead, I focused mostly on the science and retrieval aspects (see additional comments in "Specific comments" below).

Specific Comments:

Beginning on line 32 (Introduction) and discussion about using LH in NWP: The authors write that LH aims to increase buoyancy in the atmosphere. If one thinks of most deep convection as rooted in the lower atmosphere (or below 2 km), then buoyancy in the lower troposphere is most relevant. However, LH heats the atmosphere to a larger degree as one moves up – in other words, LH is larger at 5 km than it is at 4 km, and larger at 4 km than 3 km, larger at 3 km than 2 km, and so forth. If heating is larger aloft, then LH is doing the exact opposite: it is stabilizing the local atmosphere. And this is what we expect of convective LH – the "job" of convection is to stabilize the atmosphere. Thus, I recommend removing all science text about buoyancy being enhanced by LH as reasons for use in NWP. Instead, I would recommend using the arguments presented in the original conference pre-preprints on using LH in NWP:

https://ams.confex.com/ams/22WAF18NWP/techprogram/paper_124540.htm

https://ams.confex.com/ams/88Annual/techprogram/paper_134081.htm

Upon reading those, it is clear that LH – or a perturbation in heating above the surface – allows for increased local surface convergence and local upper-level divergence to be induced. The "forced" local surface convergence in the presence of an already conditionally unstable atmosphere – or, more realistic local vertical circulations – increase convection for regions that are already unstable or conditionally unstable. But, importantly, the LH itself is not causing buoyancy because the vertical height derivative of LH is positive ($dLH/dz>0$) up to above the melting level (and that differential heating weakens buoyancy).

Line 28: at a few km, convection can be approximately resolved or "permitted" – to resolve convection, one needs a few hundred-meter resolution simulations. See Andreas Prein et al. studies and anything more recent, for example.

Line 58: I believe melting level and the PR/DPR convective-stratiform flags are used for

LUT indexing, too?

Line 75: There are orbital level DPR products – they do not provide rapid revisit for any one location, but when they are available, they do not represent a temporal resolution of a day. They represent the instantaneous LH for that type of convection characterized by that surface rainfall rate, ETH, etc. *on average*, so it is not clear what is meant by DPR having a temporal resolution of a day.

Line 91: In Eq. (1), on LHS, should be Q1-QR, not Q1-Q2.

Line 100: the authors have already introduced CSH and SLH acronyms previously.

Lines 108 – 114: it sounds as if only one LH or Q1 profile is provided. CSH also provides convective and stratiform profiles separately since both convection and stratiform cloud types exist simultaneously in a given grid box.

Line 113: what is a "decreasing flag"?

Line 128 and Figure 1: I would suggest improving the color gradation in Fig. 1 so that 0 K/hr is not centered on red and white is not the largest negative heating value. If anything, I would think 0 K/hr might be more suitable for white. I find it very difficult to interpret that color scale and visualization shown.

Line 133: Of course, differences in models used to populate the CSH and SLH LUTs might indeed cause differences in heating, but the different LUT inputs also play a role, and it probably should be mentioned.

Line 224: Is there a reference to be added that supports the claim the LH in stratiform clouds is not important?

Lines 228 onward (first 2 paragraphs of 3.1): it sounds like the authors are arguing that one cannot use CRTM simulated brightness temperature for defining convection (can a reference be provided for this on line 240?). Instead, the argument is made that one must use vertical velocity to define convection. But then, in sections 3.2/3.3, the authors note that they use CRTM brightness temperatures (e.g., lines 266, 275). So, these two parts seem to conflict with each other, and I am confused therefore about what the authors mean when they say they cannot use CRTM brightness temperatures.

Line 250: an altitude of maximum cloud water maybe on average will be correlated with updraft strength, but not instantaneously, and particularly since water condensate falls and is lagged with respect to updraft momentum dynamics. And, overall, I am a bit confused on what the paragraph of discussion about water condensates is trying to convey to the readers.

Line 252: I do not really follow the sentence "Since vertical velocity…"

Line 264: what does a "stable mean LH profile" mean? Is this about sample sizes?

Figure 2: Please convert WRF pressure level to actual pressures (hPa) or heights (km); also, the temperature legend font looks slightly messed up (with question marks appearing after the first Kelvin units).

Table 3 – the maximum precipitation rate for 245K and 250K bins are lower than the mean precipitation rate. Typo?

Minor / Technical Issues:

Below are some small/grammatical corrections I noticed (in the Abstract only). However, as I noted in my general comments, there are many grammar-type issues needing to be addressed throughout the manuscript, and for this review, there were too many to keep track of, so I stopped correcting after the Abstract.

Line 11: suggest adding "mission satellites" after "…Measurement (GPM)".

Line 12: suggest remove "its"

Line 14: suggest rewriting "using it is that its" as "using these sources is that"

Line 21: suggest adding "the" after "of", and "the" before "Goddard".