Assimilation of lidar planetary boundary layer height observations

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Response to reviewer 1

Reviewer 1

Major comments:

1. L216-L225 and the assimilation impact from 00-08 UTC: I am confused why there appears to be exactly zero assimilation impact during the overnight hours. In Fig. 2, the forecast and analysis lines are exactly on top of each other for the first four verification times. However, looking at Figure 1, the innovation (observation minus background) can still be large during the overnight hours, especially for MYNN. For example, Fig. 1 shows MYNN underpredicting the PBLH by 300 m at 0400 UTC. Given that the error covariances at this time are also non-zero, as shown in Figure 7, I would expect at least some impact. Additionally, Fig. 2 shows the MYJ and MYNN RMS values of (T, Q, U, V) being exactly equal. This seems odd given that Fig. 1 shows them predicting very difference values of PBLH. Thus, I wonder if there is some error in the assimilation scheme or analysis techniques that could be leading to this appearance of zero-impact.

The analysis incremements are never zero, but are much smaller from 0-8 UTC. But we also found an inconsistancy in the definitions geopotential height and PBLH (the former defined above ground level and the latter above sea level), and have redone the assimilation. You can now see somewhat larger changes in some of the profiles during this time. Also, the smaller increments are also due to the variation in the lidar observation error estimates, which vary substantially during the day.

2. Figure 2: Given the large data gap between 08-22 UTC and the use of only six soundings for verification, I disagree with the use of a time-series to show the assimilation impacts. This choice leads to the appearance of the impact linearly increasing between 08-22 UTC, when it likely shows a very different shape in reality. Additionally, statements line L207 (water vapor mixing ratio has little impact until 22 UTC) are not correct given that there is likely an impact beginning at 12 UTC when the innovation becomes much larger. It is just that you do not have any radiosondes confirm that. I suggest removing this figure, or at least removing the lines that connect the verification times. I also suggest removing any text referring to temporal changes in the impacts C2

We have removed the lines between the radiosonde measurement times. It became difficult to see both of the PBL model forecasts without them, so we split Figure 2 into two figures (2 and 3). The text has been changed to reflect this.

3. Figures 3-6: These figures can be difficult to interpret given the lack of any innovation information. I found myself having to flip back and forth between these plots and Figure 1 to try and understand why the impacts were small at certain times. Please include the forecast PBLH on these figures, or at least annotate the innovation (Lidar PBLH minus forecast PBLH).

We have included the forecast PBLH in the profile plots.

4. Overly general writing: Sometimes I felt that the author's made general statements when those statements only were instead meant to refer to a specific PBL scheme. For example, it is stated in the abstract that assimilating PBLH observation improves water vapor relative to independent radiosondes. However, this does not appear to be the case for MYJ (figure 2). Additional examples of this are at L217, L228, L241, and L279. Please check and modify such statements throughout the manuscript.

The text has been changed to reflect the changes to assimilation (described in item 1 above), and to make the comments more specific. Though the L228 comment was concerning a plot that we didn't include. And L279 is a more speculative statement on how changing the state variables would be carried forward in time, though we have modified this to make it more qualified.

Minor comments:

L16 (and throughout): the use of "sonde" instead of "rawinsonde" or "radiosonde" feels a little informal. Please correct.

This has been changed.

L46: I suggest stating "non-local flux schemes" since that helps separate those types of schemes from the local TKE schemes.

done.

L50: The sentence beginning "These varying and distinct" is confusing. I suggest rewording.

It has been rewritten as: "The variety of definitions PBLH make it difficult to effectively evaluate existing models or develop new ones."

L58: I am not sure what the point of this reference to GPSRO is. This seems oddly specific and overly verbose. It could probably be removed.

Removed.

L73: Jumping from the discussion of ceilometers to lidars feels a little abrupt. Please improve the flow between these two paragraphs (i.e., stating something like "we use Doppler lidars as a proxy to determine the impact of assimilating PBLH from a network of ceilometers").

We have added further wording to make this transition less abrupt.

L74: Please provide a little more information on the brand and type of Doppler lidar used. There were multiple instruments employed during PECAN so it wouldn't hurt to be more specific.

This information has been added.

L81-L82 and L94-100: I suggest moving some of this content into the methodology sections. It doesn't really fit in an introduction.

We don't agree with making this move. These details are not about the assimilation algorithm, which is described in the methodology section. I don't think details about the observations belongs in methodology because the retrievals are not a part of the methodology developed in this work. Further, you are asking for more details about the lidar observations in this section already (L74 and L82). So it seems best to leave this the way it is.

L82: I would like to see more details on how PBLH is estimated from the Doppler lidar data instead of just giving the reference. This could provide needed context for understanding how different the estimates of PBLH are between the lidars, radiosondes, and the PBL parameterization schemes.

Additional description of the PBLH retrieval algorithm has been added.

Introduction: One thing I was curious about when reading this manuscript is the motivation for assimilating PBLH instead of directly assimilating the wind profiles collected by the lidars. Lidar wind profiles have been assimilated in the past with positive results shown (Kawabata et al. 2014, Degelia et al. 2020), so why go through the extra steps of deriving PBLH from those data? I suggest adding a sentence or two in the introduction to discuss this.

We have added "But we are interested assimilating the PBLH observations directly because the ceilometer network described above will focus on these retrievals, and satellite missions which measure PBLH are also planned.".

L116 and EnOI discussion: It seems that the EnOI computes the covariance structure with a spatial component (covariance over a given area). How representative is that of the EnKF method which can estimate covariance at a single point? Does that cause any issues with extrapolating these impacts to a hypothetical EnKF system (i.e., L269)?

We are only using the EnOI to compute covariance in the vertical direction, since we are concerned with the profile correction. With the EnKF one would also compute the horizonal structure as well. In addition, the variance estimate will dependend on the distance spacing of the profiles, with a larger distance resulting in a larger variance. We chose a relatively small set of 20x20 grid locations to minimize this effect. In the EnKF, one would also include inflation and horizontal localization. These would need to be worked out when an EnKF is constructed for this data type. We have added a couple of sentences on this.

L127: Is the same method used to compute PBLH for both the stable and convective boundary layer? I know MYNN is supposed to be more accurate at night compared to MYJ.

The PBLH estimate approaches are the same at all times. The values changed in this version as we found a inconsistency in the definition of PBLH, so now the MYNN scheme is more accurate during the night. We think the manuscript is reasonably clear on this.

L132: Please also list the grid-spacing for these simulations.

The grid spacing is 3km, and was already in the first version of the manuscript.

L111: Is there a reference for the NU-WRF forecasts run during PECAN?

The only reference at this point is Santanello, et al. 2019, which is an AGU meeting abstract.

L137-139: Is this true? I would expect that the covariance/correlation would be smaller when computed over a larger region?

Over a large region the meteorological conditions become more varied, so the variance becomes larger.

L148: Please include more information on the observation error variance! This term is equally as important in the analysis as the background error covariance. How is it determined? How do you convert the lidar wind errors into PBLH errors? Do you include any representation factors?

The PBLH observations are determined from the combined velocity variance dropoff, wind speed gradient and backscatter dropoff. The uncertainty is smallest where these values decay rapidly over a short distance. When the dropoff is more gradual (as in the morning), the estimated uncertainty is much larger. This is described in the text.

L151-155: It might be good to reference an EnKF paper for these approximations since it is the same technique applied here (i.e., Houtekamer and Zhang 2016).

Reference added.

L162: Please state the chosen value of α.

$\alpha = 8$ has been added to the paper.

L172-L179: Much of this paragraph detailing the model configuration is repeated from the methods paragraph beginning at L124. Please reduce.

We reduced the deails in the results section slightly.

L181: Is there a reference for the parcel method?

We added Holzworth, 1964.

L196: Why 800 hPa? Why not compute the RMS from the surface to the top of the PBL since you know its height? Please provide some justification for this number.

We chose 800mb because this is roughly the maximum height of the PBL on this day. If we chose to compute the RMS up to levels that vary with the PBLH, then it would be difficult to make direct comparisons between the RMS at different times in the day. We have added a sentence on this in the paper.

Figure 1: Should there be an additional sounding observation during the late evening? I only see five green triangles, but you reference six radiosonde launches. Additionally, there are six verification points shown in the time series plots.

The sixth radiosonde PBLH has been added to the figure.

L207: I recommend using absolute differences instead of percent changes.

We have changed this to absolute differences.

L213: I disagree with saying the assimilation reduces by the RMS "significantly". Is statistical significance computed here? Also, this sentence appears to be referring to the impact to U-wind in Fig. 2c, of which the impacts look extremely small to me.

We have changed the discussion here, and removed the term "significantly".

Figs. 2-6: Please add (a,b,c,d) headings to each figure to match the figure caption.

We changed the caption to read upper left, upper right, etc instead of using letters to identify the figure location.

Figure 2: I recommend changing (hour) in the x-axis to (UTC) to be consistent with the text. Also please be consistent between saying "U wind" and "zonal velocity" in the figure captions.

These have all been changed to "U wind".

L221-223, L276: I disagree with the statement of the model profiles "accurately" following the radiosonde profiles in Figs. 3-4. For example, the u-wind shows errors of 4 m/s, and the mixing ratio errors can be as large as 1-2 g/kg which is not exactly "accurate".

This has been changed so that the profiles are described as "more accurate" duing the early morning than they are in the late afternoon.

Figs. 3-6: I recommend reducing the vertical extent of these profiles you are primarily focusing on impacts within the PBL. Maybe 800 hPa since that is what you use for the RMS calculations?). Also, I notice that some of the axis labels and formats are different between these figures, so please be consistent.

We have kept the upper limit at about 600 mb because we felt it was important to show how the profile reveals the location of the top of the PBL (either the model or observation estimates), so it was helpful to include a region above the PBL. This also enables us to show how much correction is made above the PBLH. But we have made the fonts on the axis labels consistent.

L235-L38: I am not sure that the discussion of vertical localization fits with the rest of this paragraph.

We removed these two sentences. This had already been discussed in the methodology section.

L244: I do not understand this statement that suggests PBLH is more representative of water vapor flux. Please elaborate.

We removed the last two sentences from this discussion.

L279-282. There is a mix-up of tenses here. The first sentence uses present tense (the water vapor mixing ratio is over corrected), while the second sentence uses past tense (the assimilation corrected: : :). Please fix. I also noticed other instances of this so I recommend doing a pass to fix issues throughout the manuscript.

These sentences have been corrected.

Typos and wording changes

1. L5-6: Please spell out the affiliations.

Done.

2. L35-39: this sentence is overly long. Please split up or condense.

Done.

3. L42: Add a comma after "Alternatively".

Done.

4. L55: Please use UTC instead of "Z" time to be consistent with the rest of the paper.

Done.

5. L62: Change the reference to Hicks et al. 2016 to use parenthesis instead of brackets.

Done.

6. L114: The sentence beginning "Instead we use: : :" seems broken. Please fix.

Done.

7. L198: ntop is not used in this equation. Please remove.

Changed to $i = 8$.

8. L233: Fix the spelling for "independent".

Done.

9. L238: Please define "WV".

Done.

10. L267: Please change "assimilation" to "assimilating".

Done.

11. L288: Sentence beginning "The covariances" is broken. Please fix.

Done.