Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-238-RC1, 2020

Atmospheric Measurement Techniques Discussions

Interactive comment on "Assimilation of lidar planetary boundary layer height observations" *by* Andrew Tangborn et al.

Anonymous Referee #1

Received and published: 8 August 2020

This paper provides a nice proof-of-concept study to show the impact of assimilating boundary layer height observations collected by Doppler lidars. When assimilating these data using a system similar to a Kalman filter, the authors' show improved fit between NU-WRF analyses and collocated rawinsonde observations. The impacts are largest when assimilated during the late afternoon and smallest at night. The author's attribute this temporal shift in the assimilation impacts due to the model background correctly predicting the PBLH overnight (zero innovation). These results are also shown for two different PBL parameterization schemes, though the author's rarely draw any conclusions regarding their comparative performance.

Overall, I found the scientific goals of this paper interesting and something that AMT readers would be interested in. However, I have a number of issues with the inter-

Printer-friendly version

Discussion paper

pretation of the results. I am confused by the appearance of exactly zero impact to the evening/morning profiles given a non-zero innovation and non-zero covariances. I also do not agree with the presentation of time- series to show the assimilation impacts when there is such a large gap in data between 08-22 UTC. Finally, I request a large number of clarifications regarding the presentation of both the methods and the figures, as well as additional references for various statements. Given these comments, I recommend major revisions.

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.

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

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).

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.

Minor comments:

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

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

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

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.

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").

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.

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

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.

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.

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)?

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.

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

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

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

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?

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).

L162: Please state the chosen value of α.

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

L181: Is there a reference for the parcel method?

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.

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.

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

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.

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

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.

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

$\sim$4 m/s, and the mixing ratio errors can be as large as 1-2 g/kg which is not exactly "accurate".

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.

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

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

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.

Typos and wording changes

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

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

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

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

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

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

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

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

Printer-friendly version

Discussion paper

9. L238: Please define “WV”.
10. L267: Please change “assimilation” to “assimilating”.
11. L288: Sentence beginning “The covariances” is broken. Please fix.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-238, 2020.

Printer-friendly version

Discussion paper