

Interactive comment on “An ensemble Kalman filter data assimilation system for the whole neutral atmosphere” by Dai Koshin et al.

Anonymous Referee #1

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The paper presents the development of an ensemble data assimilation system that extends into the lower thermosphere. The background model is the Japanese Atmospheric General Circulation model for Upper Atmosphere Research (JAGUAR), and the data assimilation is provided by a 4D-LETKF. As there are few existing data assimilation models that extend into the lower thermosphere, the newly developed JAGUAR data assimilation system is beneficial to the middle atmosphere community. The paper is well written, with a clear explanation of the model, as well as examples demonstrating the performance of the data assimilation system. I recommend the paper for publication following some revisions. Specific comments are provided below.

1. Aura MLS measures temperatures, yet the results in Figures 2 and 4 use MLS observations of winds to assist in evaluating the specification of horizontal diffusion

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and gravity wave drag. Presumably the winds are gradient winds based on the MLS temperature/geopotential height observations. If this is correct, it should be clearly stated in the text that the results are based on gradient winds.

2. I have some concerns about the bias correction that is applied to the MLS temperature observations. The bias correction is determined based on comparing MLS and SABER temperature observations, and then correcting the MLS temperatures. Previous studies (e.g., Hoppel et al., 2008, doi:10.5194/acp-8-6103-2008; Pedatella et al., 2014, doi:10.1002/2014JD021776) took the exact opposite approach, correcting the SABER temperatures to the MLS observations. It is unclear why the authors adjust MLS to SABER, as opposed to what has been done previously. Could the authors provide some justification for their approach?

3. An additional comment concerning the bias correction is the fact that SABER observes different local times, while MLS is fixed in local time. My understanding is that the bias correction is not restricted to only times when the two satellites are co-planar (i.e., observing similar local times). Is it possible that some of the bias correction is related to sampling different local times, and thus represents true differences that should not be removed from the observations?

4. In the discussion of the different model and data assimilation parameters, the authors often refer to the selected parameters as “optimal”. Because the full parameter space is not (and cannot) be fully explored, I would recommend the authors consider revising the text since these may not be the truly “optimal” parameter settings. Rather, they represent the best among the settings that were tested.

5. The localization is performed using absolute distances. Although this approach makes sense in the horizontal, the authors may want to consider adopting a log-pressure based localization in the vertical direction. This is due to the fact that, for example, a 10 km height difference in the troposphere is much different than a 10 km height difference in the mesosphere. Well I recognize that it may not be possible to

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redo all of the simulations with a different vertical localization, this is something for the authors to consider in the future.

6. The localization length scale seems to be particularly large in the vertical direction. The large vertical length scales would mean that an observation will impact nearly the whole atmosphere in the vertical direction. Is this interpretation correct?

7. The authors should double check the equation for the localization in line 520. Is a minus sign missing? That is, should this equation be $R' = R \cdot \exp[-d^2 / (2L^2)]$?

8. OmF statistics are used in evaluating the impact of the assimilation window length. Because the error will grow during the forecast window, one would expect to have larger OmF for longer assimilation windows, which is exactly what is shown in the results. This is one reason for the OmF being smallest for a 3 h assimilation window, and largest for a 12 h assimilation window. The authors should acknowledge this aspect of evaluating the assimilation window length using OmF statistics.

9. In Figure 20, there is a horizontal line at 0.1 hPa, and it is stated in the text (lines 656-657) that “A thick horizontal bar shows the 0.1 hPa level up to which MERRA-2 pressure level data are provided”. However, the MERRA-2 results shown in Figures 20e-h include results above the 0.1 hPa level. Where are these results from?

10. Line 406: “Figure 2b” should be “Figure 2d”

11. Line 434: “Figure 4b” should be “Figure 4d”

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