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Interactive comment

Interactive comment on "Using a global network of temperature lidars to identify temperature biases in the upper stratosphere in ECMWF reanalyses" *by* Graeme Marlton et al.

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Reviewer 2: We thank the reviewer for their comments, our responses and proposed amendments to the manuscript are below marked by AR

This paper details temperature biases of two ECMWF datasets (ERA-interim and ERA-5) with respect to two independent observational datasets (ground-based lidar and satellite-based MLS). Biases are calculated from long-term comparisons of over 20years for four NH lidars andâĹij14 years for MLS, focusing on the "wintertime" period(October-March). In addition, lidar comparisons were made for Pre-2000, 2000-2007,and Post-2007 to assess impacts of inclusion of different satellite sources. The

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Discussion paper

results show a significant cold bias with respect to lidar in the upper stratosphere ofâĹij3-4K in ERA-interim and a warm bias above 3hPa, both of which are reduced in ERA. The paper is generally well-written, and the conclusions are sound. What is less satisfactory, at least to this reviewer, is the lack of analysis of how biases may vary with time over the reanalysis. The brief look at the three time periods is helpful, but it seems that there are interesting results to be “mined” in the datasets beyond the long-term means.

Specific Comments 1. The title may be a bit misleading. It says, “Using a global network of temperature lidars”, but only a very limited set of lidars (4) is actually used. Can this be considered “global”?

AR: Global will be removed from the title

2. It would be very interesting to include time series of differences, in order to have closer look at the data. At a minimum, you could do plots at a few different pressure levels with a data-point for each year of the time series used (monthly means could also work, as in Figure 3 of Simmons et al. (2020)). This would make the paper more scientifically interesting, for example to see whether there are trends in the biases, or whether certain years showed much larger biases than others.

3. I am wondering why the time-averages are limited to the “winter” months (although technically this includes late fall to early spring). No explanation seems to be given why these months were chosen, except that “stratospheric variability increases in these months and we examine whether this variability is present within the reanalyses”. Were similar biases not seen in the summer, or is there a reason not to look at the summer months?

4. It would be also interesting to analyze the seasonal variation in the biases by lumping together all years for each month. But maybe that is beyond the scope of this paper.

AR: The time averages were limited to winter months, initially to see if the heightened stratospheric variability was represented. We realise this is a shortcoming and propose, to include an additional panel in figures 1,2,3 and 4 showing a contour plot of the temperature difference by month for each station as well as recomputing the mean difference for all months. Addressing point 2 we will then create a time series of temperature bias at a selection of pressure levels as suggested by the reviewer to replace figures 5 and 6

5. There is not any discussion about how background error covariances may be influencing the results, as in Simmons et al. (2020). Maybe this is beyond the scope of this paper, but some understanding of how the data assimilation assumptions are impacting the results could be useful. Particularly, could different assumptions on ERA-interim and ERA-5 help explain some of the differences, or are they mainly due to the different datasets assimilated?

AR: Given the differences between ERA-interim (Dee et al 2011) and ERA-5 (Hersbach et al 2020), there are differences in both the vertical and horizontal resolution, model physics, data assimilation and observation streams. All of which could potentially explain and contribute to the results seen here. However, it is hard to imply from our analysis a single dominant component.

6. Page 4: The discussions of errors on this page is a bit confusing. It is difficult to tell whether the quoted errors are systematic, random, or some combination (e.g., RMS). The discussion of MLS errors did a better job of specifying errors as precision and biases. AR: We will work on the lidar section to define the systematic and random errors more clearly.

7. One minor question about the GPSRO observations. Although you quote that they are available up to 40 km, the impact seems to reduce the bias from 3 hPa to 0.5 hPa. Much of this pressure range is generally above 40 km. Is the improvement above 40km due to the improvements lower down impacting the upper levels via hydrostatic balance? Or do the analysis increments have correlation lengths that extend above the

observations? Some comment on this would be helpful.

AR: First a correction: the average assimilation height of GPS-RO is to 50 km within ERA5. In addition to this, the 50 km height is a tangent height and the bending angle is then integrated from the tangent point upwards to the model top so the introduction of GPSRO will have effects above 50 km (Healy 2008). In addition to this structure functions used in the data assimilation and the dynamics of the assimilating model used for ERA5 will influence the model levels surrounding it. This discussion will be added to the revised manuscript.

8. Page 5, Line 24: Could you also include the vertical range of these two reanalyses, both in model top and in what pressure range are the data made available? AR: ERA interim has a 60 model level range from ~1000hPa to 0.3 hPa. ERA5 has 137 model levels and a pressure range ~1000hPa to 0.02 hPa. However, the top 10 levels of ERA5 are not used to give similar vertical range to the comparisons with ERA-5, these values will be added to the manuscript

9. Page 5, Line 33: May want to mention which of the “more measurements” may have an important impact on this study. Also, what observations are you referring to with “improved bias correction techniques”

AR: Section 5 of Hersbach et al (2020) details the new and reprocessed data sets used in ERA-5 that were not present in ERA-interim. The revised manuscript will include a reference to this citation in section 2.3

10. Page 5, Line 34: What do you mean by “climate forcings”?

AR: Hersbach et al 2020. Section 6.1 describes CMIP5 radiative forcings. The revised manuscript will include this citation in section 2.3 as well as changing climate forcings to CMIP5 radiative forcings

11. Page 6, Line 19: Out of curiosity, it would be helpful to know how many profiles went into the means for each site, both for lidars and for MLS. Could these numbers be

included in each panel, or in the table?

AR: Profile counts will be added to table 1 in the revised manuscript.

12. Page 6, Line 26: Regarding the warm bias in ERA-Interim above 3 hPa, it would be useful to know whether the mean ERA-Interim profiles ever “turn over” to indicate the stratopause. That is clearly seen in the lidar, and is also seen in both ERA-5 and MLS.But maybe poor resolution of ERA-Interim at these levels makes it unable to capture the stratopause very well. If that is the case, then the large warm bias in ERA-Interim at high altitudes could simply be explained.

AR: Due to ERA-interim approaching the model top it would not be possible to show the stratopause overturn. We will add to this section remarks about the large warm bias being an artefact of approaching the model top.

13. Page 7, Line 33: You mention “uncertainties in the bias” as related to the increased width of the standard deviation. Maybe I don’t understand how exactly you’re calculating the standard deviation. Could you please provide some more detail? Is the smaller standard deviation for the lidar due to the longer time series used, which reduces the standard deviation of the mean?

AR: This will be reworded in the revised manuscript to say that the standard deviation in the temperature differences between MLS and ERA increase. This increase in standard deviation increases the uncertainty of what the bias is. Due to the large number of profiles for both datasets the profile count won’t affect the standard deviation.

14. Page 7, Line 36: Is the oscillation with height in the MLS data associated with specific MLS retrieval levels?

AR: The oscillation here cannot solely be attributed to the MLS retrieval levels. Some Oscillatory effects have been seen in Wing et al 2018. Wing et al 2018 showed large oscillations in temperature difference between MLS and Lidar that spanned several retrieval levels. This may explain the larger oscillatory behaviour at the top of the profile in figure 4. The oscillations with smaller vertical length scale with variability less than 1 K at lower altitudes falls within the quoted precision for that height range shown discussed in section 2.2. We will include this discussion in our revised manuscript

15. Page 8, Line 34: You say the profiles aren't affected significantly, but there are clearly some noticeable changes.

AR: This plot will be replaced with a temperature time series at several pressure levels for each site

16. The vertical profiles of differences are a bit confusing, particularly with respect to the placement of the model levels. I'm assuming there is a solid black line with red dots overlaid only at levels where the differences are significant. Are there also black dots at levels were differences are not significant? It is difficult to distinguish these two cases from the plots. Maybe removing the black line would help, just showing dots at each level, or making the black line thin so the black dots are more easily visible. As itis, it looks like there are large regions of insignificant differences in some of the plots, where that probably isn't the case

AR: Red dots were placed at levels where there was a significant difference. However, as the reviewer points out it is hard to distinguish the model levels where there is not a significant difference. In the revised manuscript we will change the plotting to better show the model levels and the significance of the temperature difference.

17. It was difficult to distinguish the colors in Figures 5 and 6, particularly which of the shaded regions corresponds to green and which to cyan. The overlapping of theshadings may be part of the confusion. Maybe an additional key for the shading would be helpful. Alternately, one could use thin lines with the same colors as the thick lines to indicate the boundaries.

AR: This plot will be replaced with a time series plot of the differences at several pressure levels in the revised manuscript

Technical Corrections 1. Page 2, Line 3: May want to define middle atmosphere

2. Page 2, Line 8: typo “ReAnalaysis”, also “Center” should be “Center” for NCAR.

3. Page 2, Line 12: may want to capitalize Earth to be consistent with a later reference.

4. Page 2, Line 17: comma after “data” should be a period.

5. Page 2, Line 19: May want to define upper atmosphere to distinguish from middle atmosphere.

6. Page 6, Line 16: Remove hyphen in “co-ordinate”

7. Page 6, Line 36: capitalize “Mountain Observatory”

8. Page 7, Line 8: May want to reword “Simmons et al. (2020)’s”

9. Page 7, line 24: Capitalize “Mountain”

10. Page 7, Line 31: Table Mountain Observatory is referenced as panel (g), but it ispanel (h) in Figure 4.

11. Page 8, Line 34: “does not seem to [be] affected”

12. Page 9, Line 9: Hyphen after “differences” seems misplaced.

13. Page 12: Line 12: Capitalize “Observatory”

14. Figure 6. Should “table mountain” be capitalized?

15. Page 11, Line 32: Need more details on this reference (e.g., URL).

16. Page 11, Line 35: Missing source of this reference.

17. Page 12, Line 23: The Kuo reference has no journal indicated.

AR: These will be addressed in the revised manuscript

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