

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.

Anonymous Referee #2

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General Comments

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 20 years for four NH lidars and ~14 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 results show a significant cold bias with respect to lidar in the upper stratosphere of ~3-4 K in ERA-interim and a warm bias above 3hPa, both of which are reduced in ERA-

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5. 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”?
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.
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

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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?

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.

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 40 km 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.

8. Page 5, Line 24ff: 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?

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”.

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

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

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the stratopause very well. If that is the case, then the large warm bias in ERA-Interim at high altitudes could simply be explained.

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?

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

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

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 where 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 it is, it looks like there are large regions of insignificant differences in some of the plots, where that probably isn’t the case.

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 the shadings 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.

Technical Corrections

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

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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 is panel (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.

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