

Interactive comment on “Comparisons of polar processing diagnostics from 34 years of the ERA-Interim and MERRA reanalyses” by Z. D. Lawrence et al.

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1 General comments

This paper presents a comprehensive comparison between two leading reanalyses, limiting its scope to a well-defined topic: the meteorological conditions impacting chemical ozone loss in the polar lower stratosphere. As explained already by the first referee, this study is timely and pertinent. Its main limitation is the absence of any comparison with independent observations, but the extensive diagnostics and detailed discussions nonetheless provide valuable information to all scientists interested in this field.

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While the text is generally very well written (especially the introduction), it suffers from the confusing use of the words "bias" or (worse) "relative bias" to designate the mean difference between a diagnostic extracted from MERRA and the same diagnostic extracted from ERA-Interim. I also have some concerns about the vertical interpolations in MERRA and the description of the trajectory calculations.

2 Major comments

2.1 P. 31366, lines 6-10

The absence of comparison with independent measurements is the main limitation of this study. It can not be justified by the scarcity of such measurements, because many observational datasets include temperature and are not used in data assimilation. Two important examples are

- the ground-based observations collected by the NDACC network: these include Lidar and ozonesondes (which also measure temperature);
- the satellite limb sounders such as UARS-MLS, Envisat-MIPAS or Aura-MLS.

Several comparisons between such instruments and meteorological analyses can be found in the literature. I think that such comparisons are simply beyond the scope of this paper, but this limitation should nonetheless be mentioned in the Introduction, with proper references to available datasets and published comparisons. I suggest to remind it in the Conclusion as well, because this is an important venue for further research in our field.

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2.2 P. 31367, lines 2-5

If I understand well, the Potential Vorticity from MERRA is interpolated from 42 pressure levels to the model levels, followed by an interpolation to the isentropic levels where all vortex diagnostics are calculated. It looks like significant information could be lost in this process, while vorticity could be derived from the MERRA wind fields which are distributed directly on the model levels. Please check that the 42 pressure levels have a vertical resolution similar to the model levels, and/or that the diagnostics derived from p-levels PV are sufficiently close to diagnostics derived from model-levels PV. It also looks like the vertical integration of A_{PSC} and A_{vort} (to V_{PSC} and V_{vort}) is done on the vertical grid of isentropic levels (p.31370 line 2). What is its vertical resolution? Using a grid coarser than the model grid could introduce unnecessary errors in the integration. If this is the case, are such errors negligible?

2.3 P. 31371, lines 8-16

This description of the calculation of trajectories is not sufficient to allow reproducibility of the results. Livesey (2013) is a simple web page which does not provide the source code, only a brief description and output datasets using (if I understand well) MERRA fields. It looks like Livesey (2013) included diabatic motion, but from section 3.4 we understand that it is not the case here. I think that the explanations on p.31379 lines 15-21 should be transferred here. Even so, some key questions must be addressed: Was LTD code fed with daily wind fields or more frequent analyses, e.g. 6-hourly? In the first case, explain why the errors due to daily update are negligible; in the second case, update the description of downloaded datasets in sections 2.1 and 2.2.

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2.4 Section 3.1

It is explained that in this paper, the word "bias" designates the mean difference between a diagnostic extracted from MERRA and the same diagnostic extracted from ERA-Interim. This is *very* confusing because in the context of analysis evaluation, the evaluation of the "bias" uses an observational dataset as reference and is a proxy for the systematic error present in the analyses. It is explained that the words "relative bias" are an attempt to clarify the concept. This makes the text even more confusing in my view, because in the context of analysis evaluation the "relative bias" is a dimensionless ratio between the absolute bias and some value representing the investigated quantity. The differences discussed in this paper, on the other hand, have the same units as the diagnostics themselves, do not use any independent observations and are not meant to evaluate the validity of either dataset. The typical reader first looks quickly at a paper, reading the titles of the figures and the sections to understand the scope of the paper. This choice of words will unavoidably lead her to believe that some comparison with observations is performed, while this is precisely not the case (see major comment 1 above). Furthermore discussing the units of a "relative bias" is totally counter-intuitive. I strongly recommend to replace throughout the whole manuscript, "bias" and "relative bias" by "mean difference".

2.5 P31373, line9; p31374 line 15; p31375 lines 15-16

Why are most figures shown at 580K (fig. 3,4,6,7) while fig. 5 is shown at 490K ? The text mentions repeatedly that several mean diagnostic differences depend on altitude, but this is not shown on any figure. I suggest to show this dependence explicitly for some well-chosen diagnostic, ideally through a vertical profile of this mean difference. For example p31375 lines 15-16: if below 520K there is no convergence towards better agreement, why not show it? It would be interesting to show a case where the

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disagreement persists even after 2002.

3 Minor comments and corrections

P. 31363, lines 1-12: consider adding some newer references.

P. 31363, line 27: is the word "myriad" really necessary?

P. 31367, line 13: replace words "In this case", e.g. by "Here"

P. 31368, lines 4-5: for clarity, mention already here the year of introduction of COSMIC GPSRO data in the reanalyses.

P. 31369, line 10 (also line 24): how is it possible to examine "daily" minimum temperature with only one instantaneous field per day (i.e. at 12:00 UT per sections 2.1 and 2.2)? Please clarify.

PP.31369-31371: section 2.4 is too long (especially taking into account major comment 3 and next comment suggesting an additional figure). Consider splitting it into "basic" diagnostics (up to P.31370 line 12) and "advanced" diagnostics (VTC, TT195, CT195).

P. 31370, lines 14-23: The definition of VTC provided here is new. It should be illustrated with a dedicated figure. I recommend "snapshot maps" showing situations with VTC close to 1 and ≤ 0 , for date(s) of special interest (e.g. the initialization dates of the trajectories used for figures 15 and 16). Since it will probably not be possible to distinguish the PV (and temperature) isocontours by both reanalyses, the figure could mention the two numerical values of VTC for each example.

P. 31370, line 27: vortex split and SSW are two related but different events. Do you mean here simply "vortex split events" ? Same for p.31371 line 26: do you mean e.g. "major SSW with a vortex split" ?

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P. 31373, line 5: provide a reference about this likely impact of AIRS data

P. 31373, line 17: "clearly demonstrate" - consider replacing by "clearly show"

P. 31374, lines 9-10: I do not understand "...below either threshold...". Please clarify.

P. 31375 line 19: "mixing of air ...and..." -> "mixing of air ...with..."

P. 31376 line 11: Is paper by Livesey et al. already submitted? If no, consider removing this reference as there is already one for this topic; if yes, please update the reference.

P. 31376 line 17-20: the blue and red lines on Fig.10 are so close that the differences can not be discussed there in a credible manner. Consider deleting these lines and discussing the differences directly with fig.11.

P. 31377, lines 8-10: static stability was not discussed in section 2.4. This sentence is not clear and seems not useful to me.

P. 31378 lines 4-8: These are indeed important caveats on the impact of vertical integration, time averages and smoothing errors. They should be mentioned in the conclusions as well.

P. 31379 line 9: "...ERA-I could bias model runs..." -> "...ERA-I could lead model runs..."

P. 31379 lines 15-21: move to end of section 2.4 (see major comment 3 above).

P. 31381 lines 1-2: It seems to me that very similar results between ERA-I and MERRA can not "lend confidence in transport calculations using winds from these two reanalyses". They simply show that both reanalyses were well constrained by the same datasets. The only way to have confidence in transport calculations is using independent observations (e.g. of chemical tracers).

Table 1: I do not understand the difference between "System" (ATOVS, TOVS), "Instrument" which flew on several satellites (e.g. AMSU) and "Satellites" (GOES)

Figure 2: I guess that the x-axis tickmarks and monthly labels are for the 1st day of

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each month. If this is the case, please edit the labels to "1Nov", "1Dec" etc. Same for figs. 6,8,10

Figures 3,7,9,11: these figures look very similar and one easily confuses them while reading the text. I suggest to add as figure title (bold font) the name of the diagnostic difference shown on the plot (as for most other figures).

Figure 4: too small, not readable. Please re-arrange the layout (the web page layout of ACP requires wide figures). Legend: please write the three values of sPV used to draw the vortex edge.

Figure 10: please indicate on the legends the units (fraction of hemisphere area)

Figure 11: please indicate on the legends the units (% of hemisphere area?)

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 31361, 2014.