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Comment on acp-2022-159

Anonymous Referee #2

Referee comment on "The impact of improved spatial and temporal resolution of reanalysis data on Lagrangian studies of the tropical tropopause layer" by Stephen Bourquet and Marianna Linz, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-159-RC2>, 2022

This paper analyzes how the temporal and spatial resolution of model input data impact the cold point temperatures sampled by Lagrangian back trajectories. The trajectory simulations and comparisons done using ERA5 data are innovative and provide important information for the topics related to upper troposphere lower stratosphere transport. I suggest publication after the authors address my comments and make revisions accordingly.

#1. The authors investigated the impact of the resolution of the input meteorological data on the cold point temperatures (CPT) sampled by trajectories. The conclusions are largely about how the distributions of CPT differ among different trajectory runs. This is useful information, however, it has its limitation since it does not show whether increased resolution results in an improved simulation compared to the real atmosphere. For example, which input resolution results in a more accurate CPT prediction and thus better water vapor prediction compared to the ERA5 water vapor itself or to the observation? The Lagrangian cold point temperatures are important for water vapor simulation, as stated by the authors, but the authors provided limited information (texts or figures) on water vapor predicted from the CPT. These are the major weaknesses of this paper. I highly recommend such analyses be added and discussed.

#2. It is confusing that the authors claimed their trajectories as domain filling DJF trajectories, while they only released trajectories on five consecutive days in February. Technically, there are no trajectories initialized in January or in December.

Some specific questions related to this:

How long does it take for a parcel to reach the cold point level from 400 K? Do most

parcels reach their cold point on nearby dates?

The trajectories are initialized from the end of February. Given a climatological heating rate of 0.2-0.4 K/day in the TTL in boreal winter (Fueglistaler et al., 2009), it may take a parcel 50-100 days to reach 380 K. Therefore, the CPT the parcels sampled may occur in December or January. Since the authors only released parcels from the end of February, the cold point sampling may be limited to a few nearby days in January or December (This is only a rough estimate). If so, the CPTs are not technically DJF CPTs.

#3. Figures 2-4:

Figure 2: There is no map or PDF for the 1-degree run in the left panel, though the texts mentioned it. Panel e shows a warm bias by the 1-deg run. The authors claim that the warm bias is due to horizontal temperature variability. However, panels a-c only explains the warm bias by the 0.5-deg run relative to the 0.25-deg run.

For the right panel in Figures 2-4, I suggest adding PDFs of predicted water vapor and comparison to ERA5 water vapor or observation (as discussed in the first comment).

The ticks in y axis for panel d in Figures 2-4 are not consistent.

4. How are the "collocated" CPT differences computed? This question can be applied to Figures 2-4.

1) Line 179: colocated-> collocated.

2) Does collocated mean at the exact same grid point or just nearby grid points? Is there any spatial interpolation for nearby locations when computing the difference?

3) Are the CPT differences computed for points collocated horizontally only or three-dimensional? For example, Lines 229-230. There are clearly differences in vertical distributions judging from Figs. 4f-g.

4) What is the percentage of trajectories that collocate their CPTs for each run? How about the CPT locations from different runs that do not collocate? How much do those non-collocate points contribute to the temperature PDF shift (This seems to be addressed only

for the temporal resolution experiment)?

5) For the horizontal resolution experiment, the authors ruled out the under sampling of the wind in causing the warm bias. Such a conclusion can be drawn if the authors confirm that they use CPT locations obtained from the 0.5 deg run and compare the temperatures from the 0.5 deg data and 0.25 deg data interpolated onto these CPT locations. Such a comparison directly tells whether the warm bias is purely caused by temperature variability in different datasets. But it is not clear how the difference in Fig. 2c is computed. Neither is there such calculation for the 1-deg run, which shows the largest warm bias. Is there a line for the 1-deg run in Fig. 2d? The lack of information makes the authors' explanations less convincing.

6) Lines 186-192: "horizontal temperature variability" here the wording is a bit ambiguous and not consistent with previous texts. Do the authors mean temperature variability between different resolution data?

Summary and final thoughts for comment # 4: The method of the spatial-temperature difference analyses is not consistent or unclear throughout the paper, which makes their explanations of the temperature PDF shifts less convincing.

It seems from the paper, the CPT differences can be attributed to a) horizontal or vertical locational differences, b) given the same 3-D location, the temperature variability among different resolution data, c) differences in wind sampling. It would be helpful for the authors to include quantitative conclusions (for each of their experiment sets) on how much each factor contributes to the CPT bias in percentage and K.

I also suggest consistency in the text when referring to temperature variability between different runs and other terms alike.

#5. Lines 290-291 vs Lines 295-296. The conclusions here stated seem to contradict each other. Lines 290-291: Do the authors mean far too large of a fraction of the lower resolution trajectories are traced to the stratosphere?

It would also be helpful to reference the figure number in these two paragraphs.

Minor comments:

Line 126: The full title for CDO was not given.

Lines 148-149: Is the vertical velocity w ? Are the pressures and potential temperatures interpolated to the parcel's location or Lagrangianly integrated?

Line 152: ...relative humidity with respect to ice

Lines 154-156: Are new trajectories released daily for 20 days in January or just released on one day in January? Which 20 days?

Lines 310-316:

1) "warm bias ... 0.5 K and 1.4 K". vs Line 243: "...the mean cold point temperature for 1, 3, and 6 hour resolutions are 184.8, 185.2, and 186.2 K..." The numbers here do not match.

2) "the shifted temperature distribution for the 6 hour data results in a 26% increase in water vapor... from the cold tail)." This estimate is not discussed in the main text before the summary.

Lines 327-329: The fractions (0.58, 0.62, and 0.64) appeared in the summary but not in the main text. In section 2.3 the author stated that the trajectories for the dispersion experiment are released and tracked in January 2017. However, this paragraph is not consistent with those ("DJF 2010 to 2019"). Table 1 shows that the DJF 2010-2019 run is done for 1 hour resolution only.

Lines 337-338: The authors actually can evaluate the water vapor predicted by the CPTs against ERA5 water vapor or observation.

Comments on the summary section in general: The authors' summary of their results and the significance/impact of their findings is a bit repetitive and dispersive. For example, both the first and last paragraph of the summary section mentions that increasing horizontal resolution beyond 1 deg does not bring significant improvement. Another example is that the impact due to temporal resolution is mentioned sporadically and repetitively throughout the section.

References

Fueglistaler, S., Legras, B., Beljaars, A., Morcrette, J.-J., Simmons, A., Tompkins, A. M., & Uppala, S. (2009). The diabatic heat budget of the upper troposphere and lower/mid stratosphere in ECMWF reanalyses. *Quarterly Journal of the Royal Meteorological Society*, *135*(638), 21–37. <https://doi.org/10.1002/qj.361>