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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of temperature resolution on trajectory modeling of stratospheric water vapour" *by* T. Wang et al.

Anonymous Referee #1

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This study compares trajectories in the TTL calculated with three different temperature datasets. The focus of the evaluation is on the statistical evaluation of the minimum temperature along the trajectories as they cross the tropical tropopause, and the corresponding water vapour entering the stratosphere. It is shown that the overall humidity values and in particular the seasonal cycle and interannual variability are only very weakly sensitive to the choice of the temperature dataset. The objective of the study is well justified and the results are in principle relevant; however, important aspects of the paper are not well explained and/or conceptually fuzzy - as outlined in my comments below. Therefore major revisions are required to turn this study into a fully consistent and convincing paper.

Major comments:

A) Is this paper really about the impact of "temperature resolution"? First, the study only considers the vertical resolution aspect, and not the horizontal nor the temporal one. In partiuclar temporal resolution might also matter, but this is not mentioned in the paper. Then, "resolution" to me sounds very technical (e.g., like running a model with two different resolutions). But this is not exactly the problem, nor what you do. My point is that if the MERRA assimilation cycle was runnning with a model with higher vertical resolution, then the resulting field would not necessarily capture more of the, e.g., gravity wave signals because capturing them is not only an issue of resolution but also of the representation of the wave triggering mechanisms. I would like the authors to discuss more critically and explicitely what they actually do. I think it is good and relevant, but it is not well described by "resolution". Maybe then the authors might also consider rephrasing the title of their study.

B) Three datasets are used and I am perfectly fine with the first two of them: MERRA is used because it is "standard", available for a long time period etc. GPS is used because it is based upon independent observations with fine vertical resolution. My expectation is that this data set should be as close to reality as possible. But then why use the synthetically created MERRA-Twave dataset? I understand that this dataset would be valuable if we did not have GPS. But since we have GPS what can this dataset tell us in addition? I suggest to better motivate the use of this third dataset, or to focus on the analysis with MERRA and GPS only.

C) The discussion of the impact of atmospheric waves is insufficient. The general statement at the beginning of section 2.2.2 "Waves are underrepresented in reanalyses" does not make sense. Clearly planetary and synoptic-scale waves are/should be perfectly captured by reanalyses. It remains unclear, which part of the wave spectrum is considered here. Kelvin waves, gravity waves? When discussing the role of, e.g., gravity waves on the temperature field in the TTL, then maybe also the temporal resolution should be discussed. Six-hourly fields, from MERRA or GPS, cannot capture the temporal propogation / evolution of waves. This could potentially also affect the

minimum temperature along the trajectories.

D) The paper has not been very carefully written. Several sentences/formulations are unclear:

- p. 29210 line 11: what is meant by "finite resolution"? Every resolution is finite, do you mean "fine"? (This problem occurs in several places.)

- p. 29213 line 15: "the carrying methane" sounds odd to me. Not clear how the methane values are initialized in the trajectories.

- p. 29213 line 19: what is meant by "limited in the tropical 110-50 hPa"?

- p. 29213 line 28: "total diabatic heating rates from all sky": please explain this better.

- p. 29214 line 8: "not represented well in current coarse model levels": you probably mean "... in models with coarse vertical resolution"; but I think this is not really the point (see comment A): even with more levels MERRA would not correctly capture all gravity waves emitted from tropical convection.

- p. 29217 line 5: what is "the curly nature" of a temperature profile?

- p. 29220 line 4: "We see slightly drier air in GPS run expected"??

- p. 29220 line 20: The sentence "Note that ..." is too long, and it is not clear what is meant by "the two are strongly coupled".

E) p. 29215 lines 24ff: This is an interesting result, but it is not well discussed. How can this happen? How can MERRA be too cold at model levels (compared to GPS) but too warm in between? MERRA values in between model levels are calculated by linear interpolation and therefore I would expect that a cold bias at the model levels is "transferred" to the layer in between.

F) In section 3.1 I have a problem in understanding the selection of the trajectories. My impression is that trajectories are selected if they reach the 90-hPa level (this is

considered as the entry point in the stratopshere). This is fine with me, but this implies that (during the time period considered) some trajectories maybe don't reach the 60-hPa level. But then you determine FDP statistics up to 60 hPa! Does this not lead to a biased distribution? Should you not select trajectories that reach 60 hPa instead of 90 hPa?

G) p. 29218 line 19: The bimodal FDP distribution with MERRA data is interesting (Fig. 5). But in principle the distribution should be even more peaked! When using linear interpolation between model levels, then minimum temperature must occur exactly at one of the model levels. So the smearing out of the two peaks is an effect of the temporal resolution of the trajectory output. I assume that you determine the minimum temperature from 6-hourly values along the trajectories. Then of course it can happen that the time when the trajectory reaches the exact pressure of a model level is "hidden" (i.e., in between two times) and therefore the "real" location and value of the minimum temperature is missed. This indicates that the temporal resolution can play an important role, and I suggest that the role of temporal resolution (of the wind fields, of the trajectory output) is discussed in the paper.

Minor comments:

1) p. 29212 line 8: maybe a terminology detail: here you write about "resolved but underrepresented waves" - does this (see comment A) also indicate that your study is not mainly about resolution, but more about "effects of gravity(?) waves on the temperature field"?

2) Section 2.2.2 is very difficult to understand. If you keep this MERRA-Twave dataset in your study, this paragraph should become less technical (for the technical aspects the reader can be referred to Kim and Alexander 2013). Here the reader should be able to learn the general concept.

3) Comparing Figs. 5 and 7b: something is probably not correct with the scales of the FDP events. Values in Fig. 7b are about 4 times smaller, but in both cases they should

integrate to 100%.

Editorial comments:

- p. 29213 line 19: "Noted" should read "Note"

- p. 29214 line 9: should read "... that use an idealized parameterization of ..."

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 29209, 2014.