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Comment on acp-2021-884

Anonymous Referee #2

Referee comment on "Suppressed migrating diurnal tides in the mesosphere and lower thermosphere region during El Niño in northern winter and its possible mechanism" by Yetao Cen et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-884-RC3, 2021

This manuscript investigated the migrating diurnal tidal variability in the mesosphere and lower thermosphere due to the El Niño-Southern Oscillation, and the driving mechanism of this variability. This is one of the important issues related to the interannual variability in the MLT region. The authors showed the significant negative correlation between the residual of diurnal tidal amplitude in the MLT and the Niño3.4 index, and attributed this diurnal tidal variability to its tropospheric source forcing change, background wind effect, and the modulation of the gravity wave drag. Although this paper included some interesting results, overall I think that the paper only has decent scientific progress since it is already well established of the negative correlation between the SOI/Niño3.4 index and the DW1 amplitude in the MLT region. The analysis is a good start point but I think the results presented herein are incomprehensive. Additional analysis with deeper informative results is needed to justify publication in ACP. I will indicate a major revision for this manuscript and think this manuscript can make an excellent contribution after major revision.

Major comments:

- **Data and method**
  - The archived model data from latest WACCM 6 and SD-WACCM-X version 2.1 runs are both publicly available on CESM website, with significant change from previous version. The authors should provide reasons why they chose an older version of model output.
- **Tidal forcing**
  - The author stated that the amplitude and phase of DW1 in the MLT could be potentially modulated by the ENSO and used a DW1 vector amplitude to combine their anomaly related to the Niño3.4 index. I think it will be better to assess the ENSO impact on the DW1 amplitude and phase separately.

- Do the authors have an explanation why the negative response becomes much weaker at the height of ~95 km in Figure 2A (even positive correlated in the Northern hemisphere low-latitude region)? SABER data has a great quality at this altitude and the DW1 amplitude roughly maximizes at the same region. I therefore think the result presented herein weakened the conclusion in the manuscript. Also, if the change of the tidal forcing due to the ENSO phase is the main driver of the DW1 anomaly in the MLT region, the negative response in the SABER DW1 is likely to be coherently equal in height.
- In Lines 314-315, the authors averaged the DW1 heating rate with identical altitude in Pedatella et al. 2013, and drew an opposite conclusion (negative correlation) with the previous paper (positive correlation). However, the DW1 heating rate between 5-10 km in Figure 4 is weakly positively correlated with the Niño3.4 index. This result seems not consistent with the text in Line 314-315. I hope the authors can provide some more explanation to support their statement.

- **Effect of background wind**
  - Figure 5: It seems to me that the result is not robust enough to be an independent section. My main concern is the statistical significance. The coefficient is small (the mean value of R in the MLT is roughly equal to one in McLandress, 2002, DOI:10.1029/2001GL014551) and the climatological value of R from the WACCM should be included in the manuscript, at least in the supplement. I also think the authors should perform the F-test and assess the statistical significance, similar to the tidal forcing section.
  - Besides, it is hard to justify the change of R-value is the driver of the DW1 interannual variability; or the change of R is just related to the ENSO phase and has a similar trend as to the DW1 variability.
- **Effect of gravity wave drag**
  - The authors can make a great contribution in this section with a thorough analysis. For example, is slow or fast waves to contribute most to the DW1 variability? Besides, do the authors have reasons not to mention the frontally generated GW impact on DW1 variability in the present manuscript? The zonal mean GW forcing due to the frontal systems in WACCM is about a order of magnitude stronger than that from the convective GWs (Richter et al., 2010, DOI:10.1175/2009JAS3112.1). Apparently, the authors should be able to identify the impact from two different GW sources on the DW1.
  - I am a bit confused about the definition of the gravity wave "drag". Does this result imply the DW1 phase is modulated by the ENSO-related GW variation?
  - I also would like to suggest the authors may consider pulling Figure S3 and S4 into the main text and clarify the difference between GW forcing and drag, not just mathematical definition but moreover the physical interpretation (Lines 359-363).
- **Summary**
  - I find it quite unusual not to have a Discussion section in a manuscript. The authors may consider to add this section, particularly to provide a "big picture" perspective for readers and remind them the importance of your study.

Minor comments:

- **Introduction**
  - I am surprised that the authors do not refer one of recent work related to the ENSO impact on the DW1, Sun et al., 2018 (DOI:10.1186/s40623-018-0832-6).
  - Line 58, one important work is missed in the QBO-DW1 reference, Forbes et al., 2008 (DOI:10.1029/2007JA012737).

- Lines 78-79: the statement might be misleading. I think in Lieberman et al. (2007), they've stated the altered heating pattern result in a stronger forcing in DW1 component. Current text seems to suggest the solar heating rate is globally enhanced during 1997/1998 and caused the tidal forcing became stronger.

- **Data and Methods**
  - Line 115, CO2 -> CO_2
  - Lines 175 and 183, F107 -> F10.7
  - Lines 194-205, Which numerical method is chosen to compute the Hough functions?
- **Results**
  - Few more words for the definition of El Niño and La Niña.
  - Line 220: I cannot find Zhou et al. (2020) in the reference.
  - Lines 243-244: How do you get these values (23%, 20%, 17%)?
  - Line 334: I do not see Wu et al. (2017) in the reference.
  - Figure 2: the colorbar unit might be wrong, should be K/index or unitless.
  - Figure 6: the unit should be m s-1 day -1 K-1 or m s-1 day -1 index-1.