Response to reviewers

Reviewer #1 (Comments to Author (shown to authors):

In general it's a thoughtful analysis of an important issue, the effect of El Niño on tidal oscillations in the mesosphere and lower thermosphere (MLT). It may be that this paper is more relevant to Annales Geophysique since, while it contains some detailed analysis of tropospheric effects, its ultimate intent is to understand the MLT. However, there is a larger issue- that of novelty. A recent paper by Vitharana et al., (JGR, 2021, doi:10.1029/2021JA029588) quite clearly states and demonstrates the anti-correlation between DW1 and El Niño.

And they both attribute similar causes. Thus compare: Vitharana "due to changes in tropospheric forcing" vs. Cen "heating rates in the tropical troposphere". And both analyze SABER data.

Certainly there are areas where Cen's analysis can be deeper than Vitharana so Vitharana should not be considered the last word. For example, while the negative correlation is now established, there are the relative roles of different components of the effect (heating, filtering by stratospheric winds, GW forcing) that the present work can contribute. Furthermore, Vitharana appears to misquote Pedatella and Liu, 2012 by saying that their results are consistent with that older reference. When in fact, I concur with the present authors in saying that Pedatella and Liu reached the opposite conclusion. But this present submission should be reworded and re-oriented to be following Vitharana's analysis. This probably means more work on "fleshing out" the details of the causes, for example the GW effect (which seems pretty clear in Figure 6). Their conclusions presently seem more like a simple listing- but I think they could, and should, give more information on the relative importance- perhaps one cause is more important at one altitude for example? (relevant to 4th and 5th bullets below)

1. I do not see where Ramesh showed a positive correlation between MLT DW1 and El Niño as stated on lines 94-95. Ramesh had lots of "predictors" and it wasn't clear what was forcing what. Perhaps the authors could clarify if I've missed something.

Response: Thanks for your suggestion. As added in lines 101-105 in the revised manuscript, "As suggested by the WACCM version 6 simulations with self-generated QBO and ENSO, Ramesh et al. (2020) illustrates the linear response of latitude-pressure variation of DW1-T to the seven predictors including ENSO in four seasons. They suggest that the response of DW1 to ENSO is significantly positive in the equatorial MLT region during the NH winter (Figure 5 in Ramesh et al. 2020)."

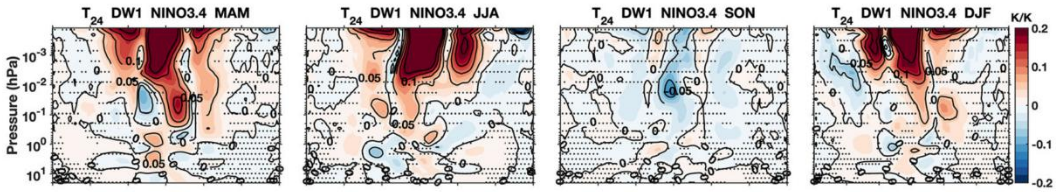

Figure R1 (Figure 5 in Ramesh et al., 2020): The seasonal variation of latitude-pressure distribution of ΔT24 responses to Niño3 averaged for three WACCM6 realizations. The responses in stippled regions are not significant at the 95% confidence level ($p > 0.05$). Contour intervals = 0.05 K/K, (The fourth row of figure 5 in Remash et al., 2020).

2. There is not a clear statement as to what SABER shows for the overall structure of the tide compared to WACCM. Do the authors agree with Vitharana? In which case, they can just state that, but also refer to the relevant figure in Vitharana. This is relevant to the 4th bullet below.

Response: To compare the distribution of the climatological mean DW1 tidal amplitude of SD-WACCM simulation with SABER observation, Figure R2 (Figure 1 in the revised manuscript) shows the average DW1 temperature amplitude in SABER observation and SD-WACCM during the winter from 2002 to 2013. The boreal winter (December-January-February, DJF) mean amplitude of DW1 temperature is the largest (~12 K) in the equatorial mesopause region extracted from TIMED/SABER observation. Although the mean amplitude in SD-WACCM is weaker than that in SABER, the distribution of the DW1 T amplitude in SD-WACCM simulation is quite similar to that derived from SABER observation, with the maximum at 90-100 km above the equator. There are some differences between SABER and SD-WACCM: SABER has a weaker peak above the equator at 70-80 km, but this peak cannot be seen in SD-WACCM. The interannual variation of the equatorial MLT DW1-T amplitudes weighted by phase difference in SABER and SD-WACCM also agree well during the northern winter of 2002-2014 (Figure 2a and 2b in the revised manuscript).

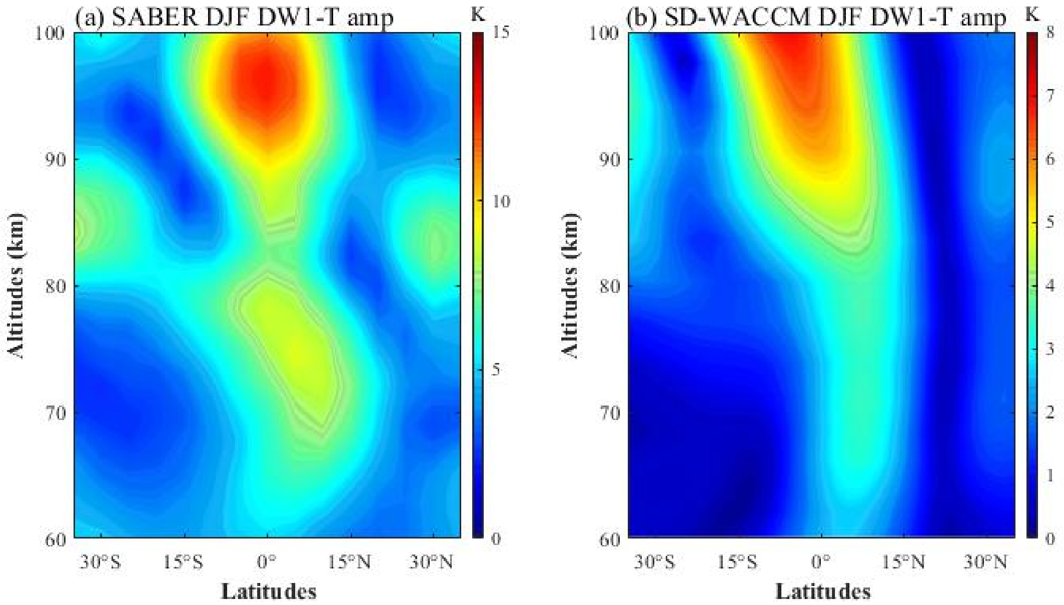

Figure R2 (Figure 1 in the revised manuscript). (a) The average DW1 temperature amplitude of SABER observation during 2002-2013 winter (DJF, Dec-Jan-Feb). (b) the same as (a), but for SD-WACCM.

As mentioned above, Vitharana et al. (2021) have stated that the MLT DW1 negatively responds to ENSO by calculating the multi-linear regression for all the months from 2003-2016. However, it is noted that the responses of MLT DW1 tide vary quite a bit to ENSO among different seasons (e.g. Zhou et al., 2018; Kogure et al., 2021). Thus, calculating the regression by binning the data among different months together may underestimate the actual response of MLT DW1 tide during the particular season due to the masking between different responses with each other. Indeed, the negative responses of MLT DW1 to ENSO during the winter are approximately five times larger than those estimated from all the months' data (Vitharana et al., 2021), while the location of the most significant responses is also different. It should be pointed out that the MEI indices were "normalized" by setting the maximum value to 1 in Vitharana (2021), which is different from the method we used (set the standard deviation to 1). If we also adopt the same method as Vitharana et al. (2021) did, the regression coefficients of MLT DW1 amplitude response to ENSO in winter (Figure R3) is over -3 K per index at 90 and 100 km, while the maximum of regression coefficients in Vitharana (2021) is only -0.6 K per index at the same region. The comparison between our results with those of Vitharana et al. (2021) has been rewritten in lines 82-93 and 248-253 in the revised manuscript.

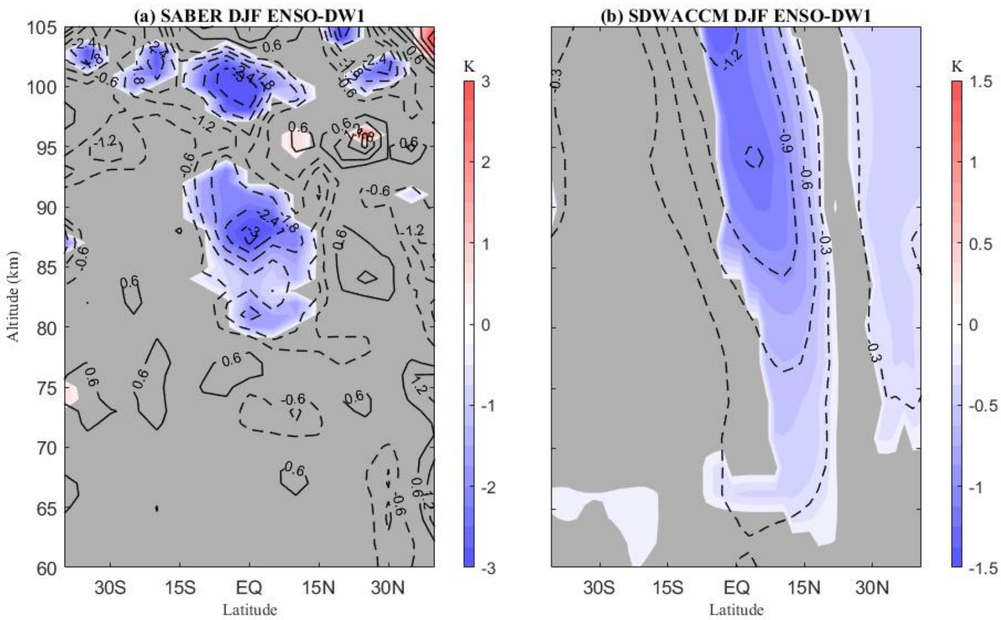

Figure R3. The linear regression coefficient of normalized Niño3.4 in SABER (a) and SD-WACCM (b) DW1-T. The contour interval is 0.6 K for SABER and 0.3 K (N3.4) for SD-WACCM. Red represents positive response and blue represents negative; the gray regions denote confidence levels below 95% for F-test.

3. I notice the authors use WACCM4, not WACCM6 which is the latest. While this is probably acceptable, they should at least note this and offer any comments on possible differences. For example, WACCM6 uses a self-consistent QBO (which might allow for better characterization of feedbacks?) and a different (better?) GW scheme as well as higher spatial resolution.

a. I'm not sure I fully understand line 249, but it does seem to speak to the question of feedbacks between QBO and ENSO which, if so, is relevant to the question of the WACCM model version number. Can they clarify?

Response: Thanks for your suggestion. WACCM is a high-top model that can be used as the atmospheric component of the Community Earth System Model (CESM1) of the National Center for Atmospheric Research. WACCM4 is based on the Community Atmosphere Model, version 4 with the vertical model domain extended to ~145 km.

The "Specified dynamics" version of WACCM4 is based on WACCM4 and nudged to meteorological fields from Modern-Era Retrospective Analysis for Research and Applications (MERRA) reanalysis data in the troposphere and stratosphere (from the surface up to 1 hPa) and then is freely run in the MLT (above 0.3 hPa) (Kunz et al., 2011). With the relaxation, the atmospheric variables such as QBO are consistent with the reanalysis in the troposphere and stratosphere.

WACCM6 is the latest version of WACCM in CESM2, transitions to higher horizontal and vertical resolution, which can simulate finer structures in the atmosphere. WACCM4 includes a representation of the quasi-biennial oscillation (QBO), achieved by relaxing equatorial zonal winds between 86 and 4 hPa to observed interannual variability (Marsh et al. 2013). Compared with WACCM4, the QBO in WACCM6 is self-generated (Gettelman et al. 2019). WACCM6 is able to simulate, albeit imperfectly, the QBO, the equatorial zonal wind propagation, which in observations has an average period of ~28 months. Similar to WACCM5 (Mills et al., 2017), WACCM6 simulates a reasonable QBO with 70 levels in a free running configuration.

Both WACCM4 and WACCM6 can be run fully coupled to active ocean and sea ice model components or use specified SST (Marsh et al., 2013). Pedatella & Liu (2012 and 2013) utilize the WACCM4 with self-generated ENSO and no QBO signal, while Ramesh et al. (2020) use the WACCM6 simulations with self-generated ENSO and QBO. Different from these two simulations, the SST which follows the observation is prescribed in SD-WACCM. And the SD-WACCM atmospheric variables such as QBO are consistent with the reanalysis in the troposphere and stratosphere.

As suggested by Figure 5 in Ramesh et al. (2021), the equatorial MLT DW1 positively responds to ENSO in the northern hemispheric winter, which is the opposite of that in the SABER observations and SD-WACCM simulations. The difference in generating ENSO and QBO among different versions of the model could play a role in the divergence in the ENSO-DW1 relationship.

Two main adjustable parameters in the frontal gravity wave source specification have been changed since WACCM4 due to the increased horizontal resolution in WACCM6: The frontogenesis threshold in WACCM6 is set to 0.108 K^2 (100 km)$^{-2}$ $\cdot h^{-1}$ and the source stress of frontally generated waves is set to $\tau_b=3\times10^{-3}$ Pa (Gettelman et al. 2019). In WACCM4, the frontogenesis threshold is set to 0.045 K^2 (100 km)$^{-2}$ $\cdot h^{-1}$ and the source stress of frontally generated waves is set to $\tau_b=1.5\times10^{-3}$ Pa. Different parameterization schemes and resolutions may have some effects on DW1-ENSO relationship. However, the completely opposite DW1-ENSO relationship in SD-WACCM and WACCM6 should not be simply attributed to the gravity waves. Indeed, DW1-ENSO relationship in WACCM4 are similar to those in WACCM6 (Pedatella & Liu, 2012 & Ramesh et al., 2021). Other factors which differ in different version of WACCM could affect the DW1-ENSO connection, more investigation and comparison between models are needed to determine the effect of the gravity wave parameterization is in the future work.

3. *The effect of R on DW1 seems to maximize at latitudes below the peak of the DW1*

(reference is to Figure 5 but this is where a statement or a figure as to the overall structure of DW1 would be helpful). As a result, I wonder whether it is really relevant. Or at least not at the peak- this is where going beyond a simple listing of causes could be useful.

Response: Thanks for the suggestion. The ratio of the absolute and planetary vorticity R is equivalent to changing the planet rotation rate. In classical theory, the vertically propagating DW1 is restricted near the equator due to the planet's rapid rotation. Therefore, a faster rotation rate (positive R anomalies) will suppress the latitudinal band (i.e., waveguide) where DW1 can propagate vertically. On the other hand, the slower rotation rate (negative R anomalies) favors the vertical propagation and is thus able to enhance the amplitude of DW1 at the low latitudes (Mclandress. 2002b). When the ratio of the absolute and planetary vorticity R-value at a certain height becomes larger, the upward propagation of tide is suppressed, which lead to weaker tides above there. We modified Figure R4 (Figure 6 in the revised manuscript) to show significant areas of the multivariate linear regression (MLR) coefficient of R on Niño 3.4. The green thick solid line represents the sum of the equatorial ratio of the absolute and planetary vorticity R values (15-30°N and 15-30°S), and the thick lines indicate the area where the regressed coefficients are significant. The mean R value (15-30°N and 15-30°S) response to ENSO is significantly positive at 60-90 km, which would lead to the suppressed propagation of DW1 above these areas.

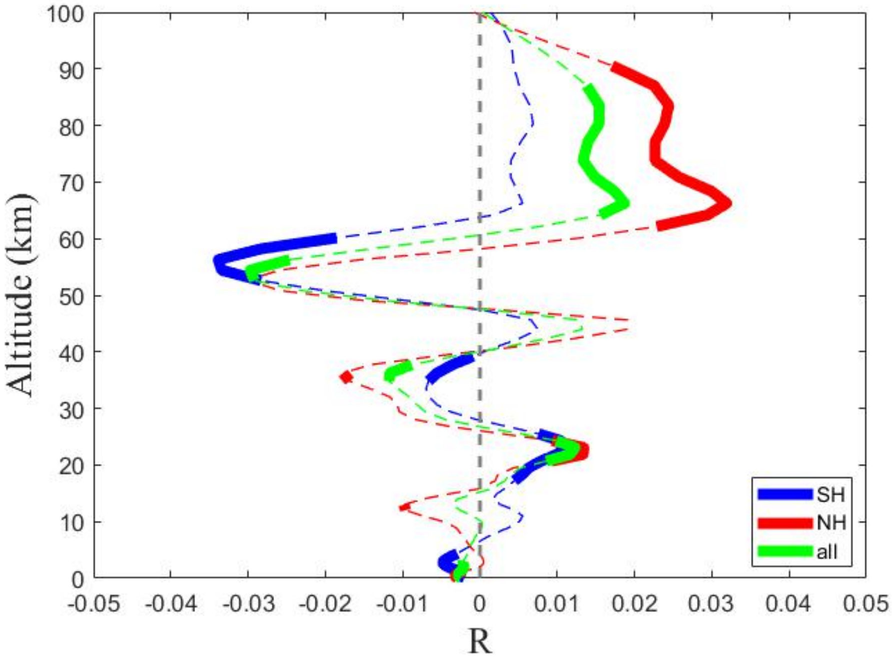

Figure R4 (Figure 6 in the revised manuscript). The anomaly of the ratio of the absolute and planetary vorticity, δR. The thin, dashed red, blue and green lines denote the averages of the Northern Hemisphere (from 15°N to 30°N), Southern Hemisphere (from 15°S to 30°S) and the whole (15-30°N and 15-30°S) , respectively. The thick, solid lines denote confidence levels below 95% for the F test.

4. In general, I think the GW analysis could use more detail. Overall, I think it's believable, but I would like more information- specifically I think they should put more effort on teasing out the effects of source forcing and filtering that they allude to in lines 377 and 378. If the gravity waves in WACCM are linked to convection, then shouldn't they be able to quantify the change in GW forcing more rigorously? Presumably there are certain phase speeds which are more or less relevant here?

Response: Thanks for your suggestion. In this study, the GW analysis with respect to the response of diurnal tide to ENSO is based on the parameterized GWs drag from the "specified dynamics" WACCM. In the standard setup of SD-WACCM4, the gravity wave source spectrum includes wave components with phase velocities in the range from -80 to +80 m s^{-1}, at intervals of 2.5 m s^{-1} (Beres et al. 2005). In SD-WACCM, the GW drags are separated with respect to different excitation sources, while the detailed information such as the phase speed are not available in the model output.

The discussion between GW drag generated by frontal systems and convection has been added in lines 387-388 in the revised manuscript as "The GW in the tropics is primarily induced by the convection, while the GW in the middle to high latitudes is mainly generated by the frontal systems (Figure S5, S6).".

Reference

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