

Atmos. Chem. Phys. Discuss., referee comment RC1  
<https://doi.org/10.5194/acp-2021-884-RC1>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on acp-2021-884

Anonymous Referee #1

---

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-RC1>, 2021

---

In general it's a thoughtful analysis of an important issue, the effect of El Nino 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 Nino.

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)

Specific comments:

- 1. I do not see where Ramesh showed a positive correlation between MLT DW1 and El Nino 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.
- 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.
- 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?
- 4. 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.
- 5. 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?
- 6. Grammar: line 223 and line 386 conflict. Line 223 says simulations (plural) and line 386 has the singular. Which is it? (note line 136 uses singular)
- 7. Data availability statement should say something about WACCM.