Review of "*Aura/MLS observes, and SD-WACCM-X simulates the seasonality, quasi-biennial oscillation and El Nino Southern Oscillation of the migrating diurnal tide driving upper mesospheric CO primarily through vertical advection*", by Salinas et al.

Recommendation: Revisions.

This paper reports the morphology and long-term variations in the diurnal cycles of T and CO observed by MLS, and extracted from an SD run of WACCM-X. The objective is to determine whether CO can be interpreted as a passive tracer of tidal motion. The authors demonstrate that the structures of diurnal CO and T closely track each other in both the data and in WACCM-X. By computing the mass budget of CO in WACCM-X, they are able to attribute the presence of tidal CO to vertical advection. The diurnal CO is also found to vary at QBO and ENSO periods.

This paper presents new information in the form of diurnal CO analyses, performs useful cross validation among MLS, SABER and WACCM-X T and CO, establishes the role of vertical tracer advection for tides, and reinforces earlier findings of QBO and ENSO variability in the propagating diurnal tide. Publication is therefore recommended following the revisions suggested below.

1. Lines 78-83: Does MLS sample at 2AM and 2PM at all latitudes? A latitude versus local time map might be helpful.
2. Sections 3 and 4: Figures 2 and 3 are described in exhaustive, almost mind-numbing detail. Instead of listing the altitude and latitude of every positive and negative extremum in each panel, I suggest a more concise wording with the goal of leaving the reader with the following "take-home" messages:

    a. The structures are dominated by (1,1) in March. A line plot of the (1,1) mode would be useful here.
    b. WACCM-X DW1 exhibits an additional "pulse" above 90 km in March that is not seen in MLS, both in CO and T, due to either a shorter vertical wavelength in WACCM-X, or to a phase offset between the model and the data.
    c. Patterns of T and CO are more asymmetric in June than in March. Please lose the "distortion of (1,1)" terminology. (See comment 8 below.)

3. Figures 2 and 3 have a lot of relatively empty space in them, with the interesting features crowded above 85 km. I suggest replotting them with the vertical axis starting at 75 km.
4. The chaotic middle and high latitude features in T and CO during winter months probably reflect variations in the zonal mean T and CO, instead of tides.
5. Line 194: Rewrite as “Although the latitude structure of DW! MLS CO μ' and SD-WACCM-X CO μ' have similarities to the DW1 temperature…”.
6. Line 196: Rewrite as “…later use this to prove that the DW1 affects CO.”
7. Lines 204 and 224: “aliasing of other tidal components into MLT T’ and CO”. I suggest being more specific here. Mention aliasing of migrating semidiurnal tides if the asc-desc LT difference is not 12 hours; also, are you thinking of terdiurnal tide leakage?
8. Lines 228-229, 240, 249, 607: These areas of the paper all refer to “distorted” of the (1,1) mode.

   (1,1) is an immutable eigenmode, characterized by a maximum at the equator, minima around 24N and 24S, and a uniform vertical wavelength of ~27 km. If the global structure of the tide deviates from (1,1) this is not due to “distortion” of (1,1), but the presence of additional Hough modes such as (1,2), (1,-1), etc.

9. Lines 230-231: The Forbes, McLandress, and Mukhartov papers cited do not discuss any relationship between the tides and the wave-driven residual mean circulation (v*,w*). Do you mean to say “zonally averaged winds”?
10. Lines 239: Delete the reference to nonmigrating tides in the aliasing discussion, as they do not alias to the zonal mean or the migrating tides. Nonmigrating tides do not alias into the zonal mean.
11. Provide a reference for equation 2.

    How is the DW1 component of the nonlinear terms defined? Do they arise from the advection of the DW1 components of μ by zonally averaged (u,v,w)? Or is it advection of time-mean μ by the tidal (u,v,w)?

12. Equation 3: This equation and its physical basis needs to be explained. I did not see any obvious analogies with the expressions in Eckermann et al. 1998. Since vertical motion does not appear, I presume it is inferred

adiabatically from T’ through $\partial T/\partial t = N^2 w'$. Is this correct? For tidal motions, why does the frequency not appear in equation 3?

13. Lines 307-322. This section is much too wordy and repetitive. Since the vertical gradient of time mean μ is positive in the upper mesosphere (as seen in Figure 1), we don’t need to read through hypothetical negative time-mean gradient scenarios. This entire segment can be summarized as:

    “Equation 3 indicates that when the vertical gradient of the time-mean zonal mean μ is positive, then an increase in μ' requires T’ > 0, which under adiabatic conditions implies a net downwelling. Conversely, a decrease in μ' implies T < 0', and net adiabatic upwelling.”

    Line 327 and 330: Replace “good” with “positive”.

    Lines 331-333: “For both MLS CO μ' and SD-WACCM-X CO μ', figures 4c and 4d indicate that the positive perturbations are driven by a relative downwelling due to the DW1 tide while the negative perturbations are driven by a relative upwelling.”

    Since we are not shown either w or $\partial u/\partial z$, there is no way to deduce vertical motion information from anything in Figure 4. Either show these variables, or remove this sentence.

14. Lines 372, 416, 417, and page: Replace “regress” with “project”. “We project the latitude profiles of CO μ' onto the (1,1) Hough mode profile.
15. Line 407: “Figures 6a and 6b showed MLS CO h' is weaker than SD-WACCM-X CO h'.

    Actually, MLS looks stronger than WACCM-X to me.

16. Figures 7a-c and 8a-c are difficult to read in general, and certainly for more nuanced features such as “Above 90 km, their seasonality shifts into having a primary peak close to June solstice”. I recommend staring the vertical axis at 75 or 80 km, or presenting the main features as line plots at selected representative altitudes.
17. Lines 480, 511, 513: CO h' increases…”

    What are the units of Figures 9c-f? Amplitude? Correlation?

What aspects of h’ and h_{μ} “increase”

18. Line 493: Change “of temperature” to “tide”.
19. Line 514: “Most studies have found that the (1,1) mode should decrease during El Nino events”.

    In fact, Lieberman et al. (2007) showed that (1,1) *increased* during ENSO events. The reason is that the climatological dry tongue disappears during the El Nino phase, leading to a more longitudinally uniform water vapor distribution, and therefore a stronger (1,1) forcing by water vapor heating.

20. Section 7: The Summary is much too long, and repeats details that were already worked over in the main body of the paper. The entire section can be condensed to:

    “This work uses 17 years of CO observations provided by the Microwave Limb Sounder (MLS) on-board the Aura satellite to analyse the seasonal and interannual variability of the DW1 component of upper mesospheric CO. These were then compared to simulations by the Specified Dynamics – Whole Atmosphere Community Climate Model with Ionosphere/Thermosphere eXtension (SD-WACCM-X). CO DW1 is dominated by the (1,1) mode in both MLS data and WACCM-X. However, MLS only observes one pulse of the (1,1) mode between 80 km and 95 km while SD-WACCM-X simulates two pulses. This could be due to MLS’ limited vertical resolution, or it could be due to inaccuracies in SD-WACCM- X simulation of the background atmosphere and/or tidal vertical propagation.

    The model-data comparison revealed that the structure of upper mesospheric MLS CO’s DW1 component is primarily driven by DW1-induced vertical advection over all latitudes during equinox seasons, and over all latitudes except the winter middle to high latitudes during solstice seasons. This could suggest that MLS CO’s DW1 component over the winter middle to high latitudes may be driven by other mechanisms such as meridional advection, eddy diffusion and/or chemistry. It could also suggest that the data over the winter middle to high latitudes may be affected by inadequate sampling.

In addition, we find that the interannual variability of MLS CO (1,1) and SD-WACCM-X CO (1,1) is primarily driven by the QBO and ENSO's effects on DW1- induced vertical advection. These conclusions suggest that we can use CO as a tracer for vertical advection due to the DW1 tide and the (1,1) mode on seasonal and interannual timescales. **"**

Grammar and style:

1. Line 40: New paragraph at "While".
2. Line 97: New paragraph at "Model".
3. Pages 11-12 are a bit too verbose. Consider deleting line 302 (If CO μ' and CO μ' are similar, then we can argue that vertical advection does primarily drive CO μ') and lines 308-312 (Equation 2 indicates…)
4. Line 370-371: Rewrite as "In this section, we examine seasonal and interannual variations in the (1,1) mode of CO."
5. Line 378: New paragraph at "Figure 6".
6. Line 446-459: "For example, Smith et al (2010) proved… very similar but for mesospheric SABER water vapor." Delete, unnecessary verbiage.
7. Line 477: New paragraph at "Figure 9".
8. Line 565: New paragraph at "Figure 10b".