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.

Summary:

This paper presents results from two observational datasets and a numerical simulation to show that diurnal variations in CO observed by MLS are the result of vertical advection by the migrating diurnal tide. The paper then goes on to show consistency of this interpretation with tidal variations on 6-month, QBO, and ENSO time scales.

The results are interesting. The authors should be commended for recognition that key information about atmospheric tides can be found in observations of trace species in the atmosphere and for then extracting the signal. The signal variation with season and with the QBO is also a useful contribution. The demonstration that observed diurnal variations in a chemical field are due to transport rather than photochemistry is also a strength of this investigation.

However, the paper needs some work. The interpretation of results, especially the confusion about how the perturbations in temperature and CO are related to DW1 (major comment #1, below), leads to misleading claims about the tidal impacts. My primary concerns are laid out in the comments below, along with suggestions to address them. Of the major comments, please take particular care to address #1 and #8.

Recommendation: major revisions needed

Major comments:

1. There will be some readers who are not familiar with the analysis method of subtracting observations from the ascending and descending parts of the orbit to get the diurnal variation. It is important that you explain to them how to interpret these results. In particular, a maximum or minimum of this value does ***not*** indicate a maximum or minimum in the tidal amplitude. The phase of the actual 3-d tide varies with altitude. At the point where the local times of the observations differ from those of the daily minimum and maximum of the tide for that particular altitude by $+\pi/2$ or $-\pi/2$ (i.e. by 6 hours), the ascending minus descending difference will be identically zero, regardless of the tidal amplitude. A difference in the altitude at which the T’ or μ’ goes to zero in different datasets means that the tide phases are different but, by itself, does not give any information about the relative amplitudes of DW1 in the datasets being compared. This confusion begins in the abstract and is seen throughout the paper. For example, line 85 labels the feature as “DW1-induced perturbations”, which is not inaccurate although not completely clear, but later this distinction is dropped. Line 144 uses the misleading term “DW1 component of CO and temperature”. To ensure that readers are not confused, I recommend not to use the term DW1 at all. Referring to the signals as T’ and μ' is okay as long as they are well-defined and carefully explained.
2. It is not clear why the SABER results are included. As noted in the paper, SABER can resolve DW1 in temperature. However, the actual tidal structure is never compared with the field

referred to as DW1 in the paper, which is not the actual tide (see comment above). Such a comparison could be useful to illustrate the relation between the tide itself and the perturbations analyzed in the paper.

3. The term chemical lifetime is used in a manner inconsistent with common usage. The lifetime normally is the time it takes for a molecule to decay due to photochemistry, not the timescale for it to be generated. Relevant to the discussion (lines 30-32; Section 5.1), the production of CO actually has a diurnal timescale (produced only during daylight) but its lifetime is defined by its relatively slow chemical loss. It appears that Eq. (2) does not even include a loss term.
4. At several points (e.g., lines 122-126; 204-205; 442-443), you speculate that discrepancies between MLS and SABER observations or WACCM are due to aliasing by the semidiurnal tide. While this is likely a contributing factor, there are other contributors that could also be important. One is the much poorer vertical resolution of MLS in the upper mesosphere. Since DW1 has a vertical wavelength of ~25-30 km, the MLS field of view, which smears the observations over ~9-12 km, can substantially reduce the amplitude.
5. Another contributor to discrepancies between MLS and other data is a result of the change in local time of the MLS measurements with latitude. As the Aura orbit approaches the turn-around latitudes near the poles, the times of ascending and descending observations get closer together and eventually coincide. Therefore the differences between them are less and less representative of the diurnal tide. Around 70-75°, the time differences are closer to 6 than to 12 hours. The average of ascending and descending times in high northern latitudes is opposite (12 hours different) to that in high southern latitudes. It is therefore no wonder that the MLS latitude structure at high latitudes in Figure 3 does not look like that of the (1,1) Hough mode. Attention to these local times could be relevant to the discussion of hemispheric differences (lines 596-601; 609-610).
   In addition to revising the discussion to take the confusing latitude variation into account, I suggest to redo the fit of the results to the (1,1) Hough function but using a limited latitude range such as only latitudes equatorward of +/-40° or +/-50°.
6. Related to the previous comment, the perturbation in μ' due to tidal transport depends not just on the tidal winds but also on the vertical gradient of the mean composition. Don't lose sight of this when you are interpreting differences; see in particular the discussion of the hemispheric distortions seen in Figures 3 and 5.
7. (Figure 10c-10f and discussion) You are not alone in splitting time series into two or more segments to improve the apparent correlation. However, this should not be done unless you can provide a valid reason, based on physics, for doing so. Even the full 18-year timeseries is very short for identifying variations with the timescale of ENSO. What I see from your results is that there is no consistent impact of ENSO on your tidally influenced variables. The obvious interpretation is that the coincidences that appear for a few years are due to random interannual variability, not a causal mechanism. Leave it that way rather than trying to force small segments of the data into an agreement. I recommend that you drop the ENSO comparisons and discussion completely and merely mention that no consistent relation was found.

Minor comments

1. (lines 58-60) There are other observational studies that have shown a QBO in diurnal tidal winds. See Burrage et al. (1995), Pramitha et al. (2021), Xu et al. (2009).
2. Section 2 indicates that the data are on pressure levels but all results are presented as a function of altitude. How was the altitude determined? Did you actually interpret the observations and model output to altitude or do you use an approximate altitude such as the global & seasonal average or a fixed log-pressure to altitude ratio? Were the Hough mode fits done on altitude or pressure levels?
3. Section 5.1 is confusing. What function are the terms being fit to with the least squares fit?
4. (lines 309-310) "… if the vertical gradient of a tracer's daily-mean zonal-mean component is positive (the gradient increases with height) …": omit the words in parentheses if you mean that the tracer increases with height, not its gradient.
5. Check the sentence beginning at line 320; this looks like it's backwards.
6. I could not follow any part of the paragraph beginning at line 407. Why do you say that MLS CO h_μ' and SABER h_T' are weaker? The magnitudes shown in the plots look larger. The second sentence is somewhat garbled so it is not clear what point is intended by these comparisons.
7. (line 426ff) "Above 90 km, their seasonality shifts into having a primary peak close to June solstice." This shift could be because the phase of DW1 is such that the 2 AM and 2 PM differences due to the tide itself are small (as in my first major comment). For example, a seasonal shift in the tide phase could contribute. A look at the phase of the full DW1 from SABER could help with the Interpretation.
8. (Figure 9) The phase lags can be reduced or eliminated by using a different pressure level for your QBO index. This might improve the MLR analyses shown later.
9. Please include references for the data you use for the QBO and ENSO indices.

Reference not cited in the manuscript

Burrage, M. D., Hagan, M. E., Skinner, W. R., Wu, D. L., and Hays, P. B., 1995. Long-term variability in the solar diurnal tide observed by HRDI and simulated by the GSWM, Geophys. Res. Lett., 22,2641–2644, doi:10.1029/95GL02635, 1995.