We wish to thank the reviewer for their helpful comments. We have modified the manuscript as suggested. Below shows our responses to all the comments. Reviewer's comments are in bold red while our responses are in black. Note that, unless otherwise specified, all line numbers mentioned in the responses to comments refer to the numbers in the new (no tracking) manuscript.

REVIEWER 1:

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 +p/2 or -p/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.

We first clarify that we do not simply subtract the ascending and descending parts of the orbit. It is not enough that a data point is part of the ascending and descending parts of the orbit. We make sure to check that the values are at around ~2 AM and ~2 PM local-times when calculating these zonal-means.

In figure 2, we have added the following sentences in lines 161 – 165 explaining how to interpret the results: *"When a dataset has full local-time coverage, figure 2 would come in the form of amplitude contour maps and it would be accompanied by a phase contour map. The amplitude map will then clearly indicate where exactly the tides are strongest. In contrast, figure 2 and the other figures showing μ' cannot indicate where exactly the tidal amplitudes are strongest. It can only indicate where the tide significantly affects CO (tidal perturbations) but it cannot indicate the relative strength of this influence."*

We agree with the concerns on the use of the term "DW1". After re-checking the manuscript, we first point out that the first parts of the results section do not mention the term DW1. We carefully only used the terms T' and μ'. This is because we acknowledge that in these sections, we have not yet established that DW1 predominantly drives T' and μ'. In the next section though, we have established through a comparison between MLS and SABER that T' may be predominantly driven by DW1. We complemented this by mentioning numerous previous papers establishing this. Then, the next section shows that the DW1 driving T' may also be driving μ'. Despite that, the reminder of the results section only use the terms T', μ' or (1,1). We do not mention anything about the DW1 tide although the term pops up in the section-headers. It is only in the summary and conclusions section that after all the results, we can conclude that much of T' and μ' is primarily driven by DW1. As for the abstract, we changed "DW1-induced perturbations" to "local-time perturbations".

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.**

Section 4 compares SABER T' with MLS T'. Note that SABER T' is the DW1 component of SABER T because the DW1 amplitudes and phases were first calculated before calculating SABER T'. In this section, we do show there is good agreement between SABER T' and MLS T' giving additional confidence that MLS T' may be predominantly driven by DW1.

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.**

We have modified lines 30-32 to define the term chemical timescales and avoid using the term chemical lifetime. The new lines are: *"This reaction makes the timescales of the chemical* *reactions driving CO's variability (hereafter referred to as chemical timescales) longer than dynamical timescales (Minschwaer et al, 2010)."* We have also included the loss term in equation 2 as well as in the Appendix B discussions.

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.**

We don't change lines 122-126 (original manuscript) because we use that to lead to our rationale behind the use of SABER observations in the next paragraph.

But for lines 204-205 (original manuscript), we have added the following sentence: *"It may also be attributed to differences in the instruments' vertical resolutions. SABER has a vertical resolution of ~2 km while MLS has a vertical resolution of ~10 km (Remsberg et al., 2008; Livesey et al., 2011). Given that DW1 typically has a vertical wavelength of ~25-30 km, MLS' coarser vertical resolution can substantially reduce the amplitudes."*

For lines 442-443 (original manuscript), we have modified the lines into: *"Figures 6a, 6c and 6d shows that the seasonality of MLS CO h'_{μ} may be affected by the incomplete local-time sampling of MLS or its coarse vertical resolution."*

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 turnaround 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°.**

As mentioned in our reply to comment #1, we first clarify that we do not subtract the ascending and descending parts of the orbit. We are subtracting values at ~2AM and values at ~2PM. Hence, a contributor to the discrepancies that we point out relates to the uneven sampling across latitudes. While we are confident in the adequate sampling over the low-latitudes, this may not be the case over the mid-latitudes. This is alluded to, for example, line 235-240: *"These differences between MLS T' and SABER T' over the mid-latitudes may be a result of MLS inadequate sampling causing significant aliasing from other tides."*

However, we did redo the fits to only use values equatorward of +/- 50 degrees. The values didn't change significantly but these new values are the ones used in the revised manuscript's plots.

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.**

This is immediately mentioned in the paragraphs following equation 3 and, in the results section, we use this to argue whether the perturbations are due to a net downwelling or net upwelling (lines 335-336 and lines 364-365).

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.**

We have removed figures 10c to 10f as well as the pertinent discussions. We have also modified the first few sentences of section 6.4 into: "The previous sub-section found that QBO and ENSO variabilities are present in both MLS CO h'_μ and SD-WACCM-X CO h'_μ. In this section, we quantify the changes in MLS CO h'_μ or SD-WACCM-X CO h'_μ due to QBO. We don't quantify the changes due to ENSO because there were only a few events during our data-span. Hence, any estimated response may be biased."

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).

Xu et al (2009) and Pramitha et al (2021) are already there. We added Burrage et al. (1995).

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?

All calculations are done on pressure levels but for the plots, we replaced the pressure levels with their approximate altitudes.

3. Section 5.1 is confusing. What function are the terms being fit to with the least squares fit?

After presenting equation 2, we clarified the fit by adding the sentence: *"The DW1 component of each term is calculated by fitting the terms into the equation* $X(\lambda, t) = \bar{X} + \hat{X}_{n,s}\, cos(\pi t/24 - (-1)\lambda - \hat{\psi}_{n,s})$ *using 2D 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.

Omitted.

5. Check the sentence beginning at line 320; this looks like it's backwards.

Corrected.

6. I could not follow any part of the paragraph beginning at line 407. Why do you say that MLS CO hμ' and SABER hT' 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.

We have changed this entire paragraph into: "Figures 6a and 6b showed MLS CO h'_{μ} is stronger than SD-WACCM-X CO h'_{μ}. Figures 6d and 6e also showed that SABER h'_{T} is stronger than SD-WACCM-X h'_{T}. A larger MLS CO h'_{μ} than simulated is consistent with a larger realistic MLS or SABER h'_{T} than simulated. An underestimation of SD-WACCM-X h'_{T} indicates inaccuracies in the simulated background atmosphere, tidal source or tidal dissipation mechanisms."

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.

We have added the following lines: *"This could suggest that the latitude structure of DW1's phase during solstice (equinox) causes maximum (minimum) values when taking the difference of values at ~2 AM and at ~2 PM. This consequently enhances (reduces) MLS CO* h'_{μ}*. This is difficult to validate with a very high degree of uncertainty though even with SABER data because of the differences in MLS and SABER's vertical resolution."*

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.

Although the QBO affects other pressure levels, it primarily originates around ~30 mb. We do not use a QBO index in other pressure levels nor do we want to remove the phase lags because we want to point out that there is a noteworthy lag which indicates some form of wave-mean flow filtering could be involved.

9. Please include references for the data you use for the QBO and ENSO indices.

These have been specified in the “Data Availability” section.

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.