Response to Second Round of Reviewers: Comments on "Analysis of Migrating and Non-Migrating Tides of the Extended Unified Model in the Mesosphere and Lower Thermosphere" by Griffith and Mitchell

Quotes from the reviewer are in bold, and responses are indented. We first wish to thank the reviewer for the insightful and useful comments provided on the second version of the manuscript. We have made the changes requested and believe the manuscript is strengthened as a result.

Responses to Reviewer #3:

General Comment(s):

1.) While the model results presented in Figures 1, 2, 7, 8, 10, 12, 13, 14, 15 are important, they are extremely difficult to see and are too small. I would suggest the authors make the axis labels slightly bigger and in bold type font, while also making the plots larger. This would make them much easier to read.

We agree, that in the current draft format the figure sizes are too small. The axis labels have been made as large as possible so that they do not overlap in response to the first round of reviews. The size of the figures is dictated by the journal format which has been adhered to in this draft. We will discuss what can be done to make the figures as large as possible with the Publications Production Office when the manuscript goes for typesetting and publication.

2.) The word "tidal modes" or "modes" is used incorrectly throughout the manuscript. Typically, "tidal modes" or "modes" in this context refers to an individual tidal components "Hough mode" or latitudinal structure. Each "Hough mode" is represented by an accompanying latitudinal function, referred to as a Hough function, or the eigen functions of Laplace's tidal equation. The latitudinal structure of any individual tidal component (e.g., DW1, SW2, or DE3) is determined by the superposition of all the different Hough modes. I would strongly suggest changing "tidal modes" or "modes" to tidal components or component(s) throughout the entire manuscript. Please see a tidal review by Forbes [1995][1] for more details.

We agree. This has been changed in the manuscript.

3.) There is very little discussion throughout the manuscript about the phases of the different tidal components produced in the model. The manuscript would be greatly enhanced if there were some phase comparisons between the tidal phases produced in ExUM and other models, as well as observations.

We agree that phases are an important consideration. However, we feel that this would make the paper too long, adding many more figures. This is an interesting topic that will be addressed in future studies of the ExUM. We have added a footnote to this effect in the paper (Page 5).

4.) While the authors discuss how one major source of tidal dissipation is handled (ion drag, which is not included since the model top is at 120 km), there is little to no discussion about how the other major tidal dissipation processes including eddy and molecular diffusion are handled in the model. Were these discussed in previous papers? If so, one or two sentences of how these types are handled would suffice. If not there needs to be some discussion about how these things are handled, parameterized, in the dynamical core in the ExUM. A follow onto this question would be how are the specific heats handled in ExUM? Are they height varying? Please elaborate on this as well.

We agree that additional clarification is required here, and it has been added to the text (L153 - L155). Eddy and molecular diffusion are not used in the MLT in this version of the ExUM (the paper by Griffin et al. (2018) (below) suggests this has its most dominant effect above 150 km). The specific heats are also not height varying and are just the standard values for air. This is a reasonable assumption up to the turbopause which is the primary region of interest in this paper. These are all additions which can be made to improve the model in later versions, but this paper represents a first look at tidal components from the newly extended Unified Model.

Griffin, D. and Thuburn, J.: Numerical effects on vertical wave propagation in deep-atmosphere models, Quarterly Journal of the Royal Meteorological Society, 144, 567–580, https://doi.org/10.1002/qj.3229, 2018.

5.) On L165-205 there is extensive discussion on how the background temperature profile is nudged towards climatology. How robust are the simulated tidal amplitudes and phases to this background temperature profile? For example, *Jones et al.* [2018][2] showed that tidal results in the NCAR TIME-GCM were very sensitive to changes in the how the model fields were constrained. The authors should comment on how tidal results shown in Section 3 and 4 might depend on this choice of background temperature profile?

We agree that additional clarification is required here, and we have added a comment acknowledging that this is the case (L210 – L218):

"There will naturally be some variation in the modelled tidal fields when this background temperature profile is varied *Jones et al. [2018]*. However, the main focus of this work is to provide a closer look at the migrating and non-

migrating components of atmospheric tides present in the newly extended model and show that they are of reasonable order of magnitude and compare reasonably with other models and with observations. A detailed analysis of the sensitivity of the tidal fields to the background temperature profile is beyond the scope of this work -- we note that the goal in future development of the ExUM is to replace this background temperature profile with appropriate radiation and chemistry schemes for the MLT. As well as this, the primary diagnostics used are zonal and monthly mean fields for climatological variations, which will be less sensitive to such variations in the background temperature profile. Nevertheless, it is worth bearing this in mind when considering the results presented here."

6.) L240: States " we only show results from a single simulation …" What does this mean? The simulations performed as part of this study should be clearly outlined in this work, so that independent reproducibility of results presented herein would be possible.

Apologies for the confusion. This means nothing more than the model is run several times with exactly the same setup to ensure robustness of the results (to phenomena such as computational glitches). This was performed and showed no difference in model results between simulations. We will change the wording of this in the manuscript for clarity (L257 – L258).

7.) Daily tidal amplitudes simulated in ExUM could be validated against NAVGEM-HA and TIMED/TIDI calculated by Dhadly et al. [2018][3].

Thank you for pointing out this useful reference. A comparison of the daily tidal amplitudes in the ExUM with those provided in *Dhadly et al. [2018]* has been added to the manuscript (L809 – L818).

<u>Minor Comment(s):</u>

1.) Why was the month of January chosen for a number of the plots? This was not stated in the manuscript. I do not have any issue with it, just would like to know the authors' rationale behind this. The readers might be interested in this type of information.

Thank you for bringing this to our attention. January is chosen solely for illustrative purposes, to give an initial insight into the tidal components present in the model, before showing more detailed tidal decompositions which are given for all months. A footnote has been added to this effect (Page 12).

2.) Several times throughout the manuscript the Global Scale Wave Model (GSWM) is incorrectly referred to as the Global Wave Scale Model (GWSM). Please correct this in all places throughout the manuscript.

This has been corrected appropriately.

3.) When referring to atmospheric tidal and ionospheric coupling, I believe the authors have neglected some pretty influential pieces of work. While, I understand, it is not necessary to cite every paper I would suggest the authors cite the following work by Immel et al. [2006].4 There are number of other works that could be cited as well, but please cite at least Immel et al. [2006].

The introduction was reduced in length at the request of the initial round of reviews. However, we agree that the Immel et al. [2006] paper in particular is a notable work and we have added this reference to the introduction (L34).

4.) L26: Strike *etc.*

This has been corrected appropriately.

5.) L27: Strike *and tides*.

This has been corrected appropriately.

6.) L63: Replace *have been suggested to result from* to *are in part driven by*.

This has been corrected appropriately.

7.) L77: Strike *the*.

This has been corrected appropriately.

8.) L81: *Nonmigrating* should be non-migrating.

This has been corrected appropriately.

9.) L124: Replace *ask* with *seek to answer*.

This has been corrected appropriately.

10.) L128: Replace *can be suggested to* could.

This has been corrected appropriately.

11.) L184: What altitude is this *z* referring to? Geopotential height, geometric height, etc. Please state what the variable z is.

This z altitude is just a standard height coordinate, representing the vertical height above sea level. This has been added to the text (L186).

12.) L194: Strike the first *mean* and replace it with *and*. (This occurs in other places in the manuscript when referring to *zonal mean monthly mean*. Please replace this throughout the manuscript.)

This has been corrected appropriately.

13.) L210: It is not clear why WACCM-X scale heights are used? What are the purpose of these? Is this self-consistent with the use of the CIRA climatological temperature model used for the background temperature? Please add additional details to address this.

The WACCM-X scale heights were used to assess the variation in scale height under different solar conditions (solar minimum compared to solar maximum). These give a reasonable baseline on which to determine the vertical level set in the model. For the background temperature profile, we thought it more appropriate to use CIRA. Naturally, the WACCM-X temperature profile and the CIRA climatological temperatures used for the background temperature profile will exhibit some differences, but both will provide a reasonable initial implementation which can be tuned in future versions of the model. This has been added to the text (L226 – L228).

14.) Sentence on L282-283: It is not clear the dominance of the different migrating tidal components with latitude. From a theorical and observation perspective, this is what one would expect, but it is hard to see this in Figure 7. I would suggest adding some additional details to describe this switch in the latitudinal structure.

Agreed. Clarifying detail has been added to the text (L300 – L301).

15.) L355: Replace *non-migrating tidal components* with *tidal spectrum*.

This has been corrected appropriately.

16.) L414-415: Please provide any details as to why DE3, DE2, DE1, DO, and DW2 magnitudes are weak in ExUM at polar latitudes?

The reason for this difference is at this stage unknown. We will add a comment to this effect (L432 – L433).

17.) L450: Strike *clear*.

This has been corrected appropriately.

18.) L456: Strike *The*.

This has been corrected appropriately.

19.) L472: Add *semidiurnal* between non-migrating and components.

This has been corrected appropriately.

20.) L528-529: It is stated that the short term variation in DE3 is 125%. Is this relative to the 30-day running average values? If so, please state that. Seeing that the DE3 peaks at 4 K, but the short term variation is up to 5 K is puzzling. (This occurs with other waves as well, i.e., the short term variation is greater than 100%. Make sure this is clear to the reader.)

We agree that this is confusing – it is relative to the 30-day running average. This has been clarified in the text (L538 - L539 and L605, L611, L613).

21.) Strike *The* at the beginning of the sentences on L546 and L548.

This has been corrected appropriately.

22.) L600: Add *are* between there and a.

This has been corrected appropriately.

23.) L637: DE3 is a Kelvin wave (equatorially-trapped wave), please state this as the reason there is no meridional wind component.

This is an excellent point that we had not appreciated. We will add this to the manuscript and thank the reviewer for pointing out this interesting observation (L659 – L660).

24.) L690: Strike *the* between DE3. (This occurs below when referring to other tidal components. Please strike those leading *the*'s as well.

This has been corrected appropriately.

25.) L791: *Tides* should be *tide*.

This has been corrected appropriately.

[1] Forbes, J.M. (1995). Tidal and Planetary Waves. In The Upper Mesosphere and Lower Thermosphere: A Review of Experiment and Theory (eds R.M. Johnson and T.L. Killeen). https://doi.org/10.1029/GM087p0067
[2] Jones, M. Jr., Drob, D. P., Siskind, D. E., McCormack, J. P., Maute, A., McDonald, S. E., & Dymond, K. F. (2018). Evaluating different techniques for constraining lower atmospheric variability in an upper atmosphere general circulation model: A case study during the 2010 sudden stratospheric warming. *Journal of Advances in Modeling Earth Systems*, 10, 3076– 3102. https://doi.org/10.1029/2018MS001440
[3] Dhadly, M. S., Emmert, J. T., Drob, D. P., McCormack, J. P., & Niciejewski, R.(2018), Short-term and interannual variations of migrating diurnal and semidiurnal tides in the mesosphere and lower thermosphere. *Journal of Geophysical Research: Space Physics*, 123, 7106– 7123. https://doi.org/10.1029/2018JA025748
[4] Immel, T. J., Sagawa, E., England, S. L., Henderson, S. B., Hagan, M. E., Mende, S. B., Frey, H. U., Swenson, C. M., and Paxton, L. J. (2006), Control of equatorial ionospheric morphology by atmospheric tides, *Geophys. Res. Lett.*, 33, L15108, doi:10.1029/2006GL026161.