Response to 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 reviewers for the insightful and useful comments provided on the first version of the manuscript. We have made the changes requested and believe the manuscript is strengthened as a result. The introduction has been significantly shortened and the discussion has been significantly strengthened to include more comparison with observational studies. Technical aspects relating to the model development have been shortened but there remains enough detail to aid other researchers in developing whole atmosphere models. We have also made some minor typographical/readability changes to the text.

Responses to Reviewer #1:

We disagree that this is a technical report. The goal of this paper is firstly, to report new developments and capabilities of the ExUM, and secondly to investigate non-migrating and migrating tidal fields produced by this new model and benchmark them against observations and other modelling studies. We believe this makes the work entirely suitable for publication in Annales Geophysicae. We note that the second reviewer had none of these concerns. However, we will clarify the scientific focus in the abstract and introduction and reduce its length.

We agree however that there is bias towards comparison of modelling studies. To this end, we will add to the discussion comparing the ExUM fields with additional satellite and meteor-radar observations. We add comparison to the studies of Zhang et al., (2006), Forbes et al., (2008), Li et al., (2015) for comparison with SABER temperatures and Wu et al., (2008a, 2008b, 2011), Pokhotelov et al., (2018), Dempsey et al., (2021), Angelats i Coll & Forbes (2002), Oberheide et al., (2006, 2007), and Huang and Reber (2004) for comparison with TIDI, UARS and meteor-radar wind measurements. See, for example, the shortening of the introduction on Pages 2-4 and additions to the discussion on Pages 30, 32 & 35-36 in the revised version of the manuscript.

However, we note that a paper regarding detailed quantitative comparison is beyond the scope of this work and is fact the goal of ongoing research.

The overlapping material is related to introducing/specifying the extended model and so will be similar by its very nature. We agree that the overlap is in some places excessive and have removed some overlap, now referring the reader to this previous work (e.g. L146).

References:

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Wu, Q., Ortland, D., Killeen, T., Roble, R., Hagan, M., Liu, H.-L., Solomon, S., Xu, J., Skinner, W., and Niciejewski, R.: Global distribution and interannual variations of mesospheric and lower thermospheric neutral wind diurnal tide: 1. Migrating tide, Journal of Geophysical Research: Space Physics, 113, https://doi.org/10.1029/2007JA012542, 2008a.

Wu, Q., Ortland, D., Killeen, T., Roble, R., Hagan, M., Liu, H.-L., Solomon, S., Xu, J., Skinner, W., and Niciejewski, R.: Global distribution and interannual variations of mesospheric and lower thermospheric neutral wind diurnal tide: 2. Nonmigrating tide, Journal of Geophysical Research: Space Physics, 113, https://doi.org/10.1029/2007JA012543, 2008b.

Wu, Q., Ortland, D., Solomon, S., Skinner, W., and Niciejewski, R.: Global distribution, seasonal, and inter-annual variations of mesospheric semidiurnal tide observed by TIMED TIDI, Journal of atmospheric and solar-terrestrial physics, 73, 2482–2502, https://doi.org/10.1016/j.jastp.2011.08.007, 2011.

Zhang, X., Forbes, J. M., Hagan, M. E., Russell III, J. M., Palo, S. E., Mertens, C. J., and Mlynczak, M. G.: Monthly tidal temperatures 20–120 km from TIMED/SABER, Journal of Geophysical Research: Space Physics, 111, https://doi.org/10.1029/2005JA011504, 2006.

Responses to Reviewer #2:

Comments:

Lines 147 – 168: These paragraphs to a certain degree belong to the model description. Page 6: Are these footnotes necessary? It is textbook knowledge.

Agreed. The introductory paragraph on GW parameterization is left in the introduction and those specific to the ExUM are moved into the model description section (e.g. L152-L164). We agree and have removed the footnotes accordingly.

Line 211: There has been rocket observations of mesopause temperatures lower than in CIRA (e.g. Lübken et al., 2004). Would you like to comment on that?

Thank you for this comment. At this early stage, we use CIRA as a nudging profile for temperature as it provides a reasonable year-round temperature climatology for the MLT. We are aware that there are discrepancies between CIRA and observations such as those performed by Lübken et al., 2004, but more sophisticated temperature mechanisms and schemes will be added in the future to replace this nudging scheme and it is only intended as a reasonable estimate so that we can get a first look at the nature of the non-migrating tides in this newly extended model.

Line 220, Figure 2: Some authors have reported a double mesopause (e.g. Yu and She, 1995). Would you like to comment on that?

As above.

Line 268, Figures 6: The summer wind reversal at middle latitudes seems to be a bit too low. Would you like to comment on this?

Biases are to be expected at this early stage of model development. The objective of this paper is to test the newly extended model to discern whether the migrating and non-migrating tides produced are qualitatively reasonable and quantitively similar to other studies. These more detailed biases will be addressed in future work to improve the ExUM, where more physically realistic schemes (such as an MLT chemistry scheme) will be introduced. We will add a comment in the paper acknowledging that biases (such as you have mentioned) exist in this new extension of the model (L234-L237).

Section 2: Could you add a brief description of latent heat release in the model?

A brief sentence is added directing the reader to the more thorough overview presented in Walters et al. (2017). (L149)

Lines 360 pp, summary of section 3.2: To what degree are these values supported by observations? This is the topic of another paper, but a brief discussion would be helpful here (also for the summary of 68°S results)

This is a good suggestion. A brief discussion has been added following each summary, comparing the model values to observations. (L342-L354 & L407-L415)

Line 362: Add here that this is at high altitudes

This has been added to the text. (L399)

Figure 13: Observed SDT amplitudes at NH middle latitudes have been found to be > 40 m/s (e.g. Pokhotelov et al., 2018). This seems to be more than the modeled ones. There is a SH/NH difference, and do the SH amplitudes agree better with the observations?

Agreed, thank you for making us aware of this. The meteor radar results from Pokhotelov et al., (2018) are larger than the values we observe in the NH. Interannual variability is another source of variability between the observations and modelled values, so it is important to have this in mind when comparing the fields. The SH amplitudes seem to agree better with observations, for example those observed by Dempsey at al., (2021) at Rothera are between 20 and 40 m/s in keeping with the values given by the ExUM. We will add comment on this in the discussion, noting that interannual variability will also play a role in the agreement of the model and observations. (L840-845)