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Comment on angeo-2021-21

Anonymous Referee #1

Referee comment on "Analysis of migrating and non-migrating tides of the Extended Unified Model in the mesosphere and lower thermosphere" by Matthew J. Griffith and Nicholas J. Mitchell, Ann. Geophys. Discuss.,
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This paper presents the principle Fourier components, at periods of 12 and 24 hours, generated by a modified version of the Met Office ExUM. These components are interpreted as atmospheric tides (both migrating and non-migrating) and are compared to tidal oscillations generated by other models and, to a lesser extent, observations.

This paper runs at over 1000 lines and reads like a technical report for the model developers, or perhaps an introductory chapter in a monogram-style PhD thesis. It lacks an over-arching science question. And as such I don't see that this is acceptable as a scientific paper in AG in its current form. There may be alternative journals dedicated to detailed aspects of model development that are more suitable for this paper in its current form. But to turn this technical report into a scientific paper suitable for a scientific journal, such as AG, I could suggest two approaches, either of which would be acceptable:

1. to investigate the model parameters and present before/after plots demonstrating the sensitivity of the modelled tides to various inputs. This can be used either to elucidate one or more important mechanisms in atmospheric dynamics, or to demonstrate that the "improvements" to the ExUM model described in this paper lead to improvements in the ability of the model to reproduce realistic tides. This latter point is fundamentally lacking in this manuscript as presented.

2. to quantitatively compare the model output closely to observational data and try to explain/interpret differences.

Where this is attempted in the paper, the model output is largely focussed on latitudes 8 S and 68 S but, as far as it is possible to tell, nearly all comparison with observational data are at other latitudes (e.g. the high latitude northern hemisphere observational studies of non-migrating tides presented in Iimura et al. and Hibbins et al.) Hence the similarities/differences discussed are vague and qualitative at best.

If option 1 is chosen it should be noted that the paper concludes with a list of further studies that could be undertaken with the model (L760-765), but it is not clear what the "specific recommendations on further developments" to the model, promised in the abstract and conclusions (L16 and L796), actually are. The further studies described on L760-765, although potentially interesting, are not justified by any of the material presented in this paper. Indeed the paper states "the focus of our study is not on the development of the GW parameterization" (L744), but the 5 suggestions for further studies are specifically associated with changes to the GW parameterization in the model. Similarly the authors state that they have already "improved on the implementation of the ExUM" (L771) compared to that presented in Griffith et al. (2021), and Section 2 details the amendments made to the model. But these are presented without any attempt to determine whether these changes improve the representation of the tides in the model (or not). So it is difficult to accept that these are necessarily improvements if the aim of the paper is to model tides accurately. Hence the conclusions are not currently supported by the evidence presented in the paper.

If option 2 is chosen then a comprehensive literature review will reveal that there are many observational studies that have attempted to extract migrating and non-migrating tides from real observational data (beyond those limited cases discussed in the current manuscript). Rather than comparing their model output with other models (as has largely been done in this manuscript) the authors should look to quantitatively compare their model output with real observations using the correct latitude/longitude/date/height/parameter from the observations. For example although there is a small section in 4.2 (L 738-743) where the authors compare observational satellite data with their model, the majority of this section compares ExUM with other models from different altitude levels. Why not compare like with like - what is to be gained by comparing ExUM (95 km) output with data/models from 90km? In addition, if such a comparative study was presented, the authors need to be keenly aware that each observational study will suffer its own unique set of limitations (e.g. vertical/temporal/spatial resolution issues) that will affect the observed tides and will need to be carefully pre-processed in the model output before a quantitative comparison can be made. Only then can robust conclusions about the accuracy of the tidal oscillations generated by this model (as clearly stated as an goal in L 3-4 of the abstract) be made.

On a final general note, I encourage the authors to write succinctly. Maybe this is a consequence of the material originally being written for another purpose (see comments above), but I would ask if a paper like this really needs a 159 line introduction? Is every plot essential to tell the story? Ask what could reasonably be edited while still addressing the (new) scientific objectives. On the same theme, there appears to be some very close overlap between passages of text in this manuscript and that presented in an earlier paper by the same first author (<https://doi.org/10.5194/angeo-2021-6>) in the same Journal. Although I have not attempted a line-by-line comparison of the two documents, some examples include L122-128, L234-240, L271-274 (and the footnotes at the bottom of P8) in this current manuscript.