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Interactive comment on "Impact of Gravity wave drag on the thermospheric circulation: Implementation of a nonlinear gravity wave parameterization in a whole atmosphere model" *by* Yasunobu Miyoshi and Erdal Yiğit

Anonymous Referee #1

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Review of "Impact of gravity wave drag on the thermospheric circulation: Implementation of a nonlinear gravity wave parameterization in a whole atmosphere model" by Miyoshi and Yigit.

This manuscript describes initial results from a "whole atmosphere" general circulation model where an existing middle atmosphere gravity wave drag parameterization based on the Lindzen scheme is replaced with a parameterization based on "non-linear interactions" following Medvedev and Klaassen that includes dissipation terms for thermospheric processes. The model simulations for perpetual June conditions

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show that when a non-orographic GWD parameterization with thermospheric effects included (EXP2) is applied throughout the entire model domain, the resulting zonal mean zonal wind and amplitude of the migrating migrating tide (SW2) in temperature fields are quite different compared to results from similar model simulation using a Lindzen type GWD scheme from 0-100 km (EXP1). The authors claim that the SW2 in EXP2 is in better agreement with observations than EXP1 SW2 due to deceleration of the zonal wind by the thermospheric GWD. The authors conclude that parameterized GWD in the thermosphere plays an important role in the momentum budget of the thermosphere and is essential for low resolution whole atmosphere models to more realistically simulate the atmosphere-ionosphere system.

The manuscript presents interesting results from a whole atmosphere model highlighting the effects of parameterized GWD in the thermosphere on the zonal winds, which in turn affect the amplitudes of SW2. Neither the GCM or the GWD parameterization are new, but this seems to be the first time these tools have been used together to describe the effect of parameterized GWD on some aspects of the thermospheric circulation. The results of this investigation in whole atmosphere modeling would be of interest to Annales Geophysicae readership. However, I think in its present form the manuscript is incomplete regarding description of the methodology and discussion of the results. As a result, the authors do not reach substantial conclusions concerning the role of GWD in the thermosphere that can be supported with the results in the manuscript. I recommend major revisions, as described in detail below. Specifically, these revisions should: (1) provide more background information on GWD and whole atmosphere modeling; (2) include more detail regarding the experimental design and numerical methods used to analyze the model output; (3) describe the impact of the GWD parameterization on zonal mean temperature in the thermosphere in addition to zonal mean winds; (4) include the impact on the diurnal migrating tide (DW1); (5) describe in more detail the better agreement with observed SW2 from EXP2. I believe these revisions are necessary in order to provide a substantial contribution to this area of research.

Major comments:

(1) It would be helpful to the reader if the Introduction briefly described the approach of GWD parameterization and the challenges presented by extending these parameterizations from the middle atmosphere to the thermosphere, where different physical processes apply (e.g., page 4 lines 1-2). It may help to first cite a basic reference describing the difference between linear and nonlinear approaches (e.g., Fritts and Alexander, Rev. Geophysics, 2003 may be one such reference) to GWD parameterization and then cite and briefly describe in more detail why the thermospheric environment requires modified or addition physical terms. The introduction should also cite other related studies involving GCM simulations of the thermosphere at high resolution (e.g., Liu et al., GRL 2014 https://doi.org/10.1002/2014GL062468) or with parameterized GWD (Becker, JAS, 2017, https://doi.org/10.1175/JAS-D-16-0194.1, or England et al. JASTP 2006 https://doi.org/10.1016/j.jastp.2005.05.006), and describe how the present study fits in with previous work. These are just some examples, the authors no doubt are more aware of recent work in this field than I am, but the point is that there is already a fair amount of research in this area that should be noted in the manuscript.

(2) The experimental design needs more description. Specifically, a. what is the model time step? b. How long was the perpetual June simulation carried out? Is this just 30 days of simulation? Figure 1 caption states only 30 days of results were averaged (June 1-30). c. Is there any spin up to the model to reach a steady state? This is important to know; is the SW2 signal steady or varying strongly with time throughout the simulation? d. Can the authors explain why is there no experiment where the “linear” middle atmosphere scheme is applied above 100 km? It is not surprising that a simulation with parameterized GWD artificially cut off at 100km will produce a different thermospheric state than a simulation with parameterized GWD extending throughout the entire model domain. In its present form, the study isn’t really telling us that the Yigit scheme is needed for a GCM in the thermosphere, it’s just telling us that more drag on the winds gives a different SW2. This is rather superficial. To be more substantial,

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it would help to know the benefit of the nonlinear Yigit scheme? What dissipation terms are important (nonlinear interaction, radiative damping, diffusion, ion drag, etc?). Running a simulation with the linear scheme in the thermosphere could answer this.

(3) Figure 1 plots zonal mean zonal wind, but not temperature. Since Figure 2 shows the SW2 in temperature, and since the authors claim the GW thermal effects are very important (page 2 line 6), it makes sense to add temperature to Fig 1 to give a complete description of the differences in the background thermospheric state between EXP1 and EXP2.

(4) Effects of GWD on SW2 are discussed, but not DW1 or other tidal modes, particularly nonmigrating tides. Why where these modes not considered? Earlier, Miyoshi et al (2014) used this exact same model at slightly higher resolution (1 degree latitude/longitude) and found that the diurnal tide is important above 200 km. Since these tides are not acting in isolation to one another, it makes sense to describe the impact of GW drag on both DW1 and SW2 at least. Please also mention in section 3.2 how the tidal amplitudes are obtained from the model.

(5) The description of the agreement between EXP2 SW2 and observations (Section 3.2, not 1.2 as in the manuscript) is not entirely convincing. SABER estimates are quoted as 15-20 K but it's not clear if this is for June conditions, over what years, or what kind of uncertainty is associated with this number. Does 15-20 K mean that is the typical range of values? Do the SW2 amplitudes from EXP1 and EXP2 vary widely over the simulation period? It would be most helpful if Figure 2 plotted SABER SW2 results as a function of latitude and altitude for June to provide a comprehensive comparison. The authors should also do the same for DW1 – that is, compare DW1 amplitude from EXP1, EXP2 and SABER.

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