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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of middle atmospheric temperature and circulation in the UIUC GCM to the treatment of subgrid-scale gravity-wave breaking" *by* F. Yang et al.

Anonymous Referee #1

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General comments:

The manuscript presents results from four simulations with an atmosphere general circulation model that includes the middle atmosphere. The focus is on the sensitivity of the modelled atmosphere to the specification of a gravity wave parameterization and to Rayleigh friction. The subject is pertinent to ACP.

Although the manuscript reports of a new middle atmosphere model, parts of the model design and of the simulation design are a bit obsolete. Therefore, a reader can wonder about why developing such a new model and not using (or contributing to the development of) existing ones. What is most obsolete is the inclusion of the Rayleigh friction

sensitivity experiment: nothing new is deduced from this exercise. In addition, Rayleigh friction is an unphysical - and wrong - way of representing gravity wave effects. It was used some time ago, but today I am afraid we do not need yet another paper on it, even less a new model with it. Shepherd et al (1996) demonstrated the spurious effects of Rayleigh friction. By design the Rayleigh friction cannot reverse the jets in the upper mesosphere: Do we need yet another simulation to show this? By design Rayleigh friction is not a forcing. All the text on it is simply superfluous.

A second disappointing aspect is a lack of modern literature concerning gravity wave representation in middle atmosphere models. The authors seem to have not read the literature that concerns the implementation of a few spectral gravity wave parameterizations in middle atmosphere models (reference list in specific comments). Therefore, their results are not discussed in the context of state of the art middle atmosphere (or further upward extended) models.

Another disappointing aspect concerns the model formation, not entirely based on new tools: In the development of a new middle atmosphere model, why using the Palmer (1986) parameterization instead of its following (possibly improved) developments (Lott and Miller 1997)? If there are specific reasons for this choice, please specify; The coordinate is sigma to the model top. Since Fels et al (1980) there are ways to pass to pressure coordinate above the tropopause, and therefore avoid spurious oscillations in the middle atmosphere; Replacement of a radiation scheme from one layer to the next appears a rough representation, why using such an approach?

The interesting part of the manuscript is the response to the Alexander and Dunkerton parameterization.

However, to show only a 8 year average of January is insufficient validation: the stratosphere is know to be highly variable in January. I would be surprised if the NH stratospheric results from the various tests are not within one standard deviation of a control case. Given that the manuscript is rather light (no new design nor methodology for the

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validation) at the least show also the July results.

For a possible publication in ACP under major revision, my requirements are firstly to remove the experiment on the Rayleigh friction (although substantial revision, that should be easy). Secondly, the Authors need to highlight the special and unique and new features of this model with respect to modern literature: Critically position their work. The Authors also need to justify the chosen model formulation and to extend their presentation to July. It would also be advisable to use URAP instead of CIRA86 and to show the statistical significance of their results. A further advise is to consider at least a 20 year simulation. It would also be good if the revised manuscript specifies what is meant with “extensive tuning” and includes a more critical discussion of the sensitivity of the middle atmosphere to the AD parameterization.

Specific comments:

P 9086 Line 5: Rayleigh friction is an unphysical: not a gravity wave forcing representation. Neither a forcing by design. Remove. Line 19: The middle atmosphere comprise the stratosphere: hence the last line of the abstract in not true (see your figure 8). Be more precise in your writing.

P 9087 Line 6: Hamilton 1997: not a good reference for topographic gravity waves. Line 12: “Recent years” actually almost contemporarily to the list at lines 11 and 12, or shortly after the mid 90s (a decade ago) are references of implementations of spectral gravity wave parameterizations: Manzini et al 1997, Manzini and McFarlane 1998, Medvedev and Klaassen, 2000. More recent references missing of middle atmosphere modes using such parameterization: Fomichev et al 2002, McLandress and Scinocca 2005, McLandress et al 2006, Schmidt et al 2006. Line 18 “different stages of testing” actually used routinely these days (see my reference list, and I am to claiming to have listed all the relevant work), although uncertainty in the specification as well as controversy among theoreticians are still ongoing. Please update your discussion to include modern literature and advances. Line 29 “Rayleigh friction is not a scheme.

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P9089 Line 26: Why using a sigma coordinate for a middle atmosphere models? Fels et al 1980 and Simmons and Stuerfing 1981 already introduced alternatives now used routinely.

P9090 Line 21: “either Rayleigh friction” remove Line 23-24: Interesting here there is a “momentum damping”: Is this an additional Rayleigh friction? Doest it act also on the mean flow? Please specify.

P9091 Line 10: “were used to replace” what about discontinuity in the heating rates? None? Please specify.

P9092: Line 2-3: Remove the Rayleigh friction case and the pertinent text. Useless.

P9093 Line 4 “now widely used” actually “no longer used” would be more appropriate. Please update your research.

P9094 Line 10 “and for different season” However this is not reported in Table 1 and discussed here. Line 14 “extensive tuning” : what do you mean exactly? How many simulations? How long? With which criteria were the tunings carried out and judged? Which months were looked at? Line 18: Use URAP. Line 26-27: remove (see general comment)

P9096 Line 4: 8 years: a bit short, especially for January. Given that and the different role of the planetary waves in the two hemisphere, please include results for July also. Line 13: Therefore, use URAP.

P9096 Line 19: In lower stratosphere the winds are not “quite similar”. To avoid misinterpretation, be quantitative. Line 22: “this 40-layer GCM”: which one? You have four formulations of it in discussion. Nowhere in the manuscript a standard model is defined.

P9097 Line 14: “tuning exercises” Be specific and details the trial done (see questions of previous comment on tuning”

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P9098 Line 1-2: Not clear what is meant with “upper mesosphere” and “lower thermosphere”. The text is correct only for the region for pressure lower than 0.01 hPa. Actually, the difference in wind strength is hard to see, what I see clearly instead in the highlighted region is a difference in the wind vertical shear within the cases. Line 9 “one of the tuning experiments”: How many experiments were conducted? Is this the best case you have been able to simulate? Which criteria used for the change of the parameters and the choice of the case? Describe the strategy of tuning (as requested elsewhere). Line 13: see comment for lines 1-2: I do consider 0.1 to 0.01 hPa upper mesosphere, and there I see an increase, not decrease. Line 19: Rayleigh friction is designed not to give a reversal. If you would have gotten one, something would have been wrong. Remove.

P9099 Line 8 “generally well”: quantify.

P9101 Equation 3: Andrews et al 1987 and the TEM formalism more appropriate. Line 19 “different forcing”: describe this in detail. Any other dissipation on the mean flow? Line 24 “sponge-layer friction”: what is meant? Does it act on the mean flow?

P9102 Line 1 “middle atmosphere” please be more precise and specify if you are describing features in the mesosphere or the stratosphere. This comments applies to various part of the manuscript.

P9103 SAO results are not new. See the suggested literature - but not only.

P9104 QBO by gravity wave parameterization only is not a good modelling approach. For a broad discussion on modelling see Giorgetta et al 2006.

I skip detailed comments on the conclusion because it would simply be a repetition at this point.

References:

Fels et al 1980, Stratospheric sensitivity to perturbations in ozone and carbon dioxide: radiative and dynamical response. J. Atmos. Sci., 37, 2265-2297.

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Fomichev et al 2002, Extended Canadian Middle Atmosphere Model: Zonal mean climatology and physical parameterizations. J. Geophys. Res., 107, 4087, doi:10.1029/2001JD000479.

Giorgetta et al 2006, Climatology and forcing of the quasi-biennial oscillation in the MAECHAM5 model. J. Climate, 19, 3882-3901

McLandress and Scinocca 2005 The GCM response to current parameterizations of nonorographic gravity wave drag, J. Atmos. Sci., 62, 2394-2413.

McLandress et al 2006, Large-scale dynamics of the mesosphere and lower thermosphere: An analysis using the extended Canadian Middle Atmosphere Model, J. Geophys. Res., 111, D17111, doi:10.1029/2005JD006776.

Manzini et al 1997, Impact of the Doppler Spread Parameterization on the simulation of the middle atmosphere circulation using the MA/ECHAM4 general circulation model. J. Geophys. Res., 102, 25 751-25 762.

Manzini and McFarlane 1998, The effect of varying the source spectrum of a gravity wave parameterization in a middle atmosphere general circulation model. J. Geophys. Res., 103, 31 523-31 539.

Medvedev and Klaasen, 2000, Parameterization of gravity wave momentum deposition based on nonlinear wave interactions: Basic formulation and sensitivity tests, J. Atmos. Sol. Terr. Phys., 62, 1015-1033, 2000.

Schmidt et al 2006, The HAMMONIA Chemistry Climate Model: Sensitivity of the Mesopause Region to the 11-Year Solar Cycle and CO2 Doubling. J Climate, 19, 9303-9331

Simmons and Struefing, 1981, Technical Report 28, ECMWF, Reading, UK.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 9085, 2006.

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