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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 #4

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This paper presents zonal mean climatologies of the extension to the mesosphere of a General Circulation Model. It has too be viewed as a presentation of this model, and it is always quite difficult to publish this kind of material. There is indeed a lot of hidden work done when preparing such a model, and in the end, one has to reproduce the work of others for the model to be trusted! This is not exactly calling for novelty. For these reasons I am in general quite indulgent regarding GCM presentation work. Nevertheless there are some directions that have been taken here, that should not have been considered in the first place. There are two of them, I consider them as major. On top of that, I suspect from the TEM diagnostics in Section 4.3 that all the work in the model is done by the parameterization of the GWs, and that the parameterization

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takes the place that should be occupied by Planetary Waves (I refer here to the result that the Gravity Wave Drag is the dominant driver of the Brewer Dobson circulation). It is known that if you put a lot of those GWs, the winter variability in the midlatitudes decreases a lot, this follows that the planetary waves amplitude decrease as well.

Major points:

The most important one is that Raileigh friction schemes are not acceptable to represent GWs. They can be used for numerical reasons only (this, or to put a lot of viscosity in the high altitudes is quite common) and in this case they should not affect zonal mean quantities (Shepherd et al. 1996). I recommand to disregard all the simulations where the Raileigh friction is used to represent GWs.

I would also be quite favorable that the authors modify their "numerical" Raighleigh damping for it not to affect the zonal mean flow. See description p. 9090, l. 17-18.

The second is that you can not just decide to stop mountain gravity waves at 10hPa. This is a non-sense from a physical point of view. If this was doing any good, I could eventually see it as an interesting curiosity. Nevertheless in reality, the NH and the SH have extremelly different climatologies, and this is in part due to mountains. Accordingly, mountain gravity waves have their contribution in this difference. In the future, there are good chances that the same authors will need them to refine their GW tuning of the NH and of the SH.

Intermediate Major-Minor:

I am surprised that the convergence of the EP flux plays such a small role in the model. What surprised me is that v* (TEM meridional velocity) is large in the NH, north of 60N in the Fig.˜6. At this latitude it can not solely be due to the Eulerian mean of v, which is actually strongly negative at 48km, 60N, January ECMWF data. It does mean that the difference v*-v is large, which means that the vertical derivative of v'T' is large as well. As this vertical derivative also enters into the EP flux divergence, I am surprised.

Here I do not mean that there is anything wrong, I just want to see more. If the GWs do all the job, I then suspect that v and v* are almost identical in the model even at high latitudes. In this case, the planetary wave are probably too small. To clarify this point, it would be very useful to visualize the stationary planetary wave number 1 and 2 as well. The North Pole 10hPa variability, a rough indicator of the Sudden Warming in the model should also be interesting in this respect.

I do not request it to be done here, but for future validation, note that you can evaluate the E-P flux from the ECMWF analysis or re-Analysis.

Minor points

Abstract, l.14-15: The sentence is not clear at this stage.

Abstract, l.18-19: Tell if this weak role of the resolved eddies is specific of your model.

p9088 l. 24-26: This type of sentence is better located in the conclusion, or in the Abstract. By the way, precise exactly which lessons you want to give.

p9090, Eq.˜1: Sigma coordinates? This is like Raighleigh friction for GWs, a little obsolete. But the author here can not be considered as responsible.

p9090, l. 21: nin–>non

p9091, l. 1-25: Details a little the transition from the LTE to the non-LTE scheme around 0.02hPa. Is it brutal? or gradual.

p9096, l. 1-25: This off line testing of the GW schemes using CIRA profiles is clear and good. From the corresponding Figs.3 you can already tell that the Raighleigh friction should be disregarded.

p9100, l. 3-7: TEM formalism is not a Lagrangian formalism, although it is an approximation of it. Sentence starting by: "Formally the residual...." Suppress it, in particular because it looks wrong, the Heat flux entering in the EP-flux divergence. It is legitimate to adopt the TEM formalism, it is sufficiently well known for the authors not to try to

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describe it (it is quite hard to do so briefly).

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