R e p l y

To the interactive comments to the paper Experiments on sensitivity of meridional circulation and ozone flux to parameterizations of orographic gravity waves and QBO phases in a general circulation model of the middle atmosphere A. V. Kova, N. M. Gavrilov, A. I. Pogoreltsev, and E. N. Savenkova. Anonymous reviewer No. 3.

We would like to thank the reviewer #3 for his useful comments. Our replies are given in bold font.

This paper describes numerical experiments using the MUAM general circulation model assessing the combined effects of observationally prescribed tropical stratosphere variability (e.g. QBO) and a newly developed orographic gravity wave parameterisation on (NH) wintertime stratosphere and mesosphere circulation and diagnosed vertical fluxes of ozone. This paper covers material which has been extensively researched over the last 30 years. As such, it really requires substantial novelty in science or the development of a new innovative model to be publishable. Regrettably, this paper has neither novel science nor new physics.

Furthermore, as outlined below, major questions are raised concerning both experimental design and the level of appropriate analysis. For these reasons this reviewer cannot recommend publication.

Many issues mentioned above were discussed in our previous papers. We also added some references on existing studies. About the useless of our studies for the community, several days ago we obtained the following e-mail message from the journal Earth, Planets and Space about our paper published just 4 months ago: "… We thought you might be interested to know how many people have read your article: Simulating influences of QBO phases and orographic gravity wave forcing on planetary waves in the middle atmosphere : the launch of Earth, Planets and Space Nikolai Gavrilov, Andrej Koval, Alexander Pogoreltsev and Elena Savenkova, Earth, Planets and Space, 67:86 (10 Jun 2015) http://www.earth-planets-space.com/content/67/1/86
Total accesses to this article since publication: 502
This figure includes accesses to the full text, abstract and PDF of the article on the Earth, Planets and Space website. It does not include accesses from PubMed Central or other archive sites (see http://www.biomedcentral.com/about/archive). The total access statistics for your article are therefore likely to be significantly higher."

In the present paper, we described our parameterizations, numerical model and some new numerical experiments, which can be repeated and extended by anyone who will ask for our computer code.

Major&comments

• Experimental setup appears flawed: why examine for changes in circulation from parameterised orographic wave forcing under QBO east and west? Large scale resolved waves, using HoltonWTan arguments I can understand, but not parameterised waves constrained to 'propagate' vertically. If there is a hypothesis which is being tested for it is not apparent in the paper.

The question is difficult to understand. We parameterized stationary mesoscale orographic waves, which are basically propagating vertically over mountain systems, where they are created. As far as we use realistic model of the Earth's relief, obtained OGW characteristics depend not only on altitude, but on horizontal coordinate also. Simulations are made for conditions of both easterly and westerly QBO phases. Planetary waves are simulated within the GCM model. Addition of stationary OGWs change the

circulation and PWs. We estimate these changes. Probably the text is not clear enough. We extended descriptions of the parameterizations.

• There is a substantial literature on orographic wave parameterisation and associated circulation impacts. It is not evident what new material this paper is adding to the literature.

The main improvement of our parameterization is accounting for the Earth's rotation in the wave energy equation. This is essential for stationary OGWs with zero observable frequencies. These improvements were described in the paper by Gavrilov and Koval, 2013. Here we just summarized the end equations required for practical implementation of the parameterization. This paper technically describes numerical experiments with the new OGW parameterization, which could be repeated and extended by everybody, who will ask our computer code. In addition, the paper contains some new information about OGW influence on the meridional circulation and ozone fluxes. We added some more references on existing literature, and extended descriptions of our results.

• Significance testing needs to be undertaken throughout.
Furthermore, it is not apparent how long the integrations are for the different experiments. Long integrations would be needed to peg back dynamical noise levels, especially in the upper mesosphere (c.f. figure 2). My impression from the figures (esp. figs 2b, 2c) is that the simulations are (too) short.

We tested every step of simulations. Section 3 says that before getting results we ran the MUAM model for 330 days. Many estimations of our group and other users of the MUAM showed that this prior interval is sufficient to get stable results in the entire middle atmosphere. The reviewer does not write why he has his impressions about figs. 2b and 2c. We do not see any inconsistencies in these figures.

• The paper does not offer sufficient analysis to describe/understand circulation features. Strangely, there is no plot showing where the orographic wave forcing is occurring. This would be especially relevant for diagnosing the driver of the summertime mesospheric circulation (and change).

We were instructed that the main GMD goals are technical descriptions of numerical models and experiments. Nevertheless, we extended physical interpretations of obtained plots. Distributions of our orographic forcing are published in our previous papers (e.g. Fig. 2 of the paper Gavrilov et al., 2015). We added respective references. In this paper, we concentrated on publishing of new results.

Presumably, orographic waves are filtered by the stratospheric easterlies found during the southern summer? Further diagnostics including zonal wind, temperature and resolved forcing should be assessed in the paper at the very least.

This is already done in our previous papers (Gavrilov et al., 2013a,b, 2015). We made references.

• The diagnosis of 'ozone' fluxes is too simplistic here. Only vertical fluxes are assessed and there is no assessment of source changes due to circulation based temperature changes.
One of the reason for the inclusion for orographic GWD schemes back in the 1980s was the alleviation of the cold pole bias in GCMs. This was especially relevant for ozone studies at the time, which are significantly affected by temperature effects (e.g. heterogeneous ozone destruction).

These issues are important for climatological models. There are also problems of diagnostic of existing ozone fluxes during relatively short (several days) observations. For such estimations, simple mechanistic models with prescribed experimental ozone and QBO distributions may be used. Sometimes this diagnostic may be even more reliable, as it is using real observed data and simulated circulation and vertical velocities are adjusted to the prescribed fields by the GCM model. We added this statement.

• It has not been shown how the inclusion of QBO and OGW effects has improved the model (MUAM) climate and variability as is purported in the text.

We added comparison of simulated meridional circulation with meterological reanalysis data in Figure 2a, and changed respective statements of the paper.

• The text would benefit from proofreading for language and grammar.

The text has been checked by a professional translator.

Other comments: (Line 12) “Earth’s”

(P5648, l17) this paragraph is little vague: please cite what climatological fields you are referring too and specifically which parameterisations are being referred to.

We added the references.

Perhaps too the first sentence could be rephrased to something like, ‘A defining characteristic of the tropical stratosphere variability is the QBO.’

The phrase is changed.

(P5648,l22) once defined, QBO should be used rather than quasi biennial oscillation.

Corrected.

(P5648, l23) Do the authors actually want to make this statement? How would an inaccurate modelling of the troposphere affect resolved waves originating from the troposphere and impacting the stratosphere (and the associated BrewerWDobson circulation). What impact will such (tropospheric) biases have on the fluxes of parameterised orographic waves across the tropopause? All important questions for this study.

The phrase is changed.

(P5648, l25) “troposphere W stratosphere” to” stratosphere”

Corrected.

(P5649, eq 1) can the authors please explicitly state what the relaxation parameter is as a function of latitude and height, rather than the Mathworks reference. Are higher altitudes relaxed more strongly than lower ones, i.e. following characteristic thermal relaxation timescales? Or does the stated 5 days relaxation time apply at all heights?

In the present simulations, the relaxation time is the same everywhere.

(P5649, l6) What is the reference for the Met office data? Presumably it is reanalysis data?

The reference is added.

(P5649, l25) The authors define the QBO phase between 30W35km. How might this choice impact their subsequent analysis? Is it the optimum height for diagnosing potential tropical impacts on high latitude variability? As the authors rightly point out, other studies diagnose lower heights in observations (e.g. Holton & Tan, 1980). Presumably (different) models may diagnose highWlatitude sensitivity to the QBO at different heights. The authors allude to this in the following paragraph but do not comment the consequences to the subsequent interpretation of their results. Comments?

This issue was discussed in details in our paper by Gavrilov et al. (2015). It was found that QBO phase classification by Pogoreltsev et al. (2014) is in fairly good agreement with that by Inoue et al. (2011). The authors of other models should compare their classifications of QBO phases with those by Pogoreltsev et al. (2014) and Inoue et al. (2011), when comparing their results with simulations of this paper. We added the reference.

(P5650, l24) "…atmospheric variables…"

Corrected.

(P5650, l27) "…exchange of energy and momentum between…" (P5650, section 2.3) Perhaps too much theory is taken from Gavrilov and Koval, 2013. The authors should simply repeat only those parts relevant to the present study and point to the reference, for a fuller treatment.

This contradicts to your previous major comments requiring more detailed descriptions of the parameterizations. We think that in this semi-technical paper addressed to programmers it is useful for them having all necessary formulae in one place. Here we repeated just 3 formulae directly used in this study for calculation of OGW drag and heat influx.

(P5651, eq 2) What is the parameter α? Normally meridional velocity is represented by v and zonal velocity by u. So maybe use u'w' etc in equation 2

α is the number of horizontal axis. We have two horizontal axes (usually directed to east and north) and two components of wind and wave drag. We can not replace two variables by only one.

(P5651, l9) remove second "of"

Removed

(P5651, l24) "…f is the Coriolis parameter…"

Corrected

(P5653, l8) "…lower…"

Corrected

(P5653, l14) remove ‘angle’, i.e. “…resolution of two minutes along…” (P5653, l17) Please rephrase first sentence; it does not read well.

Corrected

(P5653, l24) “In the simulations…and employed a timestep of 450 s. Model data was output every 4 hours”

Corrected

(P5654, l1) “Simulations were spun up from rest and with…”

Corrected.

(P5654, l3) The authors should check whether writing in the third person plural (i.e. we) is encouraged or whether writing in the passive tense is preferred by the journal.

We did not find any preferences in the journal recommendations for authors. We equally use both these styles.

(P5654, l8) What was the ‘prognostic equation’ used? Please specify.

We rephrased this sentence.

(P5654, l15) suggest replacing “…simulating the changes…” to “…to look for…” or “…to examine for…”

Replaced

(P5654, l21) It is suggested that the last two sentences, beginning “The differences in…” be removed as they are selfWevident following the preceding sentence.

We excluded the first phrase and reformulated the second one. We think it is useful, because from our experience people in different countries may treat the signs of the differences by different ways.

(P5655, l6) Can the authors add statements of significance to this statement, thanks. Presumably as reference is being made to the (relatively small) circulation features in figure 2a, there would be reduced significance (i.e. perhaps consistent with our ideas of the residual mean circulation at these heights but not apparent here, say, using a StudentWT test?).

The statistical significance of relatively small differences is one of key issues of this study. Other reviewers made comments about nonsufficient descriptions of the statistical analysis. Therefore, we prefer to keep this short description (15 lines) so that anybody can re-estimate the statistical confidences.

(P5655, l9) “ascent”

Corrected

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