

ACP review:

Ref#3 comments in italic:

This response is concerned with a general response to some important topics raised by the referee. A point to point response including a detailed list of changes will come later.

At first, we would like to thank the referee for pointing out the paper Cohen et al. (2014). This paper will greatly help us to generalize our results in future work.

However, our analysis was motivated with the possible effect of the EA/NP GW hotspot and only during the work we realized that it can have also general consequences in contrasting the effect of zonally symmetric and asymmetric torques.

It is tempting to repeat the methodology given by Cohen et al. (2014), but we would rather leave it for future work, while keeping the main focus of our paper on the analysis of the local effects of the GWD injection. We will refer to Cohen et al. (2014), and also to Shaw and Boos (2012), who, for example, found the same pattern in the vertical velocity response (with downwelling to the northeast and upwelling to the southwest from the zonally asymmetric torque), although in the troposphere.

Below is our response to selected referee's comments that are given in italic.

1A) The results of the paper are based on a number of 30 day simulations with the MUA (Middle and Upper Atmosphere) Model. In the real atmosphere, there is substantial natural variability, and results based on a single 30 day snap shot would likely be meaningless in a statistical sense. I suspect that this model does not have much natural variability – otherwise the authors would not be able to conclude much from such short runs – but that is unclear in the current paper, which provides little insight into the background flow and no discussion of the statistics. To remedy this situation, I recommend first establishing the quality of the model, better characterizing its January climatology (for example, showing the zonal mean wind as a function of pressure and height) along with the variability (for example, the variance of the zonal mean winds). Does this model vary much at all, or is it essentially steady, seeking only to capture the climatological mean circulation. It would also be good to show the overall impact of the gravity wave drag (GWD) scheme. A panel/overlay showing the zonal mean drag as a function of pressure and latitude might help, too, giving us a better sense the background gravity wave driving.

This is right, because a steady state is nudged at the lower boundary, MUAM does not contain day-to-day variability and captures the climatological mean circulation. The only variable component are the solar tides. We agree that the quality of the MUAM's January climatology needs to be demonstrated. We will add additional figures in the section 2.1.

Then, how sensitive are these quantities to the forcing? At the top of page 4 the authors suggest they force planetary waves 1, 2, and 3 from ERA-I reanalysis at 1000 hPa. Do they mean climatological waves (based on what period)? What would happen

if you took the waves from a given year? My concern here is that the authors need to establish that their results are robust, and wouldn't change dramatically if the climatology is altered. Varying the lower boundary would allow them to sample the natural variability of the real world; in his case they would need need to run a number of simulations for each case, and could assess the statistical robustness of their conclusions.

We agree with this comment. The overall response will be different for different background conditions. But there is a question, how reasonable it is to compare the responses of different background states to the same GWD. Naturally, each different background state means different GW sourcing and propagation conditions that should result to a different GWD. With our artificial enhancement approach we are not able to reflect this. Instead, in our simulations we have chosen a different approach. For mean January conditions we inject GWD settings stemming from our estimation based on observations (and from uncertainties especially in its meridional component) and from the mean climatology in the EA/NP region. In our paper we wanted to shed the light on the acting mechanisms and patterns of local response (e.g. build up of a positive geopotential anomaly upstream from the GWD, obstacle analogy..) that we assume will remain valid also for slightly different conditions.

Actually, we have also made simulations with slightly different background climatology and the results were almost similar (absolute agreement in the pattern, only the magnitude was slightly different in some places). We would not like to show the figures in the revised manuscript, because we think that it would have little additional value, since in our manuscript we are not exactly concerned with the precise magnitude of the response.

1B) And finally, all figures need to acknowledge the statistics. I don't mean to be the curmudgeon who rants that a result without an error bound isn't a scientific result, but you do need to either estimate the statistical certainty, or explain that everything that is shown is robust, given the lack of variability in the model.

We absolutely agree. In a revised manuscript all of the mean fields will be overlaid with stippled areas with the significance exceeding the 5% level (p- value estimated using the t-test). From Fig. R1 it can be seen that e.g. for the mean EPflux difference between Box0.1 and Ref simulation the area of most important differences, which were discussed in the text, is significant.

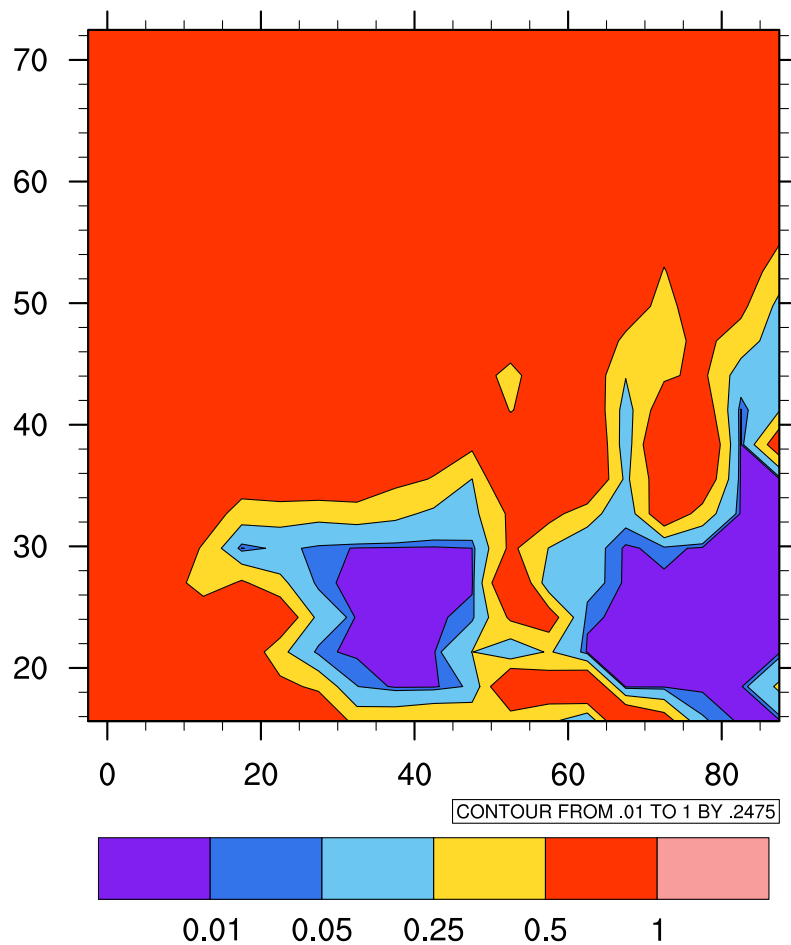


Fig. R1:Plot of p-value computed by t-test. On the x-axis is latitude and on the y-axis is a log-pressure height in km.

2Aa) Following up on my first concern, it is unclear to me what these experiments are seeking to represent. Many figures (e.g. 2, 4, etc.) show the 30 day mean, which initially suggested to me that the goal was to demonstrate the steady response to the wave driving (which I presumed had occurred over this time scale). But it was not until the Fig. 5 that I realized that the response had clearly not converged over this period!

The response to each artificial GWD in each simulation is unsteady to some extent in the sense that it constantly radiates the oscillations (their nature is discussed later in this response), but this effect can be canceled by averaging. On longer scales we can see some background evolution in response to the GWD, supposedly connected with PW creation. Previously in the manuscript, we felt that the 30day average is somewhat artificial in this respect and therefore we tried to always supplement the averaged results with information of time evolution (Fig. 5), animation (Fig. 3, Fig. 4, Fig. 6, Fig.7) or discussion on time evolution within the 30 day mean.

Following the comments made by the referee, in the revised manuscript we will at first establish from Hovmöller diagrams the time interval, which can be considered

as steady (from ca 6 days to the end of simulation, see Fig. R2). This will allow us to compute mean responses, but only for simulations with -0.5 and -10 m/s/day artificial GW induced zonal acceleration. For -70 m/s/day the simulations do not reach a steady state during the simulation. The responses in those "SSW simulations" were always presented as snapshots in selected time steps or as animations only. In the revised version, in addition, we will present time evolution of zonal mean zonal wind to prove the SSW like nature of the vortex events.

Hovmoeller plot of zonal mean zonal wind at 6hPa

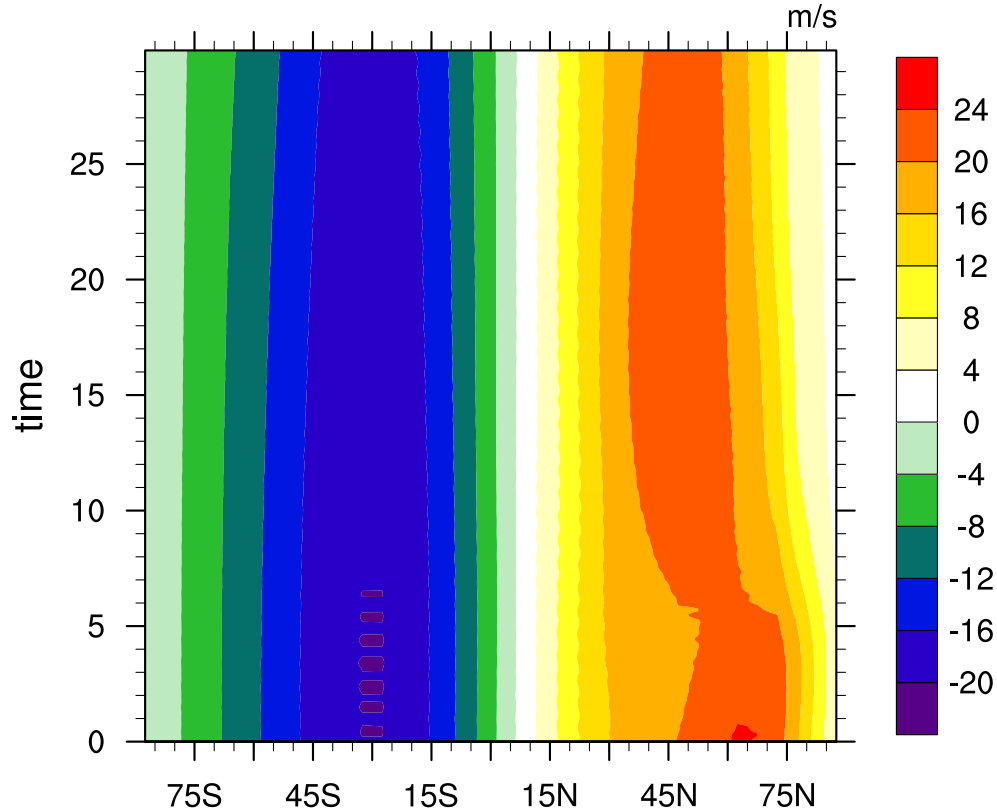


Fig. R2: Hovmöller plot of zonal mean zonal wind from 10box simulation.

2Ab) Gravity waves in the real world tend to be episodic and highly intermittent, so that the short term response is highly relevant. But if the goal is to capture the short term response, then I think the paper needs to focus on this from the start, and establish the appropriate time scale early on. This could be done, for example, by showing a Hovmoller diagram of some key quantities, such as the zonal mean wind at 6 hPa (or another key level) as a function of latitude and time, along with the evolution of the key zonal harmonics (as in Fig. 5), but again, plotted as a function of latitude and time. The goal would be to show that the key change(s) occur on a timescale of X days (where X is with hope < 30 days!), establishing that a short 30 day run is sufficient for the study. And then subsequent figures could focus on the key time period(s). I say periods because Fig. 5 hints that there is some oscillatory nature to the response.

As written above, we will present Hovmöller diagrams at the start of the Results section. Averaging will start ca 6 days after GWD injection, when the large-scale response is building up (note the agreement with the time scale of the transient response build up in Cohen et al. (2014), where it is related to the life time of PW breaking).

2Ba) I still worry, however, that the short term response may depend a lot on the initial condition as discussed above. For example, in the real world, the propagation and breaking of gravity waves will be very different if the polar vortex is very strong vs. very weak (i.e. after a Sudden Warming). So one ideally would want to sample over different background states to robustly establish the short term response. [I assume the authors are forcing the model with some climatological mean wave forcing, but would it make a difference to use waves from a given year, etc.?)

This is true, but the problem is that our GWD injection (value and ratio) is constant, not taking into account the background. With a little exaggeration we can say that for each time step in each ensemble member we would need another GWD enhancement values and only then we can sample for a robust response. We think that the presented results stemming from injection of constant GWD (estimated in accordance with the mean background conditions) are good enough to analyze the nature of the response as well as the discrepancy between effects of localized and zonally symmetric forcing.

2Bb) I do appreciate that the authors have provided information about the time evolution in supplementary videos, but I feel that the time evolution is vital to the paper, and can't be left in the supplement.

At this point, we have to acknowledge the referee's suggestion to use Hovmöller plots. By presenting lat-time plots of the zonal mean variance and of harmonic amplitudes we can replace Figure 5 and Animation 2. Also, by presenting the time evolution of zonal mean zonal wind for SSW simulations we can give a more detailed information about the polar vortex evolution, not relying on the animations only.

*3) I think it would help the paper to organize around key scientific question(s) and results. The discussion/conclusion section was more a discussion of other papers, and left me a bit confused as to what **this** paper was trying to say. In its current form, the paper comes across as a bit descriptive, e.g. we tried this, and this happened. I appreciate that this is how science often moves forward, but in the conclusions, I urge them to step back and summarize how these simulations do give us new understanding.*

I really think there is a lot of potential material here, just that the authors need to better focus the paper. Here are two key areas that could be the main result – just one or would be sufficient – and I don't mean to restrict the authors to these points.

We would like to keep the structure unchanged. This means division into polar vortex, PW and residual circulation response sections. But, thanks to the referee's comments, we will now make it clear in the paper, where, how and why the steady state and the transient response is analysed.

The discussion section will be revised and a clear summary of results will be given in the start (to satisfy Ref#1 and #2 as well). Otherwise, it is probably true that the paper can be marked as descriptive - in the sense that we are more concerned with the qualitative than the quantitative analysis of our results.

*(3a1) Based on my own interests, I was particularly excited about the zonal mean response to zonally asymmetric wave driving. Given that downward control indicates that the time mean residual circulation depends only on the zonal mean wave driving, one might think that the zonal structure of the gravity wave driving should not matter. But since zonally asymmetric GWD induces a response in resolved waves, the *total* zonal mean wave driving depends very much on the zonal structure of the GWD. To show this, downward control analysis and more discussion of the compensation and interaction between resolved and parameterized wave driving would help.*

As written at the beginning of the response, we consider a more generalized experiment at a later stage (we have a three-years grant for this research). At this moment, we have to leave it for a future work (near future), also for following reasons:

To precisely imitate the methodology of Cohen et al. (2014) it would mean to produce a set of new and very long simulations (with reduced nudging, probably) and we are not able to manage it in the period reserved for the manuscript revision. Also, for a precise estimation of the compensation ratio we would have to modify our method of GWD enhancement (e.g. to enhance the GWD by multiplying the GW parametrization output in a box). Because by prescribing a constant GWD we are missing an important and very quick response of GWs to the modified PW field. We will discuss it as a weakness in our qualitative approach, but this would probably severely harm any quantitative results.

For your interest, we show in Fig. R3 the degree of compensation computed for the response to a localized -0.5 m/s/day artificial GW induced zonal acceleration and -0.1 m/s/day meridional acceleration (Box0.1 simulation). The ratio shows almost a perfect compensation in the southern part of the artificial GWD area, but a strong amplification in the northern part - in accordance with the E-P flux divergence anomaly (Fig. 4) in the manuscript. The ratio (Fig. R3, bottom) of degrees of compensation between Box0.1 (C=0.01) and Zon0.1 (C=0.07) simulation shows (the sum in the title) that the degree of compensation is in a sum across the domain stronger, because there is less amplification in the Zon0.1 simulation. In the ratio (Fig. R3, bottom) emerge also relatively large differences in the distribution of compensation in the regions with almost zero compensation (small number/even smaller).

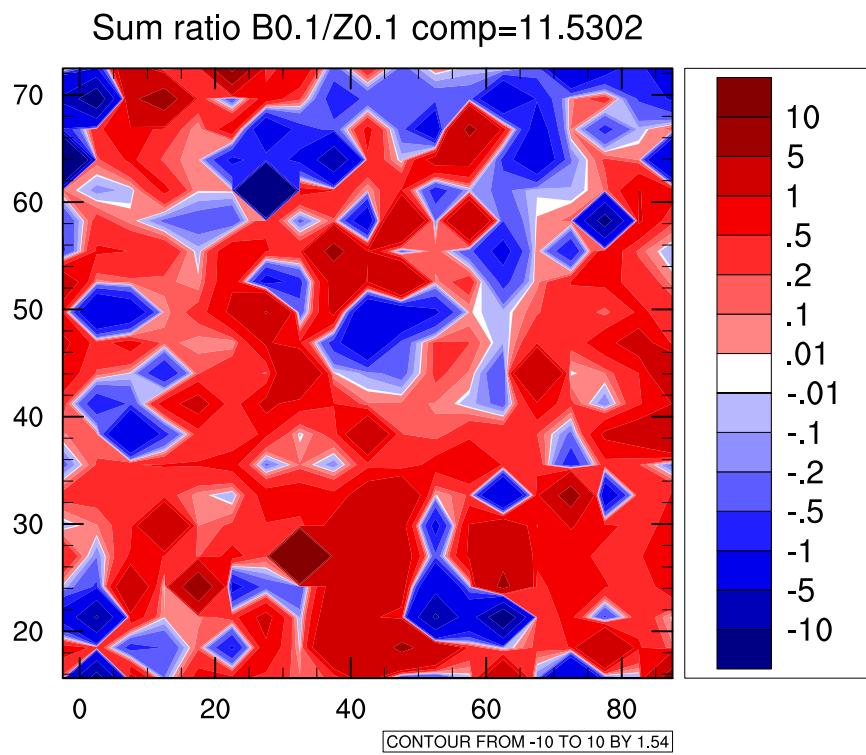
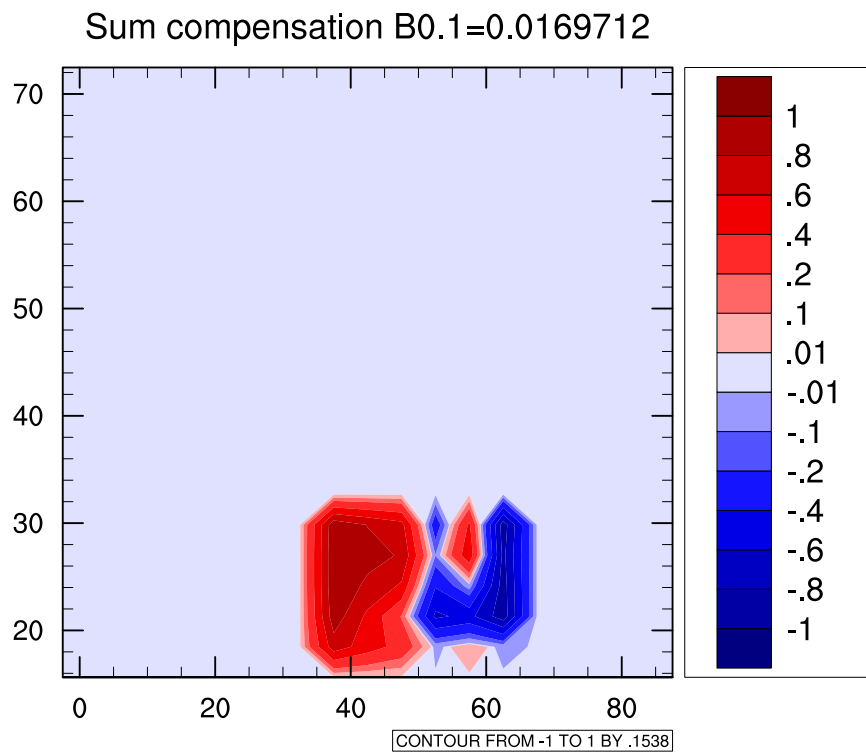


Fig. R3: Top: Lat-lev distribution of the mean degree of compensation for Box0.1 simulation. Bottom: Lat-lev distribution of the degree of compensation for Box0.1/ degree of compensation for Zon0.1 simulation.

(3a2) The authors acknowledge that nudging might limit the zonal mean response, and so drive compensation by itself. Initially I thought the nudging was done to "improve the troposphere", but upon re-reading, I realized it extends to 30 km, fairly deep into the stratosphere! How strong is the nudging in the stratosphere? Can you estimate its effective amplitude, and compare it to that of the applied gravity wave driving? Downward control can still be applied, but you just need to account for the torque produced by the nudging.

We shall take this into account. Generally, the extent of nudging is a main reason why we analyze the response at the altitude around 35km and not at some level where GWD is enhanced.

(3b) Another important and novel key result could be the impact of localized GWD on the overall resolved wave structure, following up on the Holton (1984) result that asymmetric GWD generates planetary waves. In this case, I think the time evolution of the flow is much more important. The key would be to establish how fast the resolved flow responds to the gravity wave driving, the dependence on the background state, the linearity of the response, and so forth. These results are in the paper, but I just feel they get lost in the discussion at the end.

Note that result (a) is more about the steady/climatological response, while (b) would be more about the time evolution. Once you know the targeted result, earlier figures could help lead the way.

In the revised manuscript, using Hovmöller diagrams, we are now able to better establish the time scale of the response. But, a detailed analysis of the dependence on a background state is impossible with the concept of artificial injection, as discussed under comment (2Ba).

Regarding the linearity, we do not have enough simulations to perform a robust linearity test (e.g. it is sure that there will be some bifurcation value of GWD). But, from a simple comparison of a geopotential response to -0.5m/s/day and -10m/s/day localized GWD (Fig. R4), we can see that, globally, the response cannot be regarded as linear (the ratio is variable in space).

Locally, we found near linear response between these two simulations in our results -e.g. the strength of the induced positive geopotential anomaly upstream from the artificial GWD area. But this comparison can be misleading, since both simulations (10box and Box0.1) have the same value of meridional GWD component. This means that the drag force has different orientation between these two simulations!

With certainty we can say that the response to the strongest injection of -70 m/s/day in our SSW simulations is fully nonlinear, as it contains creation of new pressure structures etc.. In the revised manuscript we will always take a great care when mentioning terms like positive or destructive interference .

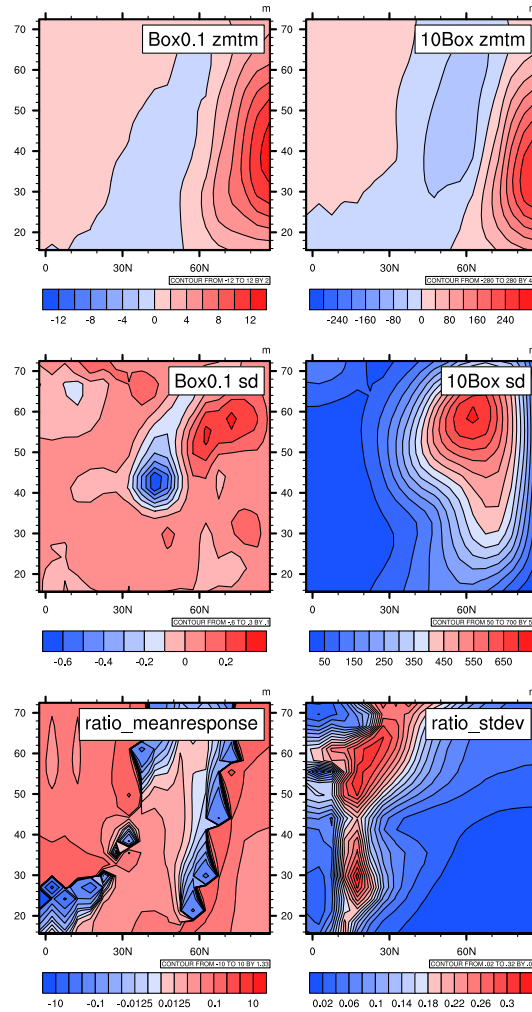


Figure R4: First row: Mean geopotential height difference between Box0.1 and Ref simulation on the left and 10Box and Ref simulation on the right. Second row: Corresponding standard deviation. Third row: Ratio between Box0.1 and 10 Box geopotential mean difference on the left and ratio of standard deviations on the right. Box0.1 simulation is performed with local enhancement of artificial gravity wave induced zonal acceleration by -0.5m/s/day and 10box with -10 m/s/day . Both have the same local enhancement of artificial gravity wave induced meridional acceleration (-0.1m/s/day).

4) Overall, the presentation of the paper needs to be improved. Small things, such as keeping the names of the simulations uniform and avoid non-standard acronyms (e.g. "gcu, gcv, gt"), and keeping the orientation of the latitudinal axes constant, really do help the reader. I appreciate that the first author is a student, and when I look back at my first papers, I'm embarrassed by the barely perceptible contour lines and tiny font size of the figures. So please take the comments below as suggestions on how the presentation could be improved, not as an attempt to be overly critical.

And as will come out in the detailed comments below, I think the paper relies too much on supplementary material. In my opinion, it's okay to have additional figures/movies for the curious reader, but all the key results of the paper should be within the paper.

We will follow the specific comments from all three referees to improve the presentation. Regarding the role of supplementary material, in the revised manuscript its importance is diminished, as some information on time evolution is now available from Hovmöller plots. Also the residual circulation part is restructured - plots showing tracer distributions are moved into the Supplement for motivation to study EA/NP only and this makes additional room for Figs. S1 and S2 (or their modification) directly in the text.

Specific comment: 9:27-35 I believe that the inertial gravity waves generally don't have a period of 1 day. The frequency is related to the Coriolis parameter, and so a function of latitude. Gravity wave frequencies is bounded between the Brunt- Vaisala frequency N and the Coriolis parameter f , with interial or near inertial waves coalescing at $f = 2 \omega \sin(\text{lat})$. At (for example) $50^\circ N$, it's 16 hours, and it will only will only be a day at a single latitude in the subtropics.

More generally, why would you expect the forcing to radiative inertial gravity waves? There's definitely something odd here. If the forcing is causing instability, I'd be quite worried about the ability of the model to resolve it, given the coarse resolution.

We would like to thank the referee for noticing this. We have made a thorough analysis of this oscillating anomalies, coming to the conclusion that the reason is most likely a non-linear interaction between anomalously generated inertia GWs and solar tides (see e.g. Walterscheid, 1981). We have three supporting arguments for such a hypothesis: **1)** the oscillations are present in the SH and NH except polar region (January conditions - no tides at NH polar region). **2)** From NH midlatitudes to SH midlatitudes the oscillations have prevailing period around 24h, but going towards SH polar region the dominant period decreases between 12 and 8 hours. This is consistent with the fact that only inertia GWs with periods lower than f can propagate towards the pole. **3)** We can observe a decrease of longer anamouous wave modes (lower than 10 cycles/Earth perimeter) when going to the SH polar regions

More generally, we expect the forcing to constantly perturb the predominantly zonal flow and so causing particle displacements. As a result, broad spectrum of waves can radiate from the region in dependence on what kind of waves are allowed by the background conditions. We considered especially the inertia GWs, because their modes can be long enough to be resolved by the model, while propagating sufficiently quickly (Fig. R5) and predominantly horizontally to be accountable for the oscillatory pattern.

References:

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