Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-465-RC3, 2020


Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Stratospheric gravity-waves over the mountainous island of South Georgia: testing a high-resolution dynamical model with 3-D satellite observations and radiosondes" *by* Neil P. Hindley et al.

**Anonymous Referee #3**

Received and published: 31 July 2020

This paper describes a comparison of various parameters of gravity waves (GWs) in the region of the South Georgia Island (South Atlantic) during wintertime as deduced from AIRS and real-date simulations using a regional circulation model. The papers is well organized and in most parts very well written. The figures are mostly of very high quality too. Furthermore, I believe that this is very worthwhile study showing the level of consistency that can be achieved in high-resolution models when compared to observational data. The reason for recommending major revision is due the fact that the authors do not sufficiently interpret the shortcomings of their model results. I believe



that the paper needs to be significantly improved regarding this aspect. The simulated very high GW amplitudes at small scales are suspicious. This may be related to a too coarse vertical level spacing in relation to the very fine horizontal grid. My major and minor comments are listed below in chronological order.

Major comments:

L169-176: The vertical resolution applied in the model is extremely coarse related to the horizontal resolution. The vertical grid-spacing is 0.6-2 km in the stratosphere, versus a horizontal grid-spacing of 1.5 km. This vertical grid-spacing in the stratosphere in not even sufficient to simulate a self-induced QBO in GCMs. More importantly, GCMs with explicit simulation of GWs (e.g., Watanabe et al., 2008, JAS: General aspects of a T213L256 middle atmosphere general circulation model) employ a vertical level spacing of 300-600 m throughout the middle atmosphere while the resolvable horizontal wavelengths in these models are of the order of 200 km. The necessity for a small enough vertical grid-spacing derives from the fact that the GWs resolved by the horizontal grid must not be spectrally biased in the vertical to too large vertical wavelengths. Indeed a too coarse vertical resolution artificially prevents the GWs from reaching dynamic or convective instability and thus being dissipating by the model's turbulent diffusion scheme.

L176-178: I do not find this statement very conclusive. The grid-spacing of a model as such does not say anything about the scales that are reliably resolved. It is the dynamical core (spatial resolution, numerics) combined with the subgrid-scale diffusion (either explicit or implicit) that determines the reliable scales of a model.

L193-196: See my 2 previous major comments and consider reformulation.

L137-348: When the model data are interpolated to a 15 km grid, the Fourier components with horizontal wavelengths shorter than 30 km must be filtered out beforehand to avoid aliasing errors from the scales below the 15 km grid. Did the authors apply this spectral filtering before re-griding the model data (for model and model-as-AIRS)?

If yes, please mention this point in the text for the sake of clarity. If not, the resulting aliasing could be an explanation for the high power in the GW amplitudes and in the MF at horizontal wavelengths of 30-40 km (e.g. Fig. 16a). In that case you might consider a substantial revision and re-submission of the paper.

L399: The authors should not only mention that model-as-AIRS produces too small amplitudes compared to AIRS, but also that the GW-phases of the MWs over the Island differ significantly in the two data sets (Figs. 4 and 5). Moreover, the slopes of the phase lines from x=100 km to 600 km in Fig. 4 differ in sign(!); that is, these GWs must propagate in different directions when comparing model-as-AIRS to AIRS. Please mention and discuss these dissimilarities.

L429-430: See my comments above: The horizontal structures in model-as-AIRS and AIRS are at best qualitatively similar over the mountain; they are dissimilar farther downstream. Please describe your comparison of results from model-as-AIRS and AIRS consistently with your high-quality figures.

Fig. 7: How did you apply averaging over the GW scales when calculating the MF. Furthermore, the regions of phases going upward with increasing x in Fig. 4c and f should give rise to a reversal from westward to eastward MF in Fig. 7c. Please clarify.

L489-493: The wave refraction argument can be applied for either upward propagating GWs (negative vertical wavenumber) or downward propagating GWs (positive vertical wavenumber). Here you apply this argument even though the longer vertical wavelengths that you expect for a westward MW in an increasing stratospheric eastward jet show up in your plot with reversed sign. How do you explain the reversal from negative to positive vertical wavenumber at 20-30 km in Fig. 6d? Why is there a noisy mixture of positive and negative vertical wavenumbers in Fig.6h? These wavenumber (wavelength) results need to be revisited.

L510-515 and L528-L532: This discussion relates to my previous comment. Please give a hint on why you possibly have positive vertical wavennumbers in AIRS. One



possibility is that the background wind in the lower atmosphere shows acceleration/deccelerations which can cause the phase lines of MWs sloping upward/downward in time-height cross-sections. Another possibility is the generation of secondary GWs from MW breaking causing downward propagating GWs (which are no longer MWs). See also Vadas and Becker (2018, JGR Atmos.: Numerical Modeling of the Excitation, Propagation, and Dissipation of Primary and Secondary Gravity Waves during Wintertime at McMurdo Station in the Antarctic), as well as Vadas et al. (2018).

L599: Note that the wind in the lower troposphere is crucial for MW generation, while the wind at higher altitudes facilitates propagation (strongly eastward) or dynamical instability (weakly eastward or westward). Again, it is unclear how dynamical instability (including critical levels) are handled by the model, given its coarse vertical level spacing in the stratosphere and the lack of information about subgrid-scale processes.

L619-623: This is another example of a very speculative discussion about suspicious features in the model data. Are stationary, non-orographic GWs indeed present around the island in the global model? Are these waves artificial? Please clarify.

L638-645: How is the simulated very large MF at scales close to the horizontal grid-scale possibly related to the coarse vertical level spacing and, in addition, to insufficient parameterization of dissipation processes in the stratosphere below the sponge layer? Your model results would imply that the vast majority of MW momentum flux resides at horizontal scales not even observable by AIRS. Hence, according to your model results, observations from AIRS are essentially useless to estimate the orographic GW MF from small Islands that is missing in global models? Please clarify.

Fig. 15: This is a very nice figure (like most of the other figures)! I cannot see the grey lines mentioned in the caption. My comment is this: The AIRS curves nicely indicate wave dissipation from about 25 km on. This wave dissipation is not reflected by the model results. Therefore, this figure rsupports my major concerns about the model: Too large vertical level spacing combined with possible shortcomings in subgrid-scale

parameterization leads to insufficient dissipation.

L858-868: Ditto.

Fig. 16a: This figure suggests that you would get a reversed power spectrum of the wave amplitude with respect to the horizontal wavenumber, i.e., increasing (instead of decreasing) power with increasing wavenumber? Please check. If this is so, this would imply that the model results at these small scales are not reliable at all.

L927: Ditto

L978-981: As mentioned above, it is not just horizontal grid-spacing (and model numerics, as you mention in L992) that determines how well a model simulates GWs. You have to consider the vertical grid spacing as well. Most importantly, inviscid fluid dynamics cannot handle GW breakdown and wave-mean flow interaction. You need an explicit dissipative process for non-transient wave-mean flow interaction (see the non-acceleration theorem, Lindzen's GW saturation theory, or the classical McFarlane paper about orographic GW parmameterization). That is why the parameterization of subgrid-scale processes (turbulent diffusion) is very important in any GW-resolving circulation model (e.g., Becker and Vadas, 2018).

Minor Comments:

L73-75: I agree with this statement. However, the authors miss the opportunity to put the orographic GW momentum flux from South Georgia into the context of the general circulation in SH winter.

L92: Please point out that the model used in this study is a real-date regional model that is forced by a global forecast model via lateral boundary conditions. Therefore, this regional model is not "essentially free running".

L135: Please be specific whether the vertical resolution relates to wavelength or grid-spacing.



L179: The vertical resolution of the global model is presumably too coarse to represent inertia GWs in the stratosphere. This could be the reason why the regional model misses these waves when compared to the AIRS data.

L205-214: This paragraph is hard to follow and distracts a bit from the very good writing otherwise in the paper.

Figure 2: Please plot the zonal wind with the same color coding as the meridional wind (blue for minus, red for plus)? Can you use a nonlinear color scale to make the accelerations and deccelerations of the tropospheric wind visible? Note that the wind in the lower troposphere determines the forcing of orographic GWs.

L245: The radiosonde observations do not provide a horizontal average over the domain covered by the model. Please reformulate correspondingly.

L266: Figure 3 is very well composed. However, Fig. 3g illustrates that the simulated winds are not in good agreement with the radiosonde data. Rather, the agreement is only reasonable. The mean meridional wind in Fig. 2d is predominantly southward from 30 to 60 km and is of the order of a few -10 m/s. The corresponding wind in Fig. 3d shows a bias of about 10 m/s.

L279: Short-timescale variability would average out when comparing time-averaged wind profiles. I suggest to accept these discrepancies and to discuss the possible implications for orographic forcing and vertical propagation of GWs in the model.

L284-L290: See my comment with respect to L226 above.

L300-306: The differences between model and radiosonde data are not minor. Invoking the "climatological level" of simulated wind in case studies of orographic GWs, which are subject to extreme intermittency, does not sound conclusive.

L360-363: These sentences are hard to understand (e.g., "vertical resolution for that vertical layer"). Please reformulate.

L377: This statement is not conclusive. What about model errors?

L470-471: A "reasonable apparent similarity" is not observed when considering the dissimilarity of individual phase lines between the two data sets in Fig. 6a and e.

page 25: Why is this new section called "Results". The previous Section 3 contained plenty of results, not just methodology.

L535-538: This description of secondary GW generation from MW breaking does not seem consistent with the aforementioned papers by Vadas and coauthors.

L539-542: This sounds very vague. I recommend to simply discard speculations of this kind. Furthermore, if you want to discuss secondary GWs in your model, then you need to consider how the model simulates dynamical instability and dissipation of resolved GWs and, hence, the necessary body forces for secondary GW generation. As discussed earlier, the very coarse vertical resolution of the model combined with the lack of knowledge about the built-in (presumably implicitly numerical) dissipation casts doubts on whether the model reliably simulates body forces from GW dissipation in the stratosphere.

L553: Note that this equation holds strictly only for a monochromatic GW or, at best, for a narrow spectrum of GWs. As soon as you have a broad spectrum, the wavelengths to be used at the rhs become arbitrary. More importantly: I am missing the Reynolds-type average of (T')**2 (see my comment on Fig. 7 above). Please clarify.

L582-585: This information clarifies my previous comment at least for Fig. 8-10. Given the size of the island relative to the model domain and the GW scales in AIRS and model-as-AIRS, you use the area-average to compute the MF. I think that is the right choice here. How would the resulting MF contribute to the zonal mean parameterized in global models?

L588: I can not see the red markers in Fig. 8-14.

L681: Again, I disagree that "observed and simulated wave fields are quite similar". As



mentioned earlier, there are even qualitative differences.

L700: What about spontaneous emission from the upper tropospheric jet stream? See Plougonven and Zhang, 2014, Rev. Geophys: Internal gravity waves from atmospheric jets and fronts.

L827-829: These differences could simply result from errors in the background wind (driven by the global model) in the lower troposphere, leading to errors in orographic forcing of MWs in the model. I believe the authors should discuss this role of the tropospheric winds somewhere in the paper.

L845-849: See my previous major and minor comments regarding the obvious and possible shortcomings of the model.

L928-935: It is hard to follow these arguments. Of course, MWs can be forced by non-stationary background winds. Furthermore hourly fluctuations of the background wind would correspond to non-orographic GWs that you force at the lateral boundaries. Your discussion of possible reasons for the model shortcomings (see also L936-940) do not mention the concerns that I raised above.

L946-947: This sentence seems not logical. Consider reformulation.

L949-950: "not so commonly"? Which observations are you aware of that show this feature of very large MW amplitudes in the stratosphere at very small horizontal scales?

L954-955: Why should intermittency of MW forcing give rise to shorter horizontal wavelengths than stationary forcing? Usually, the structure of the topography determines the spectrum that can be forced.

L963-964: Now you argue that an "overly-stable wind vector" could give rise to the high power of MWs at very small scales in models.

L993-997: I think that here you reveal a misconception about semi-implicit time stepping in circulation models. Semi-implicit time stepping is applied to suppress the artificial generation of very fast anelastic waves and sound waves; otherwise, smaller time steps would be required for numerical stability. In any event, the time step is always small enough to properly resolve the time scales of anelastic GWs that are well described by the representation of the model equations in gridspace.

L999-1000: Here you finally come up with a critical comment about the lack of dissipation in the model stratosphere.

L1002: You did not run the model at very high spatial resolution. Your vertical resolution in the stratosphere is much coarser than even in GW-resolving global models run at moderate horizontal resolution (e.g., Sato et al. 2012., JAS: Gravity Wave Characteristics in the Southern Hemisphere Revealed by a High-Resolution Middle-Atmosphere General Circulation Model). Again, your coarse vertical level spacing is certainly not adequate to support your very high horizontal resolution.

L1005: As long as we do not solve the (viscid) Navier-Stokes equations with a resolution of 1 cm in the troposphere, the performance of our circulation models will always depend on how unresolved (subgrid-scale) dynamical processes are parameterized.

L1022-1023: Yes! See my comments above.

L1030: You did not perform sensitivity experiments using the same model with different horizontal resolutions.

Typos/suggestions:

L28: play a key role

L35: recognized that GCMs

L46: match measurements

Caption to Fig. 1: and descending (c) overpass

L227: stronger than what?



L228: exceeding

L244: a meridional wind reversal

L269: cancel "In both directions"

L301: in near surface winds

L359: AIRS measurements regarding the horizontal wavelength, that is

L420: observations and for the model and model-as-AIRS, respectively.

L464: applied to the

L497: whose intrinsic horizontal

L506: upward propagating

L526: discard “striking”

L547: and constrains

L548-549: momentum flux from mountain wave sources at isolated small islands is an important area of current research

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-465, 2020.