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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mountain-wave induced polar stratospheric clouds and their representation in the global chemistry model ICON-ART" *by* Michael Weimer et al.

Anonymous Referee #2

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This is an interesting, timely and novel study, examining the ability of the ICON-ART chemistry-climate model with its flexible grid to resolve mountain waves over the Antarctic Peninsula and examine the impacts of this on PSC amounts and chemistry. The strength of this work is that it examines the impacts (of the inclusion of mountain wave-induced temperature fluctuations) on PSC chemistry, which has never really been done in such detail before. Consequently, the results/figures are very interesting, well thought out, and convincing - with a lot of time having obviously been spent preparing them. However, the interesting results are rather let down by the quality of the writing, which is in places is rather difficult to follow and unconvincing, and just not nearly polished/good enough. This is detailed below, but fundamentally some rigorous editing is

needed to address this - and to shorten the paper, as it feels over long. Additionally, the outstanding results were when the global model results were compared with the regional/40km results, but this comparison was not always made. I think the study would be much stronger if this deficiency was addressed. Addressing these suggestions (and the comments below) should be very achievable by the authors, and I hope will result in a much stronger and more impactful paper, that the novelty and importance of the results merits. In summary, this paper includes some very good results that will be of strong interest to ACP readers, but at the moment it will require major revisions before it is acceptable to be considered for publication.

Major comments

+ As it stands the paper is rather awkward to read and rather disjointed and would really benefit from some quite rigorous editing to make it more concise. I have given many examples below, but they are far from exhaustive. For example, at times the text is much too 'wordy', making it difficult to follow and unconvincing (see e.g. Section 3). The description of the results could also be tighter and more concise. The various sections also aren't well linked and the reader is never sure of the scope of the work. These issues really affect the readability/coherency of the paper. If you properly address these issues the paper will be much stronger and convincing and polished.

+ The Introduction seems rather limited and patchy, and not as coherent as it could be. I've given some suggestions as to how it could be improved below. But an obvious one would be to expand on why the Antarctic Peninsula has been identified as the focus for this case study, as well as more mention of other orographic hotspots in both the southern and northern hemispheres to reinforce the importance/impact of this work. Currently, we get to the final paragraph of the Introduction with little idea of what the 'description of the simulation' is, what model results are examined (PSC chemistry, GWs, dynamics?), or what satellite data is used in the study. I understand that this is elaborated on in the later sections, but these items also need to 'introduced' so that the paper is coherent, stronger, and easier to read. See also the comment below on lines

#252-255, which is the sort of information that should be included here.

+ Line #54-61: I think that more information is required in this paragraph, especially on the difference/increase in resolution between the global domain and the refined grid in ICON-ART, and whether the refined grid is therefore suitable to resolve the relatively small horizontal scale mountain waves that typically form over the narrow Antarctic Peninsula. For example, Noel and Pitts (2012) used a resolution of 20 km to resolve these waves, while studies such as Orr et al. (2015) suggested that a much higher resolution of $\sim$4 km was required. Perhaps the resolution used in the refined grid is justified as suitable for longer climatological runs (lower computational cost), which requires a compromise. For example, line #60 claims that these waves will be 'directly simulated', but this doesn't necessarily mean that they are realistically represented. These sort of details would be helpful.

Noel, V. and Pitts, M.: Gravity wave events from mesoscale simulations, compared to polar stratospheric clouds observed from spaceborne lidar over the Antarctic Peninsula, J. Geophys. Res.,117, D11207, doi:10.1029/2011JD017318, 2012.

+ Section 2: This is a very lengthy and detailed description of the ICON-ART model, and especially its PSC scheme. Its not clear to me why such a detailed description is required (in the main text), as this has surely been explained elsewhere. To a non-chemist such a long section is difficult to follow. If such a detailed description is required, then explain in a sentence or so why. If such detail is not required, then please remove much of it. I would encourage that much of it is deleted or moved to the Appendix, as it made getting to the results difficult and frustrating as this had to be read firstly. I also don't see any point in all the equations in the Appendix being included, so please delete.

Additionally, the description of the PSC scheme needs to discuss how it deals with the mountain wave temperature perturbations. For example, if the 'warm phase' of the mountain wave temperatures results in temperatures being higher than the PSC for-

mation threshold temperatures, then what happens? Do the PSCs evaporate instantaneously? Also, please describe whether the PSC scheme is able to advect PSCs downstream of the orography, as seen in observations (Eckermann et al., 2009).

+ Figure 5: Please include results from the global ICON-ART version in Figure 5 so that we can clearly see the benefit of moving from a resolution of 160 km to 40 km. The resulting plot would be very strong and highly novel, clearly demonstrating the direct benefits of resolving mountain waves in terms of PSCs for the first time (rather than indirectly, such as how often PSC formation temperature thresholds are exceeded, such as in Orr et al. 2020). Results for the global model should also be added to Figs 6 and 7.

Minor comments

+ Line #26. Please quantify the horizontal scale of mountain waves. Also, a high model resolution is required to resolve the actual wave dynamics/evolution, and not just the orography. For example, it is thought that as many as 8 grid points is required to adequately resolve a gravity wave.

+ Line #28: You have mentioned the resolution of global models, but not the resolution for mesoscale models. Please add this.

+ Line #30: I don't follow your statement that 'A method to bridge this gap for interactive calculations is missing so far', as in this paragraph you have stated that solutions such as parameterizing the effects of orographic GWs exist. Please revise.

+ Line #36: Some mention of the AIRS GW climatology (Hoffmann et al. 2016) would also be appropriate, as well as the various orographic hotspots that this reveals in both the southern and northern hemispheres.

+ Lines #52-52: The statement that Alexander et al. (2011) concluded that about 30% of all southern hemispheric PCSs can be related to mountain waves is incorrect. Alexander et al. (2011) includes the caveat that this number is only for the latitudinal range 60-70S. A better study to cite is probably Alexander et al (2013), which states that ‘For all types of PSC, 5% in the whole Antarctic and 12% in the whole Arctic are attributed to OGW forcing’. Please revise the manuscript to reflect this, and also check that the numbers quoted from the other studies are correct and consistent with Alexander et al. (2013).

Alexander, S. P., Klekociuk, A. R., McDonald, A. J., and Pitts, M. C. (2013), Quantifying the role of orographic gravity waves on polar stratospheric cloud occurrence in the Antarctic and the Arctic, J. Geophys. Res. Atmos., 118, 11,493– 11,507, doi:10.1002/2013JD020122.

+ Lines #77-78. Please revise the sentence ‘. . . used which is similar to other studies (e.g., Stone et al., 2019; Zambri et al., 2019; Nakajima et al.,2020) and can be found in Appendix A.’ as its not clear. Also please elaborate why these equations need to be listed in the Appendix, as its not clear. If they are not necessary then please delete them to improve the flow/readability of the paper.

+ Line #87: I have made this point already, but you can’t simply make statements such as ‘simulate e.g. mountain waves’ without justifying this, such as explaining the scale of the waves and the grid scales used by the model. You need to do this.

+ Equations (1) and (2): Please check that the temperature T is defined before it is first used. (It is defined in line #119, after these equations.)

+ Line #175: Out of the blue we are informed that ‘In order to compare the results of the PSC scheme in ICON-ART with satellite measurements and to investigate the impact of the nesting technique on mountain-wave induced PSCs, ...’. As explained above, please make this clearer much earlier on. This would make the paper much clearer.

+ Line #179: Some basic information needs to be included such as to the synoptic conditions that result in the formation of the orographic wave. For example, presumably this is due to an easterly wind over the Antarctic Peninsula? Please include this sort of

information. Also, please put the period (July) examined into the wider context of the austral winter / PSC season.

+ Line #185: Has EMAC been defined?

+ Lines #191-194: These two sentences are unclear. Please revise.

+ Lines #204-209: This paragraph could be better written.

+ Table 2: The caption just seems like a repeat of the text in the section. Please consider revising this.

+ Line #207: Here you mention that a grid spacing of 40 km will be used for the nested simulation, but there is still no justification as to why this resolution was chosen and why it is thought to be appropriate. See other comments above.

+ Line #228: Please revise the sentence: ‘. . . on the one hand and exclude tropospheric clouds on the other hand’.

+ Lines #252-255: This is the sort of information that needs to have been included much earlier, say at the end of the Introduction. So that you are clearly explaining early on to the reader the scope of the paper.

+ Line #259: I think that the results are much more nuanced and subtle than simply saying ‘can be directly simulated with the resolution of 40 km’. Especially, because you are not showing any evidence at this point that the 40 km simulation of the wave is realistic.

+ Lines #262-264: Please revise this text. As already mentioned above, there is more to accurately representing a mountain wave than just resolving the steepness/height of the orography. This is a rather naive understanding of the problem.

+ Figure 4: The 80 km results don’t seem to be mentioned.

+ Line #265: Its not clear what you are referring to by ‘Therefore, the flow over the mountain range is under represented in the model'. The global model with a much smoother/lower orography might actually have enhanced flow over the Peninsula, although this is not a given as the model has a sub-grid scale orographic drag scheme to represent unresolved drag. Please revise.

+Lines #265-266: This is not clear. Please revise.

+ Line #275: As I have tried to emphasize, I think that you need to make these sort of statements more comparative, ie compared to the 160 km model, the 40 km model represents a well defined mountain wave with an amplitude of 10 K – this wave is entirely absent in the 160 km model. So you are trying to justify that you have produced a step change improvement in the representation of mountain waves in the model by going from 160 -> 40 km.

+ Line #280: Spelling. Fourth.

+ Line #314: Not clear what 'datasets' you are referring to here. Please clarify in the text.

+ Line #317: Its not at all clear what 'the results can be found in Fig. 5' are referring to. Maybe you are referring to the model v CALIOP PSC volume concentration, but this was introduced many paragraphs previously.

+ Figure 5: I'm not convinced that a statistical analysis is possible for such a short period considered. Please justify that this in the text, or amend the language so that you say that you are simply making a comparison for the period examined. For example, what is the frequency of CALIOP measurements etc.

+ Figure 5: Its not clear what the temperature values are on the figure. The ICON-ART panel has two different sets of values, which are unclear. The CALIOP panel has different values, which range from 23K to 349K (I think this is wrongly labelled). This is confusing, and makes it unclear how to compare the model results v CALIOP. Consequently, I found it difficult to follow the explanation of the Figure 5 in lines #324-

330.

+ Lines #324-330: You haven't referred to where the improvement resulting from resolving mountain waves would be expected in terms of PSCs. Presumably, you would expect more of a benefit for ice PSCs due to their lower temperature formation threshold compared to NAT and STS PSCs, ie they require the additionally cooling from GWs to exceed this temperature. Please clarify this when referring to Figure 5.

+ Lines #367-369: How important are these missing fine scale features? What are the implications for the simulated GW temperature perturbations? Does this suggest that ideally a higher resolution is required? Can you connect these deficiencies to the results in Figure 5, 6 and 7?

+ Figure 11: The 80 km results are included but not discussed. How do these compare with the 40 km results?

+ Figures 13: It's a little confusing jumping from the Peninsula region to a global domain. Perhaps this needs a separate sub-section or added to the section describing Figure 14.

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