Atmos. Chem. Phys. Discuss., 14, C9568–C9574, 2014
www.atmos-chem-phys-discuss.net/14/C9568/2014/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Inclusion of mountain wave-induced cooling for the formation of PSCs over the Antarctic Peninsula in a chemistry–climate model" *by* A. Orr et al.

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Received and published: 24 November 2014

General comments

1. *Changes in PSC area in the model are discussed throughout the manuscript in a quantitative manner. However, the decision to include only the cooling phase of the clouds renders these numbers a bit dubious. While there is clear justification for including only cooling effects in the chemistry calculation, this is not at all clear for the PSC area calculation where exposure to both the cooling and warming phases is likely to be important. This is explained in context in the comments*

below. The suggestion is to discuss these numbers more qualitatively due to the uncertainty associated with the assumptions in the method.

In the results sections of the manuscript PSC surface area density is only shown in Figure 11, which is solely discussed in the final paragraph of section 5 in broad terms such as ***'increases of more than 50% over the northern tip of the Antarctic Peninsula, and at least 30% over the Bellingshausen Sea'.*** These findings are reiterated in the abstract and discussed in the Summary and discussion section. We argue that a limited quantitative discussion of the numbers is required (as we have done) in order to detail the result that the PSC scheme is surprisingly sensitive to inclusion of the mountain wave-induced cooling phase. Indeed, the sentence in the abstract (***'A subsequent sensitivity study demonstrated that regional PSCs increased by up to 50% during July over the Antarctic Peninsula following the inclusion of the local mountain wave-induced cooling phase.'***) leaves the reader in no doubt that the results were obtained by including only the cooling phase.

The reviewer is correct that we had not included the caveat that neglect of the warming phase, while done for justified reasons, possibly overestimates the impacts of the mountain wave parameterisation and its influence on PSC surface area density. To remedy this, the revised manuscript now includes the following changes. Firstly, the methodology (section 2.4) now clearly justifies the decision to include only the cooling phase by stating: ***'... the equilibrium PSC scheme provides a realistic representation of the existence of PSC particles when air temperatures drop below the PSC temperature formation threshold (e.g. Feng et al., 2011). However, the scheme does not represent a slow decline of PSC existence when temperatures rise abruptly above the temperature threshold. Instead PSCs cease to exist instantaneously in the scheme. For this reason only the cooling phase of the parameterised temperature fluctuations are coupled to the PSC scheme as the net impact***

on additional PSC formation will be more realistic. Consequently the warm phase is neglected and the net effect on PSC existence might be slightly overestimated.' Secondly, the summary and discussion (section 6) states: ***'Our decision to include only the cooling phase implies that this may lead to an overestimate of the impacts of the scheme, and that the diagnosed increase in PSC surface area density should perhaps be considered as an upper bound. Note that consideration of the (neglected) warm phase in the equilibrium PSC scheme would reduce the PSC surface area density change modelled towards the large scale solution obtained in the control integration. By contrast, in a microphysical scheme in which PSC particles are advected around, the particles could briefly exist in air which is above the threshold temperature during the wave-induced warming phase before temperature will fall once again to below the threshold, maintaining PSCs. We simulate this effect by using the cooling phase only.'*** Please see section 6 of the revised manuscript for the full changes made to the summary and discussion section to explain and justify this important point.

2. *"Upstream influence" on PSCs is not mentioned until the summary section of the paper, yet it is apparent in Figure 11 that was presented much earlier. This upstream influence is never adequately explained in the paper. Both Figures 6 and 11 should be plotted without smoothing to make the coarse resolution of the result more clear. This coarse resolution may also influence any discussion of upstream influence, so this should be clarified prior to publication. Suggestions to alleviate this concern appear in context in the section below.*

   Firstly, this result is discussed when Figure 11 was introduced. For example, the paragraph in section 5 discussing Figure 11 states that these results are: ***'not unexpected ... what is diagnosed in Fig. 11 ... is the difference between two climate equilibrium states ... (which is) locally strongly influenced by the additional parameterisation (adding localised cooling and thus producing***

   more PSCs), but in regions away from the direct impact the response can be determined by feedback mechanisms.' Nevertheless, we have strengthened mention of the upstream influence of the scheme when Figure 11 is discussed by the inclusion in the revised manuscript of the sentence: ***'What is of note here is that PSC differences are occurring both upstream and downstream of the Antarctic Peninsula, i.e. removed from the actual region where the parameterisation acts directly.'***

   Secondly, figures 6 and 11 are unsmoothed.

   Thirdly, the summary and discussion section clearly states that understanding/clarification of the "upstream influence" on PSCs ***'will be the subject of a future study'***, as it is beyond the scope of this manuscript.

Detailed Comments:

1. *Page 18292, (Also mentioned on 18288) line 6: "only mountain wave cooling being used" and line 28-29: "The effect of the parameterisation on PSCs is investigated by evaluating the 30 year average difference in PSC surface area density between the perturbation and control". Since parcels would alternately feel both the cooling and heating phases of the orographic wave, this approach needs better justification before showing PSC surface area density. The use for evaluating frequency of occurrence of cold T is fine, but here you look at PSC area. The time a parcel experiences the cooling phase will be half of the wave intrinsic (Lagrangian) period. This will be much shorter than the Eulerian period for stationary mountain waves. PSC particles may not grow very large in this time and might sublimate again in the neglected wave warm phase. Hence the method may grossly overestimate PSC area.*

   Yes, we agree with the reviewer and have amended our revised manuscript accordingly. Please see reply to General Comment 1 (above).

2. *18278, 18-21: "increased stratospheric cooling was passed to the PSC scheme of the chemistry–climate model, and caused a 30–50 % increase in PSC surface area density over the Antarctic Peninsula compared to a 30 year control simulation". Including only cooling makes these PSC area statistics a bit dubious. Suggest deleting these specific numbers from the abstract, and instead discussing this more qualitatively. The assumptions do not warrant such a quantitative statement.*

   With due respect to the reviewer, we prefer to keep such numbers in the revised manuscript. Please see reply to General Comment 1 (above). However, we reiterate that the numbers are already given broadly (e.g. ***a 30-50% increase***) and always given with the caveat that they are the result of ***'increased stratospheric cooling'*** only.

3. *18290: Fig 6b would illustrate the point better if plotted without smoothing of the field. This would more clearly show that this is a coarse resolution result. The smoothed field implies the result applies to higher resolution.*

   Fig. 6b was not smoothed. This is the actual coarse model result.

4. *18293, 20 and Figure 11: Comment on the mechanism for upstream influence of mountain waves on the PSC area seen in Figure 11 is needed here. How that would come about is certainly not intuitively clear. Is this a model grid/coarse resolution effect? As also suggested for Figure 6, plotting the true coarse resolution result here without smoothing would be clearer and more representative, for example, it may help to understand the upstream influence on PSC area.*

   Please see reply to General Comment 2 (above).

5. *18294, 6-8: "This in turn is also a good rational for using the negative temperature anomalies only for the call to the chemistry scheme." Yes, but you further examine geographic distributions of PSC surface area density (e.g. Next page line 9), a*

quantity that would be greatly affected by processing of air parcels through both cold and warm phases.

   This line has been removed from the revised manuscript.

6. *18294, 9-11: "So for a grid-box averaged PSC coverage only additional incidents below the threshold temperature increase the coverage" Something is wrong with this sentence structure, and the meaning is completely cloudy as a result.*

   This line has been removed from the revised manuscript.

7. *18294, 11-12: "Certainly for such an assumption to be true the sub-grid wave train should be slowly evolving horizontally compared to the model time step" This statement mixes space and time scales in a nonsensical way. A method to connect the wave scale to the model time step is with the use of the Lagrangian wave frequency (intrinsic frequency). This can be computed easily from your wave-resolving model simulation for the purpose of this discussion.*

   This line has been removed from the revised manuscript.

8. *18294, 29: "due to it representing" "It" here is ambiguous due to a run-on sentence. Suggest splitting this sentence in two.*

   Yes, we agree with the reviewer and have split this sentence in two.

9. *18295, 23: "allow" Do you mean "permits"? The sentence is rather long, but the subject of this verb is apparently "state", so use "permits" or "allows", or rewrite the sentence for clarity.*

   Yes, we have followed the reviewers suggestion and changed 'allow' to 'allows'.

10. *18295, 20-21: "The simulation of PSC differences both upstream and downstream of the Antarctic Peninsula" This is the first mention of the upstream influence of the waves on PSCs. It should have been discussed when Figure 11*

was introduced. How much of this upstream influence is simply due to the coarse resolution of topography? Perhaps all?

Please see reply to General Comment 2 (above).

11. *18296, 11-15: "However, future work will investigate replacing the quasi-equilibrium PSC scheme with the full microphysical scheme DLAPSE (Denitrification by Lagrangian Particle Sedimentation), which uses a Lagrangian trajectory scheme and as such is able to transport PSC particles away from the region of formation" If this is done, representing both warm and cold phases of the gravity waves would become essential.*

    Yes. To clearly make this point in the revised manuscript we now (in the summary and discussion section) state: ***'in a microphysical scheme in which PSC particles are advected around, the particles could briefly exist in air which is above the threshold temperature during the wave-induced warming phase before temperature will fall once again to below the threshold, maintaining PSCs. We simulate this effect by using the cooling phase only. In future work we plan is to insert the microphysical scheme DLAPSE (Denitrification by Lagrangian Particle Sedimentation) (Feng et al., 2011) into the UKCA module, and couple it to both the cooling and warming phases of the parameterised temperature fluctuations.'***

12. *Figure panels are too small in Fig. 2 and 3, but perhaps this will be remedied when the publication appears in full-page format?*

    We will ensure that the typeset manuscript is checked for this, and that all panels are readable in the eventual published manuscript.
