Reply to Referee 1

We thank Referee 1 for the constructive, helpful criticism and the suggestion for revision. We have thoroughly revised the manuscript based on the comments given by the referees. A detailed point-by-point response to the comments by Referee 1 is given below.

In this article, the authors propose a way to evaluate the fraction of ice Polar Stratospheric Clouds (PSC) affected by short-lived small-scale temperature fluctuations generated by gravity waves, using a combination of MIPAS PSC detections and reanalyses temperatures. They support their analysis by the use of backward trajectories to document the temperature history of air masses. Their results bring new quantified information on the relationship between ice PSC and gravity wave activity over both poles.

The article describes the methodology and results in a very clear way and is extremely readable. The proposed methodology is sound, the results are consistent with what is known about PSCs and gravity waves, while bringing new information that completes what we know in a useful manner. The authors clearly have a good handle on the data from MIPAS and reanalyses, which are very well described. References abound, the bibliography is complete and well selected. The figures are well designed and convey their message with clarity. I support the publication of this article in ACP, once the few remarks I have below have been addressed.

We would like to thank the referee for the encouraging statement.

Major comment
My main comment is a suggestion to the authors to clarify their intent, make some of their assumptions explicit, and better relate the approach they've devised with the scientific questions that are raised. As currently written, it is a bit hard to understand why the authors are doing what they are doing.

We agree with the referee that the aspects mentioned here and the major objectives of the study need to be introduced more clearly. Following the comments and suggestions, we carefully revised and extended the abstract and the introduction to clarify.

For instance, the abstract begins with "Small-scale temperature fluctuations can play a crucial role in the occurrence of ice clouds". This may be true, but rather vague. What small-scale temperature fluctuations are we talking about? Are we talking about temperature fluctuations created by mountain/orographic waves? Are "ice clouds" meant to be ice PSC? This first sentence is the only one that suggests what questions the authors are interested in. The rest of the abstract describes the methodology and results, but fails in my opinion to relate them to the scientific questions raised by the first sentence. After the first sentence, the abstract goes on explaining that the study is analyzing PSC occurrences from MIPAS, using the smallest difference temperature etc. How are all these steps related to better understanding the crucial role the small-scale temperature fluctuations play on the occurrence of ice clouds? Please fill in the gaps.

We revised the first sentence of the abstract to specify that we are referring to temperature fluctuations caused by gravity waves, including those generated by mountain/orographic waves. We also clarified that the "ice clouds" referred to are indeed ice polar stratospheric clouds (PSCs) and added some sentences to relate our study to the scientific question.

I was hoping the introduction would clarify these points, by describing how the methodological approach chosen by the authors is appropriate to answer the questions raised by that first sentence, but in my opinion it suffers from the same problem as the abstract. The first 3 paragraphs do a good job explaining why temperature perturbations generated by gravity waves affect the formation of PSC. The last paragraph dives straight in a summary of the selected methodology, but does not relate this methodology to the questions raised in the previous paragraphs, or explain what insights it will bring. I feel there is a missing paragraph before this one that should explain why MIPAS detecting a PSC, when ERA5 says the coldest synoptic temperatures are too warm to support PSC formation, is a sign of gravity wave activity. I have felt this as an implicit assumption throughout the whole text – please make it explicit. I understand that the authors perhaps want to be careful and avoid stating it for some reason, but not making it explicit makes it hard to appreciate what the authors' work brings to the subject under study. In addition to the abstract and the introduction, the link between the authors' approach and gravity wave activity needs to be made clearer in section 4 and 5 also.

We revised the introduction to clearly state that a major objective of the study is to assess the effects of unresolved or only poorly-resolved temperature fluctuations due to gravity waves on PSC formation. Please also see our replies to the specific comments below.

Minor comments

- *Title: the current title is very generic. Please at least refer to gravity waves in it.*

  Done. We have changed the title to "Impact of mountain-wave-induced temperature fluctuations on the occurrence of polar stratospheric ice clouds: A statistical analysis based on MIPAS observations and ERA5 data"

- *L. 1: "a decade" please state which years we are talking about here (2002-2012 ?)*

  Yes, the time period considered is 2002-2012. We have added this.

- *L. 18: "The surfaces of PSCs" I don't think we can say that clouds have surfaces. Maybe "the surface of PSC particles"*

  Fixed

- *L. 46-47: This paragraph begins by explaining the primary focus of the study is to investigate the occurrence of ice PSC observed by MIPAS and characterized by temperatures above the ice existence threshold. Please explain why, when interested in the relationship between ice PSC and gravity waves (as the beginning of the section suggests), it would be a good idea to do that (see major comment).*

  Following the major comment and comments of other referees, we added a new paragraph to the introduction that explains the physical processes of how gravity wave activity leads to PSC formation in more detail: "Atmospheric gravity waves are oscillations in the Earth's atmosphere caused by buoyancy and gravity acting on air parcels displaced from their equilibrium positions. They can significantly affect local pressure, temperature, winds, and other meteorological variables. Gravity waves play a critical role in the transfer of energy and momentum between different layers of the atmosphere, i. e., they can significantly affect the temperature structure and general circulation of the atmosphere (Fritts and Alexander, 2003;

Alexander et al., 2010). Gravity waves typically originate from sources such as mountain ranges, where air flow is disturbed and lifted; thunderstorms, where convective activity is generated; and frontal systems, where air masses are disturbed from geostrophic equilibrium. Understanding gravity waves is essential for accurate weather forecasting and broader atmospheric dynamics. Properly representing the effects of gravity waves in general circulation and chemical transport models is challenging because coarse-grid global models typically lack the spatial resolution needed to resolve the small-scale perturbations of gravity waves. In this study, we are particularly interested in atmospheric gravity waves in the polar winter lower stratosphere, because temperature perturbations due to gravity waves can trigger the development of PSCs in the cold phase of these waves, even in cases where background or synoptic-scale temperatures are higher than their formation thresholds (Carslaw et al., 1998; Rivière et al., 2000; Dörnbrack et al., 2020; Orr et al., 2020). Missing these waves in coarse-resolution global chemistry-climate simulations or reanalysis-driven chemistry-transport simulations will affect the PSC representation."

- *L. 76-85: Here you explain your alternative to using the tangent point for identifying the location of a PSC that MIPAS has detected along its line of sight. Have you tried to investigate the spatial distribution of temperatures along the line of sight? Is it frequent for cold temperatures to be spatially spread along the line of sight far from the point identified as the coldest? Or can you have two locations along the line of sight with similar coldest temperatures? This would mean the PSC location is affected by strong uncertainties. In other words, how well defined spatially is the ∆Tice_min ?*

  Since the real location of a PSC along the MIPAS line of sight remains unknown as ground truth, it is difficult to quantify the uncertainty and improvements in locating the PSC detections with different methods. Assigning the detection to the tangent point, as in previous studies, is a heuristic choice. Similarly, assigning the detection to the location of the minimum of the temperature difference between Tice and T along the line of sight (∆Tice_min) is also a heuristic but likely more plausible choice.

  In Sect. 4.2 of the paper, we discuss the sampling uncertainty of MIPAS for the ice PSC detections. In particular, we evaluated how the choice of different methods of locating the PSCs (tangent point versus ∆Tice_min) affects the spatial and temporal distributions of the observed occurrence frequencies (compare Figs. 1 and 9, respectively). We found a larger sensitivity to the height difference than to the horizontal distance of the tangent point and the location of the ∆Tice_min minimum, which we attribute to the fact that temperature changes significantly with height but not so much horizontally. In the statistical comparison with $3^\circ \times 5^\circ$ horizontal grid boxes conducted here, the horizontal distribution of ice PSCs is found to be essentially identical for both methods.

  Although we cannot quantify the uncertainty of the individual locations of the MIPAS PSC detections due to the lack of ground truth information, we performed an additional statistical analysis to determine the horizontal and vertical distances between the tangent point and the temperature difference minimum along the line of sight (see Fig. 1 in this reply). This analysis shows that half of the time the horizontal distances are below ± 200 km and the vertical distances are below 4 km.

This is consistent with the discussion in Sect. 4.2 of the paper

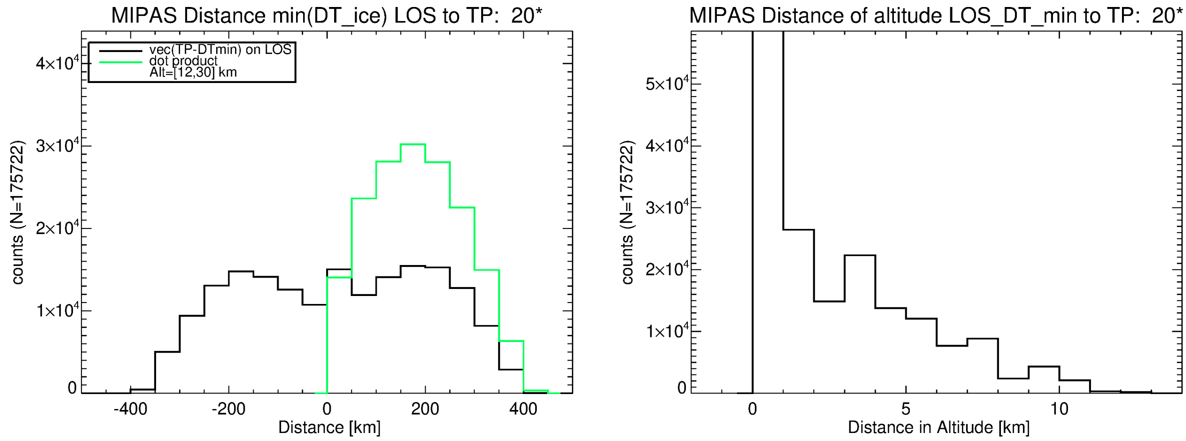

Figure 1: Histograms of the horizontal (left) and vertical (right) distances between the tangent point and the minimum of the temperature difference between T_{ice} and T along the line of sight. The statistics cover MIPAS observations during Southern Hemisphere polar winter in the years 2003 to 2011.

- *L. 101, 103: References to Hoffmann et al. 2017b and a are missing parentheses. Also please fix the citations (a should be cited before b).*

  Thanks for pointing this out. The missing parentheses have been added. We agree that a should cited before b. However, this order is done by bibtex automatically (based on the names of the co-authors) and we have not found a solution on how to change this. We hope that the Copernicus Editorial Support Team can fix this during the typesetting/copyediting.

- *Section 2.2: Please explain why you use both ERA-Interim *and* ERA5. Spatial/temporal resolutions appear similar, so why isn't it possible to pick just one reanalysis dataset? If using two datasets is mandatory, could you talk about how potential disagreements in polar stratospheric temperatures from both datasets would affect your results?*

  Thank you for pointing this out. After rechecking the data, we can confirm that only ERA5 was used in our study. The PSC dataset we use here is the one described in Spang et al. (2018). In the original dataset provided with the paper, temperature information e.g. T_{ice} is based on ERA-interim temperatures. However, updates of this MIPAS PSC dataset including the additional information of temperatures along the line of sight, were provided with ERA5 temperatures. Our original manuscript was based on one of the older versions, and when we updated the MIPAS PSC dataset, we accidentally missed updating the text in the manuscript. We apologize for this oversight and appreciate that this mistake was discovered through the referees' questions. The manuscript has now been revised accordingly. We added the specific version of the MIPAS PSC data product used in this study in the code and data availability section of the manuscript.

- *Section 2.2: I was under the impression that Hoffmann et al 2017 (the one about Concordiasi) suggested that ERA-Interim suffers from a zonally increasing warm bias in the south pole. Is that affecting your results in any way? Is this bias corrected in ERA5?*

  We have updated the manuscript that only ERA5 was used in our study. The study of Hoffmann et al. (2017) was focusing on comparisons of polar stratospheric temperatures with Concordiasi super-pressure balloon observations. Most of the balloon measurements were conducted at specific altitudes of 17–18.5 km and latitudes of 60–85°S. Biases between reanalyses and observations generally differ with latitude, height and over the season.

- *L. 116: what does it mean for the Lagrangian transport to be significantly impacted? Is it better, worse, something else? Does this suggest that using ERA5 (instead of ERA-Interim) for locating and quantifying the $\Delta Tice\_min$ would lead to different results?*

  To clarify, we rephrased: "The evaluation of MPTRAC trajectory calculations was assessed by using different meteorological reanalyses (Hoffmann et al., 2017; Rößler et al., 2018; Hoffmann et al., 2019). It was found that Lagrangian transport simulations are significantly improved by the ERA5 data compared to ERA-Interim data (Hoffmann et al., 2019). For instance, it was found that there is better conservation of potential temperature along the ERA5 trajectories than the ERA-Interim trajectories in the lower stratosphere."

- *L. 119: Here you state that your aim is to "conduct a statistical analysis of ice PSCs where the temperature at the MIPAS observation is above the frost point temperature". Again,* *in my opinion you have not made clear enough why you might want to do that (see main comment). Please make explicit the link with gravity wave activity.*

To make a clearer link to the main objectives of the study, we rephrased this sentence to: "In order to assess how temperature fluctuations due to gravity waves affect PSC formation using the MIPAS measurements and ERA5 data, we first need to identify ice PSC observations where the temperature at the location of the MIPAS observation is above the frost point temperature (Tice)."

- *L. 135: After lines 76-85, this is the second time you explain your alternative to the tangent point for PSC detection. You explain it again on lines 240-242. Please try to limit these explanations.*

  We removed duplicates on lines 135 and 240-242.

- *L. 139: "highest occurrence frequency": over which time scales? 2% is not a lot.*

  The time scale has been added. Yes, 2% means the ice PSCs are not so often detected in the Arctic. The sentence has been rephrased as follows: "The spatial and vertical distribution of the averaged occurrence frequency of ice PSCs at ΔTice_min from MIPAS measurements over the time period 2002 − 2012 is presented in Fig. 1."

- *L. 146: Here you describe that most ΔTice_min are found below the frost point. Reading this confused me at first, since on lines 119 you explain that your focus is on points that are above the frost point. I think you could limit reader confusion that making it clearer from the start that you expect ΔTice_min above the frost point to be rare, and that you take them as the sign of small-scale temperature perturbations from gravity waves, i.e. by clarifying why you are doing what you are doing (see main comment).*

  Thanks for the comment. We improved the text by better explaining what we have done and removed sentences that could be misleading.

- *L. 148-149: The discussion about ice PSC particles nucleating at temperatures colder than Tice mirrors the one found on lines 123-125, but with fewer details. Please find a way to combine both discussions. I think the discussion should happen in Section 3.1, but with the details found in lines 123-125.*

  Thanks, we have consolidated lines 123-125 and 148-149 in Section 3.1.

- *L. 151: "T-Tice-3K" I'm guessing here you mean Tice-3K*

  Yes, indeed. This has been corrected.

- *L. 175: "Fig. 5 displays the fraction of ice PSCs above Tice". I did not understand this part. As I understand, MPTRAC calculates backward trajectories starting from the point where a PSC was located according to MIPAS observations + ΔTice_min from ERA5. MPTRAC provides the evolution of temperature and other parameters along the backtrajectory, but has no way to know when a PSC was present in the air mass that it is tracking, and does not provide that information. Are you assuming that the air mass where a PSC was detected at t=0 contains a PSC over the previous 24 h through the entire backtrajectory? Please clarify my misunderstanding. (also please fix related wording line 309)*

  You are correct, we don't know when a PSC was present. We used only the evolution of temperature along the back trajectory from the MPTRAC model. We have

corrected the misleading wording. "24-hour backward trajectories are calculated by using the MPTRAC model from the point of ice PSCs observation at ΔTice_min."

- *L. 176: "(t) t=-24 (h)" – do you mean t=-24h ?*

  Yes. This has been corrected.

- *L. 177-178: It appears strange to me to say the fraction decreases by going from t=0 to t=-6h, ie going backwards in time. It would appear more natural to say the fraction increases from t=-6h to t=0.*

  We have revised these lines."The fractions of ice PSCs above Tice increase by about 17 percentage points (pp) and 10 pp in the Arctic and Antarctic, respectively, from $t = -6$ h to the observation point at ΔTice_min ."

- *L. 180: "6h before the observation, temperatures of most ice PSC... are below Tice". If I read Fig. 5 correctly, it looks to me like temperatures of most ice PSC are below Tice at all times.*

  Yes, you are correct. We have revised this part. The text reads now: "Over 24-hour backward trajectories, temperatures of most ice PSCs are below Tice. Nevertheless, there is a significant increase of the fraction that occurs within the 6 hours preceding the observations."

- *Section 3.4: If I understand correctly, your main hypothesis is that the influence of gravity waves on PSC can be inferred by ΔTice_min being positive, as it means that the reanalysis temperatures are failing to capture small-scale temperature variations due to GW. In this section, however, you look for short-scale temperature variations within the reanalysis as indicators for the influence of gravity waves. There seems to be a contradiction – either GW influence is captured in reanalyses, or it isn't. Please clarify my misunderstanding. Are you expecting ERA5 (used by backtrajectories) to better capture the gravity wave influence on temperatures than ERA-Interim (used for ΔTice_min)? Why?*

  We added the following paragraph to clarify: "Here, we computed the variance of the cooling rates along the backward trajectories over a 6 h running window and applied a variance threshold of 0.9 $K^2\,h^{-2}$ to detect the presence of significant temperature fluctuations associated with a gravity wave event. Note that while meteorological reanalyses are often capable of reproducing gravity wave events at the right time and place, especially for mountain waves, the wave amplitudes in the reanalyses are typically damped compared to the real atmospheric conditions (Schroeder et al., 2009; Jewtoukoff et al., 2015; Hoffmann et al., 2017). The degree of underestimation depends on the resolution and numerical filters of the forecast model used to generate the reanalysis and on the spectral characteristics of the gravity waves. Therefore, the simple variance filter method applied here is generally suitable to detect the occurrence of wave events in the atmosphere. However, the detection sensitivity depends critically on the variance threshold. A sensitivity test regarding the choice of the variance threshold is discussed in Sect. 4.4."

- *L. 209-210: "This observation strongly suggests a correlation with orographic waves with ice PSCs above Tice". This reads strange to me. We *know* that orographic waves trigger the formation of ice PSCs. Unless I'm mistaken, this is actually the (unstated)* *reason why you look for positive $\Delta Tice\_min$ – because they are the sign of short-scaled temperature perturbations, that are not well captured by ERA-Interim, and are generated by gravity waves. It looks like you are trying here to avoid stating that we already know that orographic waves trigger PSC formation. Please be explicit about your assumptions.*

We would like to avoid giving any wrong impression that this would be a new finding of this study. We therefore deleted this sentence. Similarly, at lines 317 to 320, we rephrased: "The ice PSCs above Tice with temperature fluctuations along the backward trajectories are primarily concentrated over the Antarctic Peninsula and the Weddell Sea in the Antarctic and encompass the east coast of Greenland and Northern Scandinavia in the Arctic, which are known hotspots of mountain wave activity."

- *L. 218-223: I am very confused by this paragraph. I understand your results find that the fraction of $T >$ Tice related to mountain waves are higher in the Arctic than in the Antarctic (l. 220-223), no problem there. But, unless I'm mistaken, your main point is that the fraction of $T >$ Tice related to mountain waves decreases as it gets closer to the observation point. This is what you open your paragraph with, and the main result from Table 1. And yet you make no attempt to explain this decrease. Why is this fraction decreasing as we get closer to observation point? I personally find this result perplexing.*

  We have revised this paragraph. Table 1 shows the cumulative fraction of ice PSCs above Tice over certain regions, demonstrating that the different timescales of backward trajectories affect the results. As longer backward trajectories are included, more ice PSCs are found to be associated with mountain waves.

- *L. 230: double parentheses*

  The obsolete parentheses have been deleted.

- *L. 226-238: This paragraph is the closest you get to state your assumption that PSCs observed at $\Delta Tice\_min < 0$ are due to small-scale temperature fluctuations from gravity waves that brought temperatures below Tice in a way that is not captured by ERA-interim reanalyses. Still, you need to make this reasoning explicit. Also: if the main hypothesis is that ERA-Interim misses temperature fluctuations that generate PSCs, why should we trust the location of the $\Delta Tice\_min$ according to ERA-Interim? The PSC detected by MIPAS might be somewhere else along the line of sight, in a place more affected by gravity waves (which are not well captured by ERA-Interim). Please address this somehow.*

  Following several review comments, we have revised the abstract, motivation, and conclusions sections of the paper to clarify the main objective and purpose of this study. In this paragraph, our new results on MIPAS ice PSC detections related to temperature variations due to gravity waves are placed in the context of existing results and studies in the literature. We have slightly revised the paragraph to clarify its purpose.

- *L. 317: "with temperature fluctuations at the observation point" – as I understand it, the observation point is fixed in time and space. How can there be temperature fluctuations are the observation point then?*

  We rephrased this to clarify: "The ice PSCs above Tice with temperature fluctuations at the observation point are primarily concentrated in and around the Antarctic

Peninsula and the Weddell Sea in the Antarctic and encompass the east coast of Greenland and Northern Scandinavia in the Arctic."

- *L. 319: "... suggests a correlation with orographic waves with ice PSCs above Tice". That ice PSC are correlated with orographic waves is a given from the beginning – you even explain the mechanism in the introduction. You could say that your results are a strong confirmation of these correlations.*

  Based on the comments by one of the other referees we omit using the term "correlation" and rephrased the sentence as follows: "The ice PSCs above Tice with temperature fluctuations along the backward trajectories are primarily concentrated over the Antarctic Peninsula and the Weddell Sea in the Antarctic and encompass the east coast of Greenland and Northern Scandinavia in the Arctic, which are known hotspots of mountain wave activity."

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