Reply to Referee 2

We thank Referee 2 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 2 is given below.

Review of ‘A statistical analysis of the occurrence of polar stratospheric ice clouds based on MIPAS satellite observations and the ERA5 reanalysis’ by Ling Zou et al.

Summary:
The manuscript uses a decade of MIPAS satellite observations of PSCs (polar stratospheric clouds) to investigate the water ice PSC occurrence with respect to frost point temperature, determines their spatial location, and examines back trajectories in order to understand the temperature history of the air parcels in which PSCs are observed. The authors consider the role which small-scale temperature fluctuations (i.e. gravity waves) play in PSC formation and quantify the fraction of PSCs associated with orographically-forced gravity waves. Because of the limb-viewing geometry of the MIPAS satellite, there are uncertainties in the position of the clouds along the line of sight observation path. The authors propose using the point where temperatures are closest to the frost point as the location of the ice PSCs.

The manuscript is well written and provides plenty of evidence to support the idea that orographic gravity waves can play an important part in driving ice PSC formation. I think that the manuscript should become acceptable for publication once the authors consider implementing the below comments.

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

Major comment:

Overall comment: Most of the manuscript builds up to the point that orographic gravity waves can explain a percentage of the ice PSC observations (33% in the Arctic, 9% in the Antarctic, as noted in the abstract). Yet it seems to me that this is already a well-established result in the literature (much of which is cited within the text already) and so these parts of the manuscript are really just supporting that which is known already. I feel that the novel parts of the manuscript, i.e. the use of MPTRAC back trajectories together with the proposition to use air parcel history (in particular, minimizing the temperature difference to the frost point) as the location of the PSCs, should be brought forward as the central point of the manuscript, and thus emphasized much more clearly in the abstract and conclusions, and indeed through the whole text. Your title could also be reworded in this context.

Thank you for your comment. Based on your comment and the comments of the other two referees, we have revised and tried to improve the entire manuscript and hope that the novel parts of our study, including the method to identify the location of PSCs along the MIPAS line of sight and the use of Lagrangian backward trajectory analyses for MIPAS ice PSC detections, are now clearer. Please see the responses to specific comments below. In addition, 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".

Line 76 onwards: Discussion about method of identifying most likely ice PSC location as minimiz-
ing relative to the frost point temperature. Does this method reduce the hundreds of kilometers of horizontal uncertainty (which you noted on line 80)? By how much? I should like to see some attempt to quantify the reduction in error as I think that is central to your manuscript. For instance, with the method here do you observe any gravity-wave connected ice PSCs upwind of mountain ranges? This may help indicate a bound on your accuracy. You would likely have better ideas on quantifying this uncertainty.

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 $T_{\text{ice}}$ and $T$ along the line of sight ($\Delta T_{\text{ice min}}$) 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 $\Delta T_{\text{ice min}}$) 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 $\Delta T_{\text{ice min}}$ 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 $\pm$ 200 km and the vertical distances are below 4 km. This is consistent with the discussion in Sect. 4.2 of the paper.

![Histograms of horizontal and vertical distances](image)

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

Yes, we mean polar stratospheric ice clouds. We have rephrased the sentence.

Line 2: which decade?

The data considered is from 2002-2012. We have added the years in the sentence.

Line 13: ‘correlation’: have you performed a correlation analysis? I couldn’t find one. Correlation doesn’t imply causation anyway.

Yes, you are right. We have not performed a correlation analysis. Using this term here was a bit misleading. We have rephrased the sentence.

Line 24 (or somewhere close by): Given that you spend much of manuscript discussing ‘small-scale temperature fluctuations’, I think you need to introduce the concept of gravity wave generation by mountains, explaining that these can cause small horizontal scale temperature perturbations in the stratosphere which are large enough to enable some PSC formation in the cold phase of these waves, even when the background (synoptic) scale temperature is too warm for their formation. And thus missing these waves in model (climate) simulations will effect the model PSC representation. Plenty of references abound for this discussion, many of which you already cite.

Following also the comments of the other referees, we added a new paragraph to summarize the background and assumptions of this study more clearly: “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.”

Line 38 (or somewhere close by): You haven’t discussed the CALIOP wave-ice PSC class, as defined by Pitts et al. 2013 doi:10.5194/acp-13-2975-2013. This class addresses a similar problem
to what you are here, namely using satellites to attempt to quantify gravity wave influence on PSC formation, but in a different way to how you do it with MIPAS. Some mention of the CALIOP wave-ice would be beneficial in the introduction.

We agree and added the following sentence referring to the CALIPSO wave ice PSC class introduced by Pitts et al. (2013): “Due to the importance of wave generated ice PSCs, Pitts et al. (2013) introduced an additional class in their PSC characterisation scheme specifically for wave ice PSCs.”

Line 51: ‘we explore the temperature variations’: There seems a bit of a reluctance in the paper to use the phrase ‘gravity waves’ or ‘orographic gravity waves’ when this is what you are doing. Maybe because you are building up to attributing the ice PSCs to gravity wave activity. Consider rephrasing. See also Major Comment above.

We agree that the relation of PSC formation to gravity wave activity needs to be more clear and explicit throughout the paper. In addition to changes in other places, we rephrased: “...we explore the temperature variations along the backward trajectories driven by ERA5 data as an indicator of gravity wave activity to comprehend their impact on ice PSC occurrence.”

Line 85: You use ERA-Interim for this identification but use ERA5 elsewhere, so why use ERA-Interim anymore? There is no explanation given, so please provide a justification. But, line 116 says ERA5 is much better for Lagrangian transport, it is “significantly impacted” as you write. So I think that you should really use ERA5 for identifying the minimum difference to the frost point. It will capture the small-scale gravity waves more effectively than ERA-Interim, which is a major point of your study. Also given the finer resolution in ERA5, I would expect that it will improve your results and likely reduce the horizontal uncertainties. See also my Major Comment ‘Line 76 onwards’ above.

Thank you for pointing this out. After rechecking the data, we can confirm that only ERA5 was used in our study. The data set we use here is the one described in Spang et al. (2018). In the original dataset provided with the paper, the respective ERA-interim temperatures along the line of sight were included in addition to the MIPAS PSC data. However, in updates to this MIPAS PSC data, the data was provided with ERA5 temperatures. Our 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.

Line 88: ‘up to -6.1km below’ reads awkwardly. Reword and simplify.

We have revised to "at an altitude of 6.1 km below the cloud top”.

Line 123: Add year to Koop reference

The year has been added.

Line 129: ‘less than 10K above’ should be reworded for simplicity

We have changed our wording to "within 10 K above Tice"
Line 139: ‘frequency over 16%’ – but your scale is cut at 16% so you can’t comment on this!

We have changed our wording to “frequencies up to 16%”

Line 142: discussion of the Arctic vortex. I think you need some references and explanations about the off-pole position of the Arctic vortex here, noting the role of planetary waves in the NH in forming the vortex in this location.

We have added the following sentence including references: "Note that while the Antarctic polar vortex is centred over the pole, the Arctic polar vortex is displaced from the pole due to frequent disturbances from sudden stratospheric warmings (SSWs) caused by planetary wave activity (Waugh and Randel, 1999; Baldwin et al., 2021)."

Line 143: A sentence linking this climatology to previous PSC climatologies would be useful (e.g. see Tritscher et al. Rev Geophys 2021, their Fig 11 and Fig 21)

Directly comparing our Fig. 1 with Fig 11 and 21 from Tritscher et al. (2021) is not possible since we show in Fig 1 a multi-year (2002-2012) averaged occurrence frequency while Fig. 11 and Fig 21 from Tritscher et al. show the occurrence frequency for one specific year (2009) and separated by month for averaged over the time period 2006-2018, respectively. Nevertheless, since the general features are the same (spatial location of the PSCs and the descending height of the PSCs during the course of the winter), we changed the sentences describing Figure 1 as follows: "The spatial and vertical distribution of the multi-year averaged occurrence frequency of ice PSCs at \( \Delta T_{\text{ice}} \text{min} \) from MIPAS (2002-2012) is presented in Fig. 1. The results of the spatial and vertical distribution of ice PSCs derived here are in general agreement with the respective occurrence frequencies from MIPAS and CALIPSO shown in Tritscher et al. (2021)."

Line 148: sentence beginning ‘Since ice PSCs…’ this sentence on thresholds was discussed already. Consolidate or remove.

We have consolidated the duplicate parts here.

Line 150-151 : You should consider a cumulative frequency plot to illustrate the points you are making here (i.e. the 90% and 100% thresholds)

Thanks for the suggestion. The cumulative frequencies can be interpreted from the two vertical dashed lines at Tice-1.5 K and Tice-3 K in Fig. 2.

Line 151: ‘T-Tice-3K’ recheck this phrase

Checked and corrected.

Line 162: Worth stating clearly that these are low fractions (a few %). But, they are similar to CALIOP results (e.g. Alexander et al. 2013).

We have added one sentence. "Low fractions (about 1%) of ice PSCs are present in the Arctic which are similar to CALIOP results (Alexander et al., 2013)."

Line 167: Years 2005 and especially 2011 were major SSWs with large ozone losses (See Manney et al. Nature, 2011, doi:10.1038/nature10556). Think about the larger context, and suggest
You cite this Manney paper, or others which you may wish, to provide this larger overview of the interannual variability.

We agree. We cite now Manney et al. (2011) for the Arctic winter 2010/2011 and Feng et al. (2007) for the Arctic winter 2004/2005 and added the following sentence in the manuscript: “The Arctic winters of 2004/2005 and 2010/2011 were both distinguished by exceptionally low temperatures and substantial ozone depletion (e.g. Feng et al., 2007; Manney et al., 2011).”

Figure 4: You should add some uncertainty bars on these panels. You have 10 years of data. These can describe the interannual variability

The uncertainty bars have been added.

Figure 6: A really bad color choice of a red star! I suggest you use black

Thanks, we have updated the plot.

Section 3.5 title and wording ‘Correlation’. Are you actually performing a correlation analysis? Seems not. I think you need to remove this word ‘correlation’ throughout your text.

We do not perform correlation analyses. We have adjusted the section title and omitted using the term "correlation”.

Line 222: Is gravity wave activity really more frequent in the NH (mountainous regions) than in the SH? You need to provide a reference here! Or, is it that the Arctic is synoptically warmer and generally hovers just above Tice, so any orographic waves are more likely to increase ice PSC fraction when compared with the synoptically colder Antarctic. Explanation is needed, either way.

Orographic waves are not more frequent in the southern hemisphere, but these have a larger impact on PSC formation in the Arctic since temperatures are generally warmer there and do not reach by synoptical cooling alone so often temperatures that are sufficiently low for ice particle formation (e.g. Tritscher et al., 2021). We have rephrased the sentence as follows to make this more clear: “This difference can be attributed to the higher importance of gravity wave activity on ice PSC occurrence in the Northern Hemisphere.”

Line 237: Can you quantify, or at least state/cite the underestimation of temperature fluctuations?

We added: “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.”

Line 238: ‘The analysis applied in this study provides important information on the frequency and significance of these discrepancies’: I’m afraid that I can’t find the text which supports this statement. Please refer back to the figure / section where this is true, or remove/reword.
We have rephrased the sentence as follows: "The analysis applied in this study (see previous sections) provides important information on the frequency of these events and once again points out the significance of the discrepancies between observed ice PSCs and temperatures derived from meteorological reanalysis."

Line 262: Uncertainty of approximately 0.2K. Is this about the figure for the polar regions? How bad is a 0.2K uncertainty? Your bins in Figure 10 are much larger so I suspect this uncertainty is not a big deal. Comments required.

The 0.2 K is the global mean temperature uncertainty in the low and middle stratosphere according to Simmons et al. (2020). Unfortunately, the temperature uncertainty in the ERA5 data over the polar stratosphere is unknown.

Line 267: You write that you use water vapor data from ERA5. But you can obtain water vapor data from MLS satellite (e.g. see Tritscher et al. 2021, Rev. Geophys.). Have you considered doing so?

Yes, we have considered using water vapour from MLS, but decided against it. If we would use MLS observations together with MIPAS we would have to deal with the uncertainties of two satellite instruments and measurements made in two different orbits. Further, the MLS measurements start from 2004, thus two years later than MIPAS and thus do not cover the same time period. ERA5 reanalysis data on the other hand is globally available for the entire MIPAS measurement period. Additionally, we think that also for calculating $T_{ice}$ along the trajectories using ERA5 $H_2O$ is more appropriate despite the uncertainties.

Line 291: How about also considering the actual generation mechanism of the waves? Dornbrack et al. (2001, doi: 10.1029/2000JD900194) provide a set of criteria for mountain wave activity. Essentially, at many times, near-surface winds are not conducive for the formation of orographic gravity waves. They only happen when surface winds are of the correct angle against the mountain range, and the background wind allows their propagation into the stratosphere. So while it may be the ‘unresolved temperature fluctuations in ERA5’ which is the issue, or it could be something else.

Indeed, thanks for mentioning this. However, the generation mechanism of waves is beyond the scope of this study. We have added one sentence in the manuscript “Other reasons, such as the formation mechanism of gravity waves (Dornbrack et al., 2001), could also affect the results, but taking these into account is beyond the scope of this study.”

Line 295: ‘are proposed to’ – what about ‘are shown to’?

They have both proposed and shown to provide a better estimate. Thus, we changed the sentence as follows: "The points with the smallest temperature difference ($\Delta T_{ice,min}$) between the frost point temperature ($T_{ice}$) and the environmental temperature along the line of sight have been proposed and shown to provide a better estimate of the location of ice PSC observation from MIPAS."

Line 300: ‘which is about 2-4 km higher above the tangent point’. This phrase needs explaining
Revised as "which is about 2–4 km higher than that detected at the tangent point"

Line 316: where has the ‘significant correlation’ been demonstrated?

See our answer to your comment above. We have omitted using the word correlation.

Line 324: As suggested above, what about the point that the Arctic is synoptically warmer and thus orographic waves’ influence on PSC formation can have a larger fractional impact?

Agree, we rephrased this text part as follows: "This difference can be attributed to the larger role of gravity wave activity in ice PSC occurrence in the Northern Hemisphere. This is due to the generally warmer temperatures in this hemisphere, which often require mountain wave-induced temperature fluctuations to initiate ice PSC formation. However, the larger size of the selected region may also contribute to the hemispheric differences.”

References


