Reply to Referee 3

We thank Referee 3 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 3 is given below.

This manuscript looked at the relationship between temperatures and the formation of ice PSCs. Unfortunately, it was difficult to read, and also rather unconvincing. For example, the importance of the science question being addressed was unconvincing (e.g., how does this work build on state-of-the-art understanding). The methods were also unclear, and muddled. For example, it was never clear to me whether reanalysis was meant to represent fine-scale gravity waves or not meant to. The writing was also poor / casual and the figures poorly explained, with many mistakes (I have only mentioned some of them below). I suggest that the authors should take their time with thoroughly revising this paper and improving the motivation, methods, structure, grammar, explanation of results, etc. To do this requires a fresh submission.

Major comments

1 The justification for this work is weak. The manuscript states that the ‘primary focus is to investigate the occurrence of ice PSCs observed by Envisat MIPAS (Spang et al., 2004, 2018) and characterized by temperatures above the ice existence threshold (Tice), as derived from by ERA5 reanalysis (Hersbach et al., 2020)’. But surely there has already been considerable research on this topic (e.g., Tritscher et al., 2021). This needs to be properly explained in the Introduction (i.e., knowledge gaps), and the novelty/importance of the science question then properly justified.

We agree that the occurrence of ice PSCs in connection with mountain waves as well as the underestimation of temperature fluctuations in the reanalysis is a known fact. However, to our knowledge, so far no one has performed a statistic on how many ice PSCs were observed by MIPAS during the time period 2002-2012 and how many of these occurred in connection with mountain waves, and how many of these remain explained. Spang et al. (2018) provides a climatology of MIPAS PSC observations, but does not analyze the effect of gravity waves/mountain waves in detail. We have improved the introduction and hope that the intention of our study and the current state of knowledge is now clearer.

2 The Introduction explains the importance of fine-scale mountain waves for PSC formation. It then states that such waves are poorly or under-represented in reanalysis. But it then proceeds to use reanalysis to resolve the temperature perturbations. I think the idea is that the work then goes onto to show that ice PSCs are shown to occur at temperatures warmer than Tice based on reanalysis temperatures, which implies that fine-scale mountain waves are likely responsible for lowering the temperature to below Tice – but these perturbations are not captured in reanalysis. This is not well explained, and together with my comment above that the justification for the work is weak, gives an overall impression of the paper being rather muddled.

Following this comment and similar feedback from the other referees, we revised and expanded the introduction of the paper to better explain the motivation of the present work. In particular, we added a new paragraph explaining in more detail the physical principles of how mountain or gravity waves contribute to PSC formation. We also expanded the rationale for why we study the MIPAS PSC detections in relation to ERA5
reanalysis data.

3 Some justification is necessary for using ERA5/ERA-Interim. For example how accurate is it at representing (environmental) temperatures in the stratosphere during the polar winter – which is crucial to the approach used here. Maybe you could also give some detail of the observational products assimilated in this region, etc. Also, how ERA-Interim and ERA5 are used is poorly explained and confusing. Some justification is also necessary for why two separate reanalyses were used, and not simply one. I would suggest that it would be clearer if the reanalysis has a separate sub-section in the methods, rather than being lumped together with the sections describing the models or satellite data.

As suggested, we have put the description of the reanalysis data in a separate subsection of the methods section. Only ERA5 has been used in this study. In an earlier version of the MIPAS data, ERA-Interim was used for estimating $\Delta T_{\text{ice min}}$. However, in the current version of the MIPAS data, $\Delta T_{\text{ice min}}$ is calculated using ERA5. We accidentally missed adjusting the manuscript concerning this change. The manuscript has now been revised accordingly.

4 The Methods section needs to be clearer / more coherent, as it has perhaps been written by a number of authors. It is difficult to follow and weak. Also, not sure that the title of the manuscript can solely mention ERA5, when ERA-Interim also used.

We only use ERA5 data. See our answer to comment 3. Based on the comments by several referees we have changed the title as follows: "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". We have also reviewed and improved the methods section and hope that it is now clearer. As suggested by the referee, we added an additional section describing the reanalysis data.

5 The figures and captions need to be improved. There are many mistakes, such as different font sizes, missing units. The captions are not complete enough. Especially, use either $T - T_{\text{ice}}$ in the text/figures or define $\Delta T_{\text{ice min}}$ properly and use that – but don’t use both or flip between the two.

We have checked all figures and captions and improved these. The considered $T - T_{\text{ice}}$ difference and $\Delta T_{\text{ice min}}$ are not the same. $\Delta T_{\text{ice min}}$ is used to derive the location of ice PSCs, where the temperature difference between the temperature along the line of sight and Tice is minimal. This term is used to distinguish this location from the tangent point. We have revised the term to ‘at $\Delta T_{\text{ice min}}$’ throughout the manuscript. $T - T_{\text{ice}}$ is the temperature difference.

6 There are a lot of minor/casual/obvious mistakes in the manuscript, including grammar. All these mistakes must be found and corrected. Also please check the headings. For example, 3.1 is ‘Ice PSCs detected by MIPAS’, but surely 3.2 is also MIPAS observations.

We have carefully checked the manuscript and improved the grammar and removed all mistakes in the manuscript.

Minor comments
1 Line 1: The opening line on the importance of mountain waves as a PSC formation mechanism
seems completed disconnected with most of the abstract – especially the earlier parts. This needs to be much more coherent.

Yes, we agree. We revised the beginning of the abstract as follows: "Temperature fluctuations induced by mountain waves can play a crucial role in the formation of polar stratospheric clouds (PSCs). In particular, the cold phase of the waves can lower local temperatures sufficiently to trigger PSC formation even when large-scale background temperatures are too high. To provide new quantitative constraints on the relevance of this effect, this study analyzes a decade (2002–2012) of ice PSC detections obtained from Michelson Interferometer for Passive Atmospheric Sounding (MIPAS/Envisat) measurements and ERA5 reanalysis data in the polar winter lower stratosphere.”

2 Line 3: The definition of $\Delta T_{\text{ice min}}$ is poorly described and confusing.

We have removed this sentence in the abstract and the detailed definition of $\Delta T_{\text{ice min}}$ can be found in Sect. 2.1.

"Therefore, instead of using the tangent point of the sample, we employ the point with the minimum temperature difference ($\Delta T_{\text{ice min}}$) between the frost point temperature ($T_{\text{ice}}$) and the environmental temperature along the line of sight ($\Delta T_{\text{ice min}} = \min(T_{\text{LoS}} - T_{\text{ice}})$), up to a maximum altitude of 30 km, to identify the most probable position of the ice PSC observation.”

3 Line 27: I would quantify what you mean by small-scale waves, i.e., explicitly give the scale in km.

We do not refer here to small-scale waves, but to small-scale temperature fluctuations. These can decrease the temperature by several degrees so that the temperature threshold for solid PSC formation, e.g. ice, are reached. These temperature fluctuations and their amplitudes cannot be resolved by the ERA5 grid.

4 Line 30: This sentence is not quite clear. Are you saying that 63% of all ice PSCs during the period of interest were associated with mountain waves? Please clarify.

Yes, Noel and Pitts (2012) analysed CALIPSO data covering the time period from 2006-2010 and found that 63% of ice PSC were observed during GW events.

5 Line 41: Grammar. Suggest 'Carslaw et al. (1999) unveiled'.

We changed this to "have unveiled"

6 Line 44: Maybe better to refer to ‘mountain waves’ throughout the text, rather than swop between gravity waves and mountain waves.

Although most case studies focus on gravity waves from orographic sources, Hitchman et al. (2003) and Shibata et al. (2003) showed that non-orographic gravity waves can also trigger PSC formation. Nevertheless, we have reviewed the text and carefully checked that the terms "gravity waves" and "mountain waves" are used appropriately.

7 Line 43: I’m not quite sure what the sentence beginning ‘Temperature perturbations . . . ’ is trying to say here.
We removed this sentence.

8 Line 44: The waves are ‘fine-scale’ temperature fluctuations, not ‘subgrid-scale’. And its not ‘may not be fully resolved’, its ‘are underestimated / not fully resolved’.

Yes, you are right. The sentence has been revised.

9 Line 61-62: Slightly awkward sentence. Can it be revised.

We revised the sentence as follows: ”The Michelson Interferometer for Passive Atmospheric Sounding (MIPAS) on board the Envisat satellite measured limb infrared spectra in the 4 – 15 µm wavelength range with high resolution from the mid-troposphere to the mesosphere.”

10 Line 68: The CI needs to be defined first, before a statement can be made saying its sensitive to PSCs. This paragraph maybe needs some reorganisation.

We added the following paragraph introducing the cloud index method: ”Spang et al. (2001) introduced a simple and reliable method for detecting clouds in infrared limb sounder measurements by comparing the mean radiances of two different spectral wavelength regions. Each region responds differently to clouds in the field of view. The first region, 788 – 796 cm$^{-1}$, is primarily influenced by CO$_2$ emissions and shows little change in the presence of optically thin clouds. In contrast, the second region, 832 – 834 cm$^{-1}$, is located in an atmospheric window region and mainly influenced by aerosol and cloud emissions. The ratio of radiances, called the cloud index (CI), is high for cloud-free conditions (CI > 4), close to one for optically thick conditions, and falls in between for the transition from optically thin to thick clouds.”

11 Line 74: ICE$_{NAT}$, STS$_{NAT}$ etc need to be properly explained/defined, ie state that class #4 is a mixture of ice and NAT PSCs.

We have added the following sentence: ”Class 1-3 are pure type classes, while class 4-6 are mixed type classes.”.

12 Line 86: Please see comment above about the CI not being properly defined. This needs to be done so that the reader understands what CI > 1.2 means.

We tried to clarify this with the reply to comment 10 on line 68.

13 Line 47 and 85: Not sure whether its ERA-Interim or ERA5 being used. Please clarify. Title says ERA5. Also, need some explanation of how reliable reanalysis is for this task.

Only ERA5 data has been used in this study as clarified in our answers to your comments 3 and 4. The introduction of ERA5 data can be found in Section 2.3 and its temperature uncertainty has been discussed in Section 4.3.

14 Line 95: MPTRAC already defined.

Correct. We have omitted defining MPTRAC here once again.

15 Line 98. Grammar. I think this should ‘e.g.’ and not ‘i.e.’.
We changed "i.e." to "e.g.".

16 Line 99: FLEXPART needs to be defined.

Done.

17 Line 107 to 111: This paragraph should be included when ERA-Interim is first used in the methods. The heading for section 2.2 should also be revised.

This paragraph has been removed and the title of subsection 2.2. has been adjusted.

18 Line 120: Repetition. It was only just mentioned earlier that ERA5 was used to calculate Tice.

Sentence has been rephrased so that this repetition is omitted.

19 Line 134: This sub-heading does not seem appropriate. Is only 3.1 focused on MIPAS?

Also 3.2 focused on MIPAS data. We have adjusted the subsection title of 3.1 to ”Ice PSC observations”.

20 Line 135: Repetition. This has already been explained in the methods. And if wasn’t completely explained there, then absolutely should be.

Indeed we have already explained this, but think that this repetition here does not matter.

21 Line 138: Please refer to latitude and longitude correctly. ‘south of 65deg’ means nothing, unless its written 65S. Also ‘longitude range of +-90deg’ is also wrong. Please ensure that lat and lon are correctly described everywhere in the text.

Done.

22 Line 139: Grammar. Highest occurrence frequency over 16%. Also ‘Over the seasons’ – not sure what that means.

Sentence has been corrected and ”Over the seasons” has been changed to ”Over the course of the year”

23 Line 145: Sentence beginning ‘In both polar regions’ does not make sense.

This sentence has been removed due to the comment by referee 1.

24 Figure 1: Colour bars are not labelled. The caption does not mention what the different panels are. Why are different font sizes used for the labels in panels b and d? Presumably panels b and d show mean values over the entire polar region? - This is not clear from the caption. In the figure, I don’t think that the labels for each panel are necessary or even helpful.

The left and right figures have the same colour bar and the labelling is solely done on the panels on the right side. The panels a and b show the occurrence frequency for the Antarctic and panels c and d for the Arctic. What the different panels show has been added to the figure caption.

25 Figure 1: This is unclear: difference between Tice and T along the line of sight (ΔTice_min). Why have such a convoluted way of explaining what Tice-T is? Also, it should be written as
\[ \Delta T_{\text{ice, min}} = T - \text{Tice} \] (ie as a formula).

We consider here the minimum difference between the temperature at the line of sight TLoS and Tice, thus \( \Delta T_{\text{ice, min}} = \min(\text{TLoS} - \text{Tice}) \). We have added the formula in the text.

26 Lines 144-145: Convoluted way of explaining what is in Fig. 2. Either use T-Tice everywhere, or defined \( \Delta \text{Tice} = T - \text{Tice} \) and use this everywhere.

Please see our response to major comment 5. \( \Delta \text{Tice, min} \) and T-Tice are not the same. \( \Delta \text{Tice, min} \) is used to derive a specific location where the temperature difference is at its minimum, indicating the presence of ice PSCs. T - Tice is a general expression for temperature difference.

27 Figure 2: Same comments as above. Figure does not have units of temperature. The label \( \Delta \text{Tice, min} \) is wrong as the axis is already labelled as T-Tice. The panel labelling of ‘ICE’ is unnecessary. Also, not sure what ‘Fraction to observations’ means.

We have improved the figure. Please find the explanation of the difference between \( \Delta \text{Tice, min} \) and T - Tice in response to major comment 5.

28 Line 147: Grammar. Comma after 56% not required.

Correct. The comma has been removed.

29 Line 148: ‘Derived from ERA5 reanalysis’ not required.

Fixed.

30 Line 149-150: Repetition. T-3K and T-1.5K are already explained.

We have consolidated the duplicate parts.

31 Line 150: Is T-Tice-3K correct? Should it not be Tice-3K? Why is a ‘)’ included here.

It should read Tice-3K. We have corrected this and removed the obsolete parenthesis.

32 Line 154: Please revise sub-headings. I’m not sure how the material in this sub-section differs from the previous section.

Both sections deal with the same data set, namely MIPAS, but these differ, because in 3.1 we describe the MIPAS ice PSC observations in general and in section 3.2 we focus on the ics PSCs that were observed at temperatures above Tice and their characteristics. We have revised the sub-header of 3.1 to “Observation of ice PSCs”

33 Figure 3 caption: Grammar – please correct. Also, normally captions say ‘Analogous’ rather than ‘Similar’ in this context.

We changed “Similar to” to “Same as”.

34 Line 160: Not sure use of ‘trend’ is appropriate here. Could simply say ‘decrease in altitude throughout winter’.
We agree and rephrased the sentence to omit the term "trend".

35 Line 161: This is a strange sentence, and does not make sense. Also, why is the physically basis for this rather randomly explained here, but not for other results?

The sentence has been revised. In the frame of the revision we have added explanations on the physical basis at several places.

36 Line 165: Rather than writing just ‘comparable’, you also need to give the values.

Values have been added. "the fraction of ice PSCs above Tice is 51% in January and February, and 70% in December due to less ice PSC observations."

37 Line 167-168: This sentence makes no sense. ‘lower occurrence of ice PSCs . . . due to few ice PSCs’.

The Arctic winter 2004/2005 and 2010/2011 were both exceptionally cold winters. Thus, the observed ice PSCs during these winters occurred rather at temperatures below Tice than above Tice (i.e. during these winters ice PSCs were rather formed by synoptical cooling than by mountain wave induced cooling by temperature fluctuations). We have rephrased the sentence.

38 Line 169: Again, the explanation for this points is welcomed for its insight, but should be done consistently, or saved for the discussion. Broad statements such as the stability of the Antarctic vortex v Arctic vortex should be introduced in the Introduction – also surely results such as this have been readily explained / shown elsewhere.

With the sentences we added on L142 the differences between the Arctic and Antarctic vortex should be now more clear.

39 Line 172: Poor English.

The sentences have been corrected.

40 Figure 4: Please improve formatting of plots, such as the values on the axis. These are different for panels b and d, despite both panels being identical.

Thanks, we have improved the figure.

41 Line 175: What does ‘point of observation at ∆ Tice_min’ mean?

∆Tice_min is used to pinpoint the exact location of ice PSCs. We revised this sentence to “we employed the MPTRAC model to calculate 24-hour backward trajectories from the ice PSCs observed at ∆Tice_min.”

42 Line 176: ‘(t) t’

This has been corrected.

43 Line 179: These are not a ‘trend’. Please use a different word.

We changed "trend” to ”behaviour”.
Figure 5: Not sure what label of vertical axis means. Caption is also far to brief and not enough information for the reader to understand the plot.

We have improved the figure and caption.

Lines 184-188: This text should be in the methods section. Why is the methods being explained in the results section?

We have removed the text from the result section.

Line 191: What is 't=-0'?

The minus is obsolete and has been deleted.

Line 193: $h^{-2}$ is not a rate. Rate is per hour.

This is not the rate, but the variance. Please note that we corrected the units of the cooling rate variances to $K^2 h^{-2}$ throughout the manuscript.

Figure 6 caption: This seems to be written by a different person as the previous captions were brief. But surely no need to define T for temperature at this stage of the paper.

We agree that that temperature at this stage of the paper does not to be introduced and thus we removed "(T)".

Line 202: Confused here, as introduction stated that ERA5 poorly resolves temperature fluctuations (fine-scale) but here says that it does.

We agree that this sentence was misleading. We rephrased: "This suggests that the time and location of temperature variations in ERA5 can still be related to the presence of ice PSCs above $T_{ice}$, especially in the Arctic, even if we consider that gravity wave amplitudes may be underestimated.”

Line 228: What does warm large spatial scales mean?

We have rephrased the sentence as follows: "The occurrence of ice PSCs in warm environments have already been reported in previous studies”.

This claim that ERA5 misrepresents fine-scale waves has never been justified in the paper. What study are you referring to?

In the introduction, we pointed out: "However, the small-scale temperature fluctuations related to mountain waves are often underestimated or not fully resolved in global reanalyses or coarse-resolution chemistry-climate models (Orr et al., 2015; Hoffmann et al., 2017; Orr et al., 2020; Weimer et al., 2021).” We have added here the reference of Orr et al. (2015), as they also discuss the difficulties of properly representing the effects of mountain waves on PSC formation in numerical simulations.

The study by Hoffmann et al. (2017, see Sect. 4) evaluated gravity wave variances from ECMWF IFS operational analyses from T511 (39 km effective resolution) to T1279 (16 km) in comparison with Atmospheric InfraRed Sounder (AIRS) satellite observations, and showed that the IFS simulations largely (up to a factor of $\sim6$) underestimated the
measured variances.

A new study by Lear et al. (2024) provides to our knowledge the first quantitative analysis of the representation of explicitly resolved gravity waves in ERA5 based on comparison with AIRS satellite measurements. The study supports the conclusion that gravity wave amplitudes are underestimated in ERA5.

References


