Response to Reviewer 2

I thank Zachary for bringing up remaining points.

1) I still think the manuscript organization needs work. I have no strong preference for how it should be reorganized, but I find the current structure unhelpful: most of the manuscript is contained in the “Data and Methods” section, which has 13 parts across subsections and sub-subsections. I have not encountered a published paper that places the bulk of its results in the methods section, and some arguments (see next comment) are confusingly split across multiple sections without these referencing each other.

Point discussed separately with the handling editor.

2) I still do not see clear evidence supporting the stated finding that ice number discrepancies are “traced back to filtering of the balloon data” (line 11). The response to my review points to lines 297–318 and Fig. 6, but I see no discussion of filtering there. Filtering is discussed later, in lines 366–375, without reference to any figure in this paper and with only two external studies cited. As written, this discussion reads more like background from previous studies than a finding of the present study. I would appreciate a clearer presentation of the argument and clarification of two related points:

As I am working with the filtered data set (stated in lines 72f), I am not discussing the associated change in fluctuation amplitudes or the removal of power near the

buoyancy frequency due to the filtering, which have been studied in earlier work.

For clarification, I now add after line 86: “The effect of high frequency filtering on the distribution are shown in Podglajen et al (2016, Fig.2).”

a. Line 366 states that the “power contained in the vertical wind fluctuations near the buoyancy frequency can be enhanced by trapping.” Is there evidence of this in the power spectra in Fig. 4, or is the evidence entirely external to the present study? The SPB spectra have roughly twice the power of the triangular parameterization across much of the frequency range shown, but not near the buoyancy frequency, N, where the SPB measurements are cut off. Relatedly, why are the triangular and sinc fits not shown closer to the data at these lower frequencies? Do we have evidence that the SPB power would decrease to the level of the parameterizations in the absence of trapping, or could the discrepancy instead reflect a poor fit by the parameterizations? I wonder whether the suitability of the different parameterizations is being conveyed appropriately; my own analysis during the earlier review found a much closer match between the SPB data and the sinc fit.

No, according to my reply above, any evidence there might have been has been removed in the filtered data set, so this evidence is external to the present study. The same holds true for turbulence (line 74). Note that I mentioned the effect of filtering on the power spectrum

in lines 118-120. I now add in 118: “The effect of filtering on the power spectrum are shown in Jensen et al (2026, their Fig.1).”

The triangular and sinc fits shown in Figs.3+4 represent idealized models that haven’t been processed by a Bessel band pass filter similar to the balloon data, hence they cannot be closer to the filtered data. We do not know how the measurement data look like without the filtering due to the necessity to remove artificial balloon oscillations (see lines 339-342 and 371f).

b. Can Fig. 6 be more explicitly connected to the discussion of trapping? Is the connection simply that Fig. 6 shows the parameterizations yielding excessively high ice crystal numbers, while trapping is one mechanism that could theoretically produce more ice crystals than the parameterizations account for? Or is there a more direct link? Specifically, is there sufficient evidence that the discrepancy can be explained by differences in the power spectra in Fig. 4 at frequencies near and above N? My concern is whether sufficient confidence exists for the conclusion that these discrepancies are “traced back to filtering of the balloon data.”

According to the above, we cannot make any inferences about the role of trapping (and turbulence, for that matter), which is why I do not wish to speculate on this issue in Fig.6. Regardless, it is clear that if trapping would occur in nature, it would increase homogeneously nucleated ice crystal numbers (lines 369-371) due to the

strong dependence of homogeneous aerosol freezing to cooling rates.

3) The “small discrepancies” lead to differences in mean ice crystal number of around 50%, and the PDFs show substantial differences when plotted on a linear scale. Perhaps “moderate discrepancies” would be more appropriate?

Agreed. I now use “moderate” in the abstract as suggested. According to the above and previous replies, it is not possible to further disentangle the reason for the discrepancies (filtering vs behavior of parameterizations near N), so I continue to list both in the abstract. As stated in the first round of reviews, this prevents deeper interpretation regarding the suitability of the parameterizations at this point.

4) Lines 55–56: “extends this work” rather than “is extended”?

Done, thank you for spotting this.

Response to Editor

I thank Blaž for suggesting structural changes.

I have one major concern that is not about the science presented. In its present form, the manuscript is suited for a "technical note" and not a standard ACP paper.

Most of the manuscript is in the Methods section, and, most crucially, the Results section is limited to a single modeling result presented in Figure 5 and again in Figure 6, in the context of other work. Additionally, while the title is more specific than in its first submission, it is still not "result-oriented" according to ACP guidelines (https://www.atmospheric-chemistry-and-physics.net/policies/guidelines_for_authors.html).

Currently, I see two options on how to proceed. The first is very easy and straightforward, and it's therefore in my opinion the preferred and most rational option. The second would prolong further the review process.

Option 1: We can keep the article as is (with you addressing the final few small scientific comments) and move it to a "Technical Note". This means the title must start with "Technical Note," so it could be "Technical Note: Modeling Frameworks for Gravity Wave Representation in Tropical Tropopause Layer Cirrus Formation Context," or something similar.

2. If you prefer to keep it as a standard ACP article, major restructuring is needed. The manuscript must be presented differently, with a greater focus on the results and a lesser focus on the methods. Currently, there is only one figure showing the results of this work (Figure 5). Figure 6 adds more context to your results and could be considered a "discussion figure."

I apologize for not noticing that in my first reading of the manuscript. If I had noticed that, I would have suggested

switching to a "technical note" at that stage. I am repeating myself here - but for option 2 the title should reflect the key outcome of the paper and in such way fulfil the ACP guidelines.

Other comments on the manuscript structure, with some overlap with Reviewer 2:
1. It's unusual to see that many subsections in Section 2 of the manuscript. It's even more unusual to see sections formed by only a few sentences. I suggest merging a few of them.
2. The Conclusion section's summary is not just a summary. From line 339 on, it is a discussion. Additionally, the initial paragraph from lines 320–326 also reads as a summary. I'm not sure what the best way to structure it is, but I'm pointing out what I see as inconsistencies.

Finally, while none of the reviewers clearly pointed this out, I would appreciate broader statements about the meaning of what is presented in Section 2 to make the manuscript more accessible to a broader audience. I often wondered about the relevance of some of the choices made, and in its current form, that is difficult to understand without a thorough read and a substantial background knowledge on the topic. Again, I apologize for bringing up the switch to a "technical note" so late in the review process. This does not reflect the quality of the science presented but rather the structure of the manuscript.

A good part of your judgement may be caused by an

unfortunate division and description of sections at different levels. I agree this can be improved. However, I respectfully disagree with the notion that “only one figure (Fig.5) shows results of this work”. In my view, a Technical Note would shift the focus more towards the description of details of the implementation of the wav1e parameterizations in both cloud models and nucleation parameterizations, while the present study makes a link to actual applications in microphysical simulations of a cloud process. That said, I agree that some technical details need to be explained, but these are limited to the representation of low concentration freezing events and the implementation of gravity wave forcing in the cloud model. I believe that for fellow scientists leaning towards an application of the methods, the description of the parameterizations and their link to both the measurement data and the cloud model is very helpful.

On the one hand, the data analysis contains results as well (the autocorrelation and power spectral analyses in Figs.3+4) and also the interpretation of the stochastic methods and link to the data analysis goes beyond technicalities. On the other hand, section 2 prepares the reader for the ‘Results’ section 3, which focuses on the link to TTL cirrus formation (application of the parameterizations introduced earlier). Also, Fig. 6 contains results from applying the parameterizations, which by itself is not a simple extension of Fig.5 for discussion.

To find a middle way that satisfies your concern and preserves the logic and integrity of the previous

structure, I'd like to suggest a major restructuring of the manuscript replacing lines 57-59 with: "Section 2 introduces and analyses the SPB measurements. Section 3 presents the microphysical model alongside its coupling with the GW forcing. Section 4 describes the stochastic WCP approaches and their intersections with the SPB data. Section 5 presents and discusses effects of GW activity on TTL cirrus formation. Section 6 summarizes the main findings and elaborates on future avenues of research that follow from this work."

In particular, in the new conclusions section, I move lines 320-326 to the new summary subsection and lines 339-349 to the new discussion subsection. In addition, I have merged the previous opening paragraph into the new summary to avoid duplication. With regard to sections with "only a few sentences", I identify only two (sections 2.1.2 and 2.3.4). As level-3 subsections now turn into regular subsections, I integrate the latter into the preceding one but opt to retain the former for clarity and as it refers to a specific figure. The short section 2.6 is now contained in the new main model section, which also integrates appendices A and B as separate subsections.

In sum, the full new manuscript structure reads:

Section 1: Introduction
Section 2: Superpressure balloon data analysis
    Section 2.1: Time series of vertical wind speed fluctuations
    Section 2.2: Distribution of fluctuation amplitudes

Section 2.3: Temporal autocorrelation function
Section 2.4: Power spectral density
Section 3: Microphysical simulation model
Section 3.1: Salient features
Section 3.2: Model equations
Section 3.3: Implementation of gravity wave forcing
Section 3.4: Numerical representation of low concentration homogeneous freezing events
Section 4: Wave-cirrus parameterizations
Section 4.1: Laplacian fluctuations with triangular autocorrelation
Section 4.2: Laplacian fluctuations with exponential autocorrelation
Section 4.3: Gaussian fluctuations with exponential autocorrelation
Section 4.4: Notes on power spectra and temperature fluctuations
Section 5: Ice nucleation simulations
Section 5.1: SPB forcing
Section 5.2: WCP forcings
Section 6: Conclusions
Section 6.1: Summary
Section 6.2: Discussion and implications

In addition, I suggest to use the title “Assessing gravity wave activity in TTL cirrus formation”, describing better what the paper specifically investigates. Results are presented and discussed in sections 5.1, 5.2, and 6.2.

I hope these extensive structural changes to the revised manuscript meet your approval.