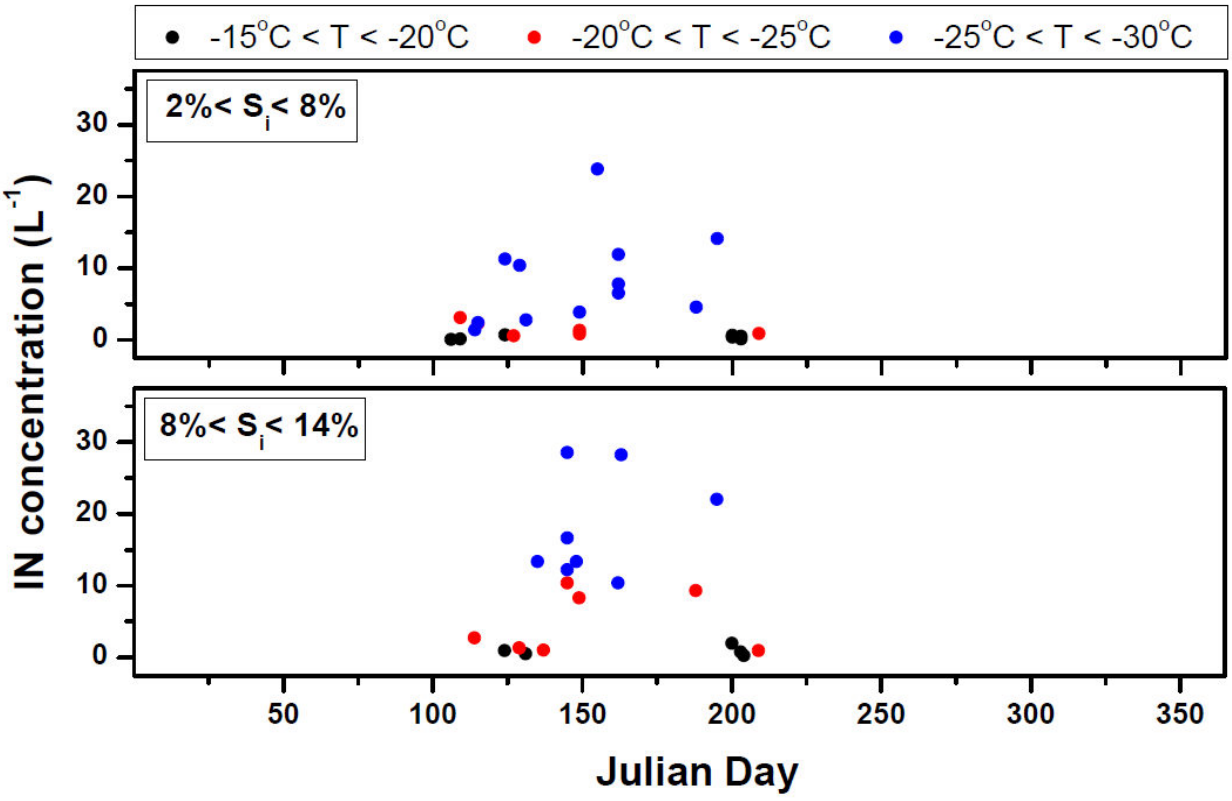
Reply to Anonymous Referee #2

We thank the Referee for the helpful suggestions which resulted in an improved manuscript.

The answers are in red.

A four month time series of ice nuclei concentrations is worth publishing in and of itself. There's a scarcity of ice nuclei measurements (especially in the Southern Hemisphere) and most are very limited in temporal extent. Given the utility of the time series, my primary recommendation to the authors is to include a plot of the time series. Pick a couple of supersaturations and plot the concentration of ice nuclei at those supersaturations as a function of time. I realize that you don't have the data to define a seasonal cycle, but even plotting the data over the time you do have would show some indication of the variability.

The Figure below shows the number concentration of IN as a function of the Julian day for different temperatures and supersaturations. Although it is not possible to perform a reliable statistical test, the results do not seem to show evidence of any particular trend during this period. Anyway, we think that the period is short to display any indication of the seasonal variability. This information (figure + comment) will be included in the revised manuscript.



Variability or uncertainty leads to my second major recommendation. These values are almost meaningless without some idea of the uncertainty. As an example, the bottom panel of Figure 4 shows a local maximum in IN concentration at S = 14% and T = −24C. The concentrations then decrease(!) as T falls further at the same supersaturation, rising again as T falls to about −30. This is unphysical. Clearly there's an uncertainty in the reported values that would smooth that unphysical local maximum out.

Aside from the inherent variability in the concentration of the ice nuclei, there must also be some uncertainty associated with the technique. The authors discuss uncertainty in S on page 31707 (lines 9 through 13). There's a mention of the uncertainty associated with the counting of activated ice crytals on line 27 of that same page. Combining the uncertainty associated with the technique and the uncertainty associated with the variability of the concentrations and showing that uncertainty on the plots would help the reader to interpret them. (You don't need to show the uncertainty for every data point. I think an error bar at the low and high end of the concentration range would suffice.) As an example, it looks as if the unphysical local maximum shown in the contour plot in Figure 4 would be within the experimental uncertainty.

We agree with the Reviewer that the variability of the results are related to both errors associated with the technique and the inherent variability in the IN concentration. Error bars to some experimental data points will be included in Figures 5 and 6, as suggested by the Reviewer and the paragraph (Page 31709, line 12) is changed in the revised manuscript in order to take into account his recommendation. The change will be as follow, Page 31709, line 12:

This figure (FIGURE 4) shows an important variability of the results which is related to errors associated with the measurement technique (uncertainties in supersaturation, temperature and activated ice crystals counting) as well as to the inherent variability in the IN concentration. Nevertheless, the results clearly show that the number ……

Editorial and minor comments

Abstract: ".. can initiate freezing at temperatures below -15 C." Why -15? There are documented instances of freezing at higher temperatures. It isn't very common, but it has happened.

The Reviewer is right. The paragraph will be changed to:

".. typically initiate freezing at temperatures below -15 C; although occasionally freezing occurs at higher temperatures."

Pg. 31701, line 14: I think the comma after the parentheses should be a semicolon. OK.

Pgs. 31701 and 31702, discussion of ice multiplication processes: I don't think this discussion is warranted here. The paper isn't about ice multiplication. This comment is just my opinion. Feel free to ignore it.

We think that the mention of the ice multiplication mechanisms is pertinent to explain that in some cases the ice crystal concentration in clouds differs in orders of magnitude with the IN concentration.

Pg. 31703, line 13: "condition" should be "conditions". OK.

Pg. 31703, line 27: "...no agreement has been reached as to the most appropriate technique..." I'll throw in another personal opinion here. There is no most appropriate technique. The technique to be used depends on the question being asked. Every technique and instrument has advantages and disadvantages.

The question being asked in this case is the technique to quantify the IN concentration.

Pg. 31705, lines 10 through 21: The description of replenishing the humid air in the box is ambiguous. Are there aerosol particles in the humid air that's injected into the chamber to increase S? If not, the number of aerosol particles in the chamber is decreasing because you remove air to keep the pressure in the chamber constant. Please clarify this.

The air injected into the chamber has the same aerosol concentration as the air inside the chamber. It will be clarified in the revised manuscript.

Discussion of the parameterization of the results, Pgs. 31711 and 31712: Another editorial comment. Feel free to ignore it. In my opinion, this discussion is of limited utility. While I think, as I said above, that publication of the results is warranted, I do not think a parameterization is necessary. The time series of the IN concentration is longer than most, but it is still only 4 months.

It is very encouraging for us to find that the parameterization coefficients are in good agreement with those found by other authors. We take it as one more indicator that these results are consistent with previous results. Considering the lack of information regarding IN concentration in the South Hemisphere, we think that the parameterization is also useful to compare the IN concentration for parameter ranges which have not been determined yet.