Response to the reviewers

August 2024

We thank the reviewers for their assessment and ideas to improve our work. In the following, we address their suggestions point by point, which has allowed us to remove some weaknesses our manuscript still had.

The authors present the results of a modeling study aimed at understanding the controls exerted by ice nucleating particles (INPs) on liquid water contents of Antarctic / Southern Ocean mixed phase clouds. Winter and summer numerical modeling simulations of cloud systems were set up, using prescribed CCN and INP concentrations, and varying the latter to investigate the impacts. The runs are guided by observational case studies. As the authors note, the simulations are generally qualitatively representative of the observations but "the model fails to accurately represent the long-lived, dense liquid and mixed-phase layers observed at the (continental) station". Indeed this is an ongoing issue in the simulation of polar mixed phase clouds that this work cannot address. Further, radiative biases are present in the model, but are noted to be on the same order as those reported in prior modeling studies.

The first main finding of this work is that contrasting air temperatures over land and ocean led to different seasonal impacts. Cold wintertime temperatures over the content supported homogeneous freezing and thus variations in INP had little impact, a perhaps expected finding. Over the ocean in wintertime, temperature were moderate enough such that liquid water contents responded strongly to INP concentrations, whereas in summertime, temperature were warm enough to limit ice formation regardless of INP concentrations. The second main finding is that the simulated changes in liquid water content have only a small radiative effect. This result seems to indicate that concern over changes in the radiation balance in the Antarctic induced by changes in cloud phase might be overstated.

An aspect of this work that represents an advance over prior treatments is that an INP budget is included, that has 16 different temperature activation bins, enabling a more realistic representation than fixed-INP concentrations. However, the slope of the cumulative distribution is fixed, using the DeMott et al. (2010) parameterization.

Overall, there is a nice description of the modeling system which is a useful addition to the literature, and the work takes a methodical approach to explore sensitivities. I have the following suggestions for changes to the paper. A number of these questions relate to the INP observations, although in the end they are used primarily to provide bounds on the variation input to the model.

If those measurements are not described elsewhere then I suggest to improve that discussion so that this work can serve to report them.

Comments

Comment 1 — Abstract: when citing the INP concentrations (line 3) please indicate these are at -20 C. and that the analysis method used refers to immersion freezing INPs.

Reply — Those are some important information. We adapted the sentence:

- Ice-nucleating particles (INPs) have an important function in the freezing of clouds, but are rare in East Antarctica. At the Belgian Princess Elisabeth Station, immersion freezing INP concentrations between $6 \times 10^{-6}\,\mathrm{L}^{-1}$ and $5 \times 10^{-3}\,\mathrm{L}^{-1}$ have been observed with an activation temperature of -20°C.

Comment 2 — line 48: the Kay et al. (2012) reference is over a decade old; have there been updates to CESM that have reduced the cited bias?

Reply — There has indeed been some progress in CESM2, as can be seen in [Gettelman et al., 2020]. We added a sentence in the text to reflect that update.

- The community earth system model (CESM) has a 30 Wm^{-2} (warm) bias in CRE_{SW} and a -10 Wm^{-2} (cold) bias in CRE_{LW} over the Southern Ocean [Kay et al., 2012] in version 1. This has since been reduced in CESM2 with the community atmosphere model (CAM) version 6 [Gettelman et al., 2020].

Comment 3 — line 113: what is the pore size of the filters? not much detail is provided about the INP observations. Does the implementation begin at -15 C because the samples were below the limit of detection at warmer temperatures? could the DeMott (2010) parameterization be extended to "fill in" some reasonable values? why not use the slope of measured INP spectra, instead of the slope of this fit based on the DeMott global (not polar) fit?

Comment 4 — line 114: how is the inlet oriented? was there no precipitation shield? what sampling inlet losses might be expected (the dimensions of the tube are not provided)?

Comment 5 — line 118: how were blank corrections handled?

Reply — We have reworked and expanded the measurement section in order to include more details, also in response to Reviewer 1's comment. A paper dedicated to the measurements is also currently in preparation. As for the inlet, we added some information about orientation and design in the section. There was no further specific precipitation shield or inlet size cut-off. Precipitation at PE is only snow or drifting/blowing snow. With a specific inlet, the risk of clogging is high during elevated wind speed periods. And heating is no option at the ambient temperatures, there would be immediate re-freezing, producing an ice cover. The simple inlet tubing provided therefore the best option.

- In addition to the weather and cloud observations, ground-based INP measurements were taken in the 2020/21 and 2021/22 austral summers. These INP measurements were taken using 47 mm polytetrafluorethylene filters with a pore size of 800 nm (Whatman Nuclepore No. 10417312), which were set up in a shelter around 500 m north of PEA station. The 47 mm filters were placed inside a hard plastic filter holder. This had a metal cap (inversed funnel type) with an inlet opening of 0.25" diameter. On it, a 15 cm piece of black conductive silicon tubing with a 0.25" outer diameter and 0.19" inner diameter was fitted. The filter holder was situated outside, located 50 cm above the shelter's roof. The end of the 15 cm conductive tubing pointed downward and was oriented perpendicular to the main wind direction (NE). The sampling losses within the 15 cm tube are negligible at the given flow rate and with particles larger than 1 µm being very rare in the PEA area [Herenz et al., 2019]. Sample duration was around 10 days per filter, and each season, blank samples were taken. The subsequent measurements were done in the same way as in Sze et al. [2022], using the two well-established off-line techniques LINA (Leipzig ice nucleation array) and INDA [ice nucleation droplet array; Lacher et al., 2024]. The INP profiles of the blank samples were substracted from the measurement results, although the difference compared to the results derived from the samples directly was very small. Our observations at PEA are compared here with observations taken from literature in order to identify suitable INP concentrations to use for the sensitivity experiments performed with COSMO-CLM^2.

As for the used parametrisation, indeed, the measured INP versus temperature profile was flatter at lower temperatures compared to the DeMott parametrisation we used, however, the measurements were not yet fully evaluated at the start of the modelling. Adding another profile would be an interesting addition, and we are planning to do that for our next publications, however, our focus here lies on the differences between different overall INP concentrations and not so much on the spectra itself. It indeed gives us too many INPs at lower temperatures, which is a point we addressed in the Discussion, we edited it in order to clarify that a bit more. The main advantage of using the DeMott-parametrisation is that it makes our results more comparable to previous papers.

- The distribution of activation temperatures, as prescribed in Eq. (2), might also be a source of inaccuracy, as we have only tested one distribution based on the parametrisation by DeMott et al. [2010] and used a scaling factor for different INP concentrations. Other distributions often have a lower increase in the INP concentration at lower temperatures, such as the "MARCUS fit" (Measurement of Aerosols, Radiation and Clouds over the Southern Ocean) presented in Vignon et al. [2021], which does not have any additional INPs activating in the lower temperature range below about $-30\,\mathrm{C}$, while having a steeper increase in activated INPs between $-15\,\mathrm{C}$ and $-30\,\mathrm{C}$.

Comment 6 — Section 3.1: how is the INP budget handled during model spin-up time (is such spin up time considered?) In other words, as INP are removed from the domain (although also regenerated from evaporating precipitation and advected in), are the INP concentrations during the analyzed period markedly different from the initial condition?

It would be interesting to see a timeline / contour plot showing the budget of INPs in the simulations, perhaps selecting the -20C point in the spectrum for this, and including a discussion of any influence on the findings.

Reply — We added a figure in the appendix that shows the INP concentration over time for the summer period in two of our cases and added a paragraph in the Discussion to address this valid concern. We expect spin-up issues to be low, thanks to the frequent exchange of air masses, so we did not include a specific spin-up time, however, we still focused our analysis on the time periods towards the end of the simulation.

- The spin-up time is expected to be low, due to the frequent and fast exchange of air masses in relation to the domain size. As can be seen in Fig. A3, INP concentrations drop slightly initially, but stay close underneath their prescribed concentration. There is a significant drop at the end of the simulation period, but this drop is likely not related to spin-up, as after 2 months of simulation, all initial air masses should have been exchanged. The L and H settings are very similar in their INP timeline too, indicating that the deviations in concentrations are caused by synoptic-scale weather systems and not spin-up errors.

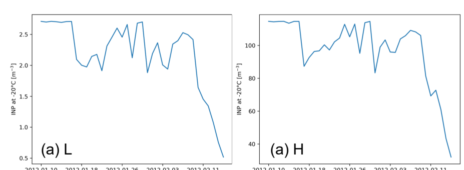

Figure A3: Average INP concentration at -20°C in the L (low INP concentration, $5 \times 10^{-3}\,\mathrm{L}^{-1}$) setting (a) and H (high INP concentration, $2 \times 10^{-1}\,\mathrm{L}^{-1}$) setting (b) for a 21x21 area around PEA at a height of 2250m.

Comment 7 — line 174: is rime splintering ever active in these simulations? This mechanism relies on the presence of specific hydrometeor types and of certain size. Evidence for secondary ice production (SIP) in Southern Ocean clouds has been published, so excluding relevant SIP processes might be a shortcoming of this work. I was confused by the statement on line 208 that increasing INPs was equivalent to representing some secondary ice production modes (although the authors note in line 201 “this is not a very accurate assumption” and again qualify this approach in the Discussion (line 355)).

Reply — While we do not have immediate evidence that rime splintering was active in the clouds we observed, our parametrisation for rime splintering is generally active at temperatures between 265 and 270 K and does not require a specific kind of ice particles to be present. These temperatures were not found in the winter (as shown in Figure A1), but fairly common across the summer period at lower levels at PEA as well as over the ocean, so rime splintering was likely active. It might be restricted by our highest temperature INP activation temperature being -15°C however, as this way, rime splintering relies on preexisting ice, either imported from the model boundaries or by the surrounding air briefly cooling down below -15°C. This should be addressed in further model development and we added a short section to the last paragraph of the discussion. We also recognize that not implementing all relevant SIP processes is a limitation of our work, but we believe it is justifiable as we are only looking at the cloud sensitivity to INPs. While these effects can be changed by SIPs, we expect that the average effect will be caused by enhancing the ICNC by a factor of 10 as stated, which can be approximated by increasing the INP concentration by that factor.

- On the higher temperature end, having the highest INP activation temperature at -15°C is a simplification as well. The concentration of INPs activating at such higher temperatures is extremely small and would likely have no measurable effect. However, not having any INPs means that rime splintering, which is active in the temperature range between -3°C and -8°C, has to rely on small amounts of ice already existing, as there is no primary ice nucleation active in that temperature range that could initiate secondary ice production. Possibly, this would increase the effects of INP concentration over the ocean in summer, which we found to be very low, as such higher temperatures are mostly found there.

Comment 8 — Line 258: "ice droplets" should be "ice crystals"?
Reply — Indeed, we adapted the text.

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