Review for "**Impacts of Arctic warming on ice nucleating particles over recent decades: Distributions and contributions of dust, marine organic aerosols, and bioaerosols"** by Ren et al.

General Comments

The manuscript by Ren et al explores ice nucleating particles (INPs) in the Arctic from 40 years of modelling simulations. Included are dust, marine organic aerosols, and bioaerosols, which can all act as INPs. They compare the modelled INPs with observations, explore the spatial and vertical distribution of each INP species and further explore changes during these last 40 years. They find that while emissions have increased over the last 40 years, the temperature increase is hindering an increase in INP concentration, rather a decrease is seen due to the large temperature change.

The paper is very interesting for especially the Arctic community, provides novel work, and deserves publication. I do have a lot of comments, however. A lot of clarifications are needed. From my perspective this is rather to improve the manuscript, to make this as good as it can be rather than discouraging as I believe it to be an important source of model data for future Arctic studies.

Specific Comments

SC1 More of a clarification really. I struggle with the usage of INA as a changing variable. To me, INA is an inherent characteristic of a particle, which we know is lower at higher temperatures. However, the way I read the manuscript it sounds like the INA is changing (as in the full spectra) rather than that we simply consider a different ambient temperature at which to explore the INA at. The main problem I have is really the use of words such as "reduction and declining" when we are simply evaluating INA(T1) instead of INA(T2). In the end the same effect, but the interpretation is different. The lines mentioned below show where I have a bit of a problem with this. In Matsui et al. 2024 this is referred to the "temperature effect", why not stick with this key word after its introduction?

L49 "reduction of aerosol INA at ambient temperature"
L55 "declining INA"
L56 "INA-driven decrease" perhaps rather → "temperature-driven versus emission driven"
L66 "decreasing INA"

SC2 My main concern which might be more of a confusion. This is in regard to Fig. 3, 5 and S6. Showing dust aerosol mass/number concentration across the full temperature spectrum makes sense, but showing a presence of INP concentration at 0°C does not. How are these INPs calculated? Any parameterisation for INPs, simplistic ones only depending on temperature as well as INAS densities are only valid within a certain temperature range making it impossible to have a presence of for example dust INPs at 0°C. Perhaps an extrapolation to warmer temperatures was done? In that case, an easy fix otherwise I am a bit confused on how these INP concentrations are obtained. Please clarify.

SC3 While the focus is on the species that are locally emitted, the interesting comparison is really with the long-range transported INPs. I think these should be shown both spatially and temporally followed by the comparison to the local species.

SC4 The authors explore the parameterisation for bacteria, which is very useful as this species is yet to be well defined in modelling. But what about the parameterisation for dust? The authors use a different one outside and inside the Arctic but how much does this impact the results? What about

other parameterisations for dust and MOA? When parts of the conclusions state the importance of choosing the correct parameterisation I believe more exploration of this topic is warranted. Easiest option would be to add more parameterisation options to the offline calculations performed for Section 3.1.

SC5 Section 3.5. I'm not sure what this contributes to the story line. The LowBac and Base were explored against observations which is very useful to see, but here I do not see many new results presented in this section. I furthermore get the feeling this is also from the authors side as the figures are mostly in the supplementary information. Thus, perhaps a shorter section could be made, starting with the lines **L317-325** and discussing the LowBac plots in Fig. 8.
To me these sentences (or variations of them) might be worth keeping;
"The number concentration of Arctic bioaerosol INPs in the Arctic lower troposphere in the LowBac simulation is less than 2% of that in the Base simulation (Fig. S3i)."
"While the magnitudes of the concentrations differ considerably, the rates of reduction over the four decades are nearly identical in the Base and LowBac simulations."
"Overall, compared to the Base simulation, the LowBac simulation produces much lower Arctic bioaerosol INP concentrations that lead to lower concentration of Arctic INPs and total INPs. The result is better agreement with the observations in Northern Norway and Alaska, although both simulations perform well at the other three observation sites."

Line-by-line
L19-22 So I struggled to understand these sentences first time I read them. Perhaps it would be more clear to start the "Nevertheless" sentence rather with a statement on the increased temperature under Arctic warming, which leads to a reduction in INP concentration as it is highly temperature dependent.

L29 Here, the authors are referring to the WBF process, which first of all should be properly cited (Wegener, 1911; Bergeron, 1928; Findeisen, 1938) and also mentioned that this is the process the authors are referring to. Furthermore, this is an elusive process that requires certain environmental and microphysical conditions and is not a process that automatically occurs in mixed-phase clouds, thus a more hypothetical language (clouds "may" shift, etc) should be used

L35 "in Arctic aerosol-cloud modelling" needs citation; some options;

Morrison et al., 2009 https://doi.org/10.1002/qj.415
Fridlind and Ackerman, 2017; https://doi.org/10.1016/B978-0-12-810549-8.00007-6
Stevens et al., 2018, https://doi.org/10.5194/acp-18-11041-2018
Wallentin 2025, https://doi.org/10.5194/acp-25-6607-2025,
Wallentin, 2026 https://doi.org/10.5194/egusphere-2025-5070

L44 Also specify the large temperature dependence
L47 "at higher subzero temperatures" Higher than what? Dust? Please specify
L50 Cite this sentence (Matsui 2024). Also, why is this INP_cloud variable introduced? It is not used later in the manuscript

Section 2 Methods:
While I understand the setup is explained in Kawai et al, this paper also needs to stand alone and provide enough information for the reader to understand the setup. So in general for this section I would like to see some details, some are mentioned below. Kawai et al also mentions for MOA "Missing values at high latitudes are imputed using the average north of 30° N for the Northern

Hemisphere and south of 30° S for the Southern Hemisphere." their line 112. What does this imply for the setup used here? How many values are missing within the studied domain and temporal range?

L73 "these three are important contributors to INPs" needs proper citation for each species

L84 Section 2.2 This section could use some more clarifications, a table listing the included aerosol species, its emission parameterisation, and its INP parameterisation would be good. Especially mention within which temperatures the parameterisations are active (to understand Fig. 3). Additionally, but not required, would be a figure showing the INP parameterisations and their dependency with temperature. Furthermore, splitting the aerosol emission and INP parameterisation sections might improve readability.
Bit more information on the included species is also required here. For example what are the assumed size bins for dust and sea salt?

L91 Perhaps mention again here that the land is actively simulated and that this is not parameterised. Could the authors mention how often the feedback from the land model is performed, i.e. how often is there a change in emission due to more exposed land areas?

L93 Sea spray emissions instead of sea salt?

L99 Every time the authors mention the aerosols included the species change. There has not been any mention of BC before. Make sure the correct ones are listed or specify that black carbon is not included in this study (L42, L62, L72, L85, and probably more)

L103 The usage of DeMott 2015 for dust might need to be justified. Why not use newer parameterisations? And specify whether this is assuming the dust is mineral dust (in comparison to fertile soil dust which is quite a debated topic currently?), also how much difference does this produce? Has anyone studied the impact? (**SC4**)

L118 What does this mean for the simulations? Does the SST and sea ice cover not change with time? Perhaps explain this a bit more in detail

L128 "the Base case applied the Diehl and Mitra (2015) parameterization for bacterial INPs, while the LowBac case adopted the Hummel et al. (2018) parameterization, which assumes lower INA for bacteria". Not needed, simply refer to the table (comment on L84).

L132 Black carbon? Reading the rest of the paper it does not look like this is included? If it is then the INP parameterisation for BC has not been reported. A brief discussion on the inclusion of this species for evaluation in the mixed-phase range would be beneficial, as other studies claim it has a small impact. Justify its inclusion perhaps by stating these other studies. Also, in Kawai submitted work seems the BC has no impact? Perhaps comment on this already in Sect. 2.2.

L133 "temperatures" As in the ambient temperature during collection or the activation temperature range of the measurements?

Results
Section 3.1 I would be interested in seeing the results from MOSAiC included here as the authors are exploring high latitude stations, Creamean et al 2022
https://www.nature.com/articles/s41467-022-31182-x and Barry et al 2025

https://doi.org/10.5194/acp-25-11919-2025 are both exploring this data in detail. Granted the full year of 2020 was included in the simulation?

L136 Source attribution would be interesting to see here, what INPs in the model are dominant for each location? Similar to Kawai Fig. 1 e-h. It cannot be compared to observations but would be an interesting first-order estimate of what seems to be important for each location. Especially the non-Arctic vs Arctic INPs would be interesting to see.

L143 It would be useful to state when these measurements took place (season), to understand results for Norway and Alaska

L144 What does the INP spectrum look like without dust? How much would one need to tune down the dust contribution to have comparable results to the observations? Would be useful to define a scaling factor.

Figure 1 Caption Please see Technical Corrections for updates to the plot.
"mean" Mean or geometric mean? Geometric mean would make a lot more sense for data that span orders of magnitude.
"interannual variabilty" Why do only some have error bars? I do not believe Alaska for example has no interannual variability. Also, what is the reported error bar in the observations, most of these stations do not several years of data? The uncertainty in INP concentration from observations should be reported consistently, why does Alert not have any uncertainty?

L162 Perhaps for some people these values mean something, for me it is hard to put this in perspective. Could these values be compared with global means perhaps? Or compared to other studies (see **L178** for MOA)

L163 When referring to all these seas, perhaps the regions could be specified in S1 to make sure people can identify which regions are where.

L167 "Aerosols in these regions consequently act as INPs and contribute to ice crystal formation in clouds." is unnecessary here.

L168 Again, for the vertically mean (would geometric mean make more sense?) and INP concentration could the authors put these values in perspective?

Fig 2 caption How is the mean calculated? Day-count? Monthly means? Again arithmetic mean or geometric?

L178 "Notably, Arctic MOA is present year-round" Can the authors elaborate on why this is the case? Leon-Marcos et al. 2025 (https://acp.copernicus.org/articles/26/1109/2026/) modelled PMOA in the Arctic and find that the PMOA peaks around May-September. This would also be a good paper to compare the total MOA emissions with. I assume the seasonality shown here is due to the seasonality of the TOC for the MOA parameterisation used by Wilson et al. 2015? Is the MOA during winter mostly located at lower latitudes, close to the cut off at 60°N?

L188 Yes, but this is not seen in the figures? → see **SC2**.

L196-199 While not untrue, this is nothing new. There is no importance to highlight here as these are not evaluated against observations. Rather, I think the important outcome of this part is that we have a first-order estimate of seasonal, spatial, and vertical distribution of INPs in the Arctic. Furthermore, "associated INPs", INP parameterisations perhaps? or what are the authors wanting to convey here?

Section 3.3

I find Figure 4 quite peculiar. So, the Arctic INPs seem to dominate over the areas of local emission in the Arctic (comparing Fig. 2 and Fig. 4). Dust across the Arctic islands and bio across the forests and MOA also mostly where it is emitted. But the local sources in the Arctic seem to only contribute to ~50% of the INPs at 90°N. Does this mean that the local sources are not transported very far? This could be quite interesting to add some discussion on, as INP measurements during the MOSAiC campaign (Creamean et al. 2022 https://www.nature.com/articles/s41467-022-31182-x) see a strong seasonal cycle in local vs long-range transported INPs.

L203 This is annual means? Would be also interesting to see this split into seasonal contributions, especially as the bioaerosols are defined by this cycle. Furthermore, could the authors partition the contributions from the non-Arctic species as well? It would be interesting to see for example how much of the dust INPs are from local sources and the same for the other 2 INP types

Fig. 5 → SC2

L224 This has been done before with other models/methods. Could the authors give some comparisons to previous studies? For example Rantanen et al. 2022 https://www.nature.com/articles/s43247-022-00498-3, would be good to compare the results presented here such that they show a similar trend to the observational datasets.

L230-231 Same for this line, make sure to include some appropriate references

L254 It looks like maybe the reductions in Arctic dust and MOA are also connected to positive trends in sea ice/snow cover? Why are these trends sporadically seen?

L258 Also refer to Fig. 7 g-i here

L265-270 Would be good to mention the reason here (the increase in temperature) to explain the results better. Ok, it is mentioned in L284 but again with the "decreasing INA" explanation. Unless there is a good reason to rename the temperature effect please stick with this established key phrase. → **SC1**

L268 How does a strong aerosol emission influence the activation of INPs? While there are more (number) this should not have any impact on the INP concentration if it is too warm. Are there some aerosol dynamics in play that leads to larger sizes which more readily freeze at warmer temperatures? Please expand on this

L307 How does the contribution of Arctic INPs increase with a decrease in bioaerosols? Perhaps I'm missing something?

Conclusions

Overall: There are no references in the conclusion, try to connect the results to other studies of Arctic INPs from the observational and modelling communities as well as comparisons to global studies to put the work in perspective

L330 Specify the model

Fig. 9 I am not a fan of introducing figures in the conclusions but as this seems to be an overview figure, alright. Could the authors at least state something like “Overview of results” or similar in the caption?

L363 I think this a bit of an overstatement as the authors are only exploring different parameterisations for bacteria. What about differences for dust? And MOA? → **SC4**

Data availability

These days data, when possible, should be made available. I can understand if the data is rather large but could perhaps a subset be made public, perhaps the model data shown in Fig. 1 which could be useful for future studies to compare with?

Technical Corrections

Fig 1 Please make the markers larger and add the error bar caps for better visibility. Add a legend for the colours in at least one of the plots. Make the marker for observations different to the marker for the model for better clarity (black/white print outs for example).
Additionally, these colours are not colourblind friendly, please make sure they are:
https://www.color-blindness.com/coblis-color-blindness-simulator/

Fig 3 Use only one y-label for the row, this would save space. For d-f the same colourbar could be used to save the reader having to identify this themselves.

Fig 6 a) Make the temperature colourbar from °0C to +5°C perhaps? Nuances could be more easily seen