REPLY TO REVIEWER 1:

The manuscript, "The influence of multiple groups of biological ice nucleating particles on microphysical properties of mixed-phase clouds observed during MC3E " by Patade, et al., explores the impact of primary biological aerosol particles (PBAP) acting as ice nucleating particles (INPs) on the properties of the microphysical processes (including, ice formation, cloud droplet, precipitation) of the mixed-phase clouds. They used for that a mesoscale model called AC and they implemented an empirical parametrization for ice nucleation that can distinguish between the different types of PBAP and has been derived based on data collected during a measurement campaign in the Amazonian area. They simulated a mixed-phase case study in the USA during MC3E campaign and they compared the simulated output with these observations. They ran several sensitivity studies where the initial concentrations of PBAP are changed or some SIP mechanisms were turned on and off to investigate the impact on the cloud and precipitation formation.

In general, this topic is important. Moreover, these types of modelling studies are interesting and potentially quite useful for the atmospheric and aerosol-climate community who wish to model heterogeneous ice nucleation by PBAP INPs and evaluate their competition with secondary ice production to eventually reduce the large uncertainty in aerosol-cloud interactions processes. However, with my full review of the manuscript, I have several major comments, questions, and suggestions that I feel the authors need to address before I recommend the manuscript for final publication in ACP.

Reply: We thank the reviewer for his valuable suggestion which helped us in improving the manuscript. We have addressed the comments carefully and have made changes in the manuscript accordingly.

General comments/questions:

The PT21 used here was derived from real measurements in the Amazonian area (mainly forest type of land surface, maybe mixed with some wetlands). In lines 366- 369, it was stated literally that “PT21's observations were used to calculate the relative contribution of various PBAP groups to insoluble organics. The parameters for the shape of PSD of each PBAP group (modal mean diameters, standard deviation ratios, and relative numbers in various modes) are prescribed based on observations from Amazon (PT21)”. Having that said, how can the authors justify the use of such a parameterization on a different type of land surface/case study, where the aerosol load are/might be different and accordingly their prescribed parameters for the shape of PSD of each PBAP group could also be different?

Reply: We have used the PBAP parameters for ice nucleation activity and **relative** abundance of the five PBAP types based on the observations from Amazon as there are no PBAP observations available over a given geographical location.

However, the **absolute** number concentration of PBAPs is constrained to agree with their typical concentration in the atmosphere based on the coincident measurement of organics from the MC3E field (IMPROVE) campaign.

We have mentioned this in the revised manuscript.

Can the authors recommend using such a parametrization in global models as well to represent also the PBAP from different types of land surface and marine areas? If yes, are there any requirements or limitations that one needs to take into account before using/implementing it?

Reply: Yes, we do recommend using the scheme and there are no limitations on its applicability. The scheme is **universally applicable** to all environments globally because it takes as its input the concentration of each type of PBAPs (bacteria, fungal). This imposes a requirement for the user to somehow specify those input concentrations and of course, these concentrations vary widely globally and through time. In PT21 there is an implementation section that gives more information about using the formulation. The PT21 default observations of PBAP size distribution are recommended when there is a lack of observations over the given location.

Currently, the observations of PBAP over different geographical locations (including the region where we carried out the simulation) are rare, which prevents us from using the region-specific PBAP observations for the present study. Hence, we follow the PT21 method noted above.

In the revised manuscript we have added some analysis in which the model estimated values of one of the major PBAP bacteria are compared with the observations. The bacterial number concentration from AC is based on the input mass fraction of bacteria. Our analysis showed that the estimated values of bacterial number concentration are overall in fair agreement with observations. Which showed that AC was able to simulate the bacterial concentration based on the assumption made for its size distribution and relative abundance in total PBAP. In addition, simulated concentrations of Fungi (~10^3 m^{-3}) and bacteria (10^4 m^{-3}) are in agreement with their typical concentration in the air given in Despres et al. 2012.

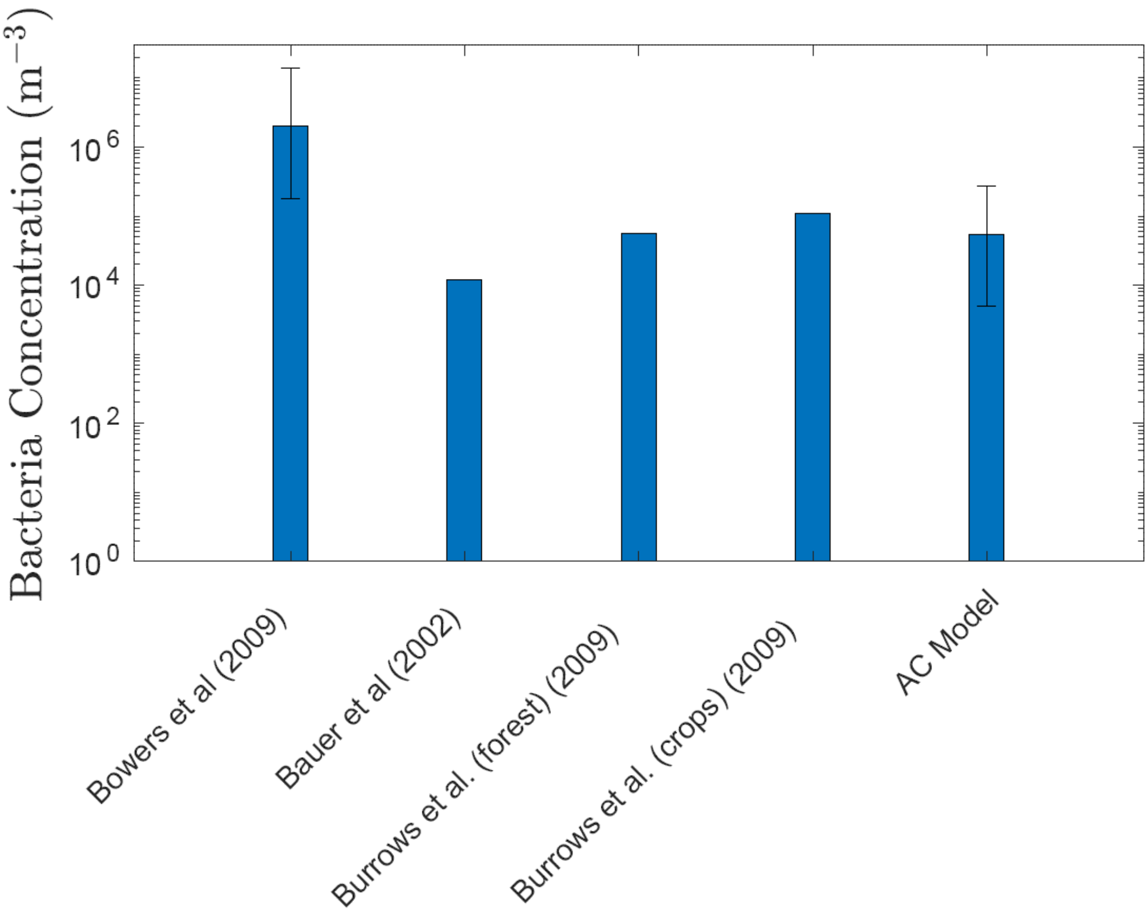

Figure : The comparison of model estimated bacterial number concentration with various observations.

Throughout the whole manuscript and when discussing the results, it was difficult to follow up on the calculated percentage values of the changes of some prognostics and diagnostics that were shown in the figures, but as vertical profiles. Therefore, it can be great if the authors provide at the end of the manuscript a table that summarizes the vertically summed domain averaged values of the model diagnostics (including, LWC, ice concentration, precipitation (convective, stratiform, and total), short and long-wave radiation/flux, and cloud fraction) for the different simulations and compare them with observation if relevant (or possible) or with other studies that were mentioned in the text even for a different region-domains/case studies. Such a table can provide an overall overview of the whole story of this manuscript that can support the conclusion of PBAP relevance to ice formation, and make it easy for the reader to estimate the changes and their corresponding percentages from one simulation/scenario to another as well as improve the overall quality of this manuscript.

Reply: We thank the reviewer for his valuable suggestion. In the revised manuscript we have added a new Table (**Table 4**) that includes averaged values of the model diagnostics for different simulations for the convective, and stratiform regions as well as for the whole storm.

We could not include the corresponding observations in the table as those are available only for the specific levels.

We have compared these averaged values with other studies in the discussion section.

The author can choose to provide one table or two tables (one where they vertically sum the values from the surface to the level of -35 C, where heterogeneous freezing is relevant and another one for full vertical summed values including the homogeneous freezing).

Reply: We have added a separate table that includes ice number concertation averaged from surface to the level of -35^{o}C (**See Table S6 and Table 4**).

Due to the high number of simulations, it was not clear if some of the ice processes were turned on or off in each simulation especially since it was sometimes confusing which one is turned on or off throughout the text (Fx, Line 782 - 783, where the authors suddenly mentioned that the SIP was turned off in the control simulation after the impression that it was on from the beginning). Therefore, another important table is necessary for this manuscript that shows the different configurations of each simulation in this manuscript. Fx, the table summarizes the 1- different types of PBAP or other INPs that were considered in each simulation, 2- simulation names, 3- simulation configurations, 4- the cloud ice process which was turned on/off, 5- the initial and the increased concentrations of different INPs considered for each simulation 6- as well as the corresponding figures numbers that are resulted from each simulation.

Reply: We are thankful to the reviewer for the suggestion. Regarding line 782-783: We wanted to say that in the 'no-sublimation breakup' simulation the SIP mechanisms of sublimation breakup and in the 'no-collisional ice-ice breakup' simulation the SIP through a breakup in ice-ice collisions were turned off.

According to the reviewer's suggestion in the revised manuscript, we have added a table (Table 3) that includes all the information about each simulation.

Vertical profiles are important, but spatial distribution (i.e maps) of the diagnostics (vertically summed or averaged) to show the spatial variability for the different simulation/scenarios could improve the quality of the manuscript

Reply: We thank the reviewer for his suggestion. We have added a few spatial plots in the revised manuscript (e.g. Figures S5 S7).

The manuscript does not provide any supplementary material that could be useful (I have added further some suggestion on how could it be)

Reply: We thank the reviewer for his suggestion. We have added the supplementary material in the revised manuscript.

Modelling uncertainties and the term “good agreement”: This manuscript always addressed the uncertainties of other work/observations and never mentioned the uncertainties that could result from running any type of atmospheric model including AC (it’s not an exception). This in return led the authors to state “good agreement in many places throughout the manuscript”, where I would be careful to say “good agreement” but rather use “acceptable agreement”, given that both models and observations have/should have uncertainties. Since the authors addressed one of them, then it’s fair to estimate and address the model range of uncertainty. This is important to avoid giving the readers the impression that only observations have uncertainty and model never have that and they are always perfect, although it’s not mentioned explicitly.

Reply: We thank the reviewer for his comment. We agree that AC is not perfect in simulating the cloud microphysical properties of the squall line case considered here. In the revised manuscript we have revised the result section accordingly and avoided the word “ good agreement” when there is bias in model simulated cloud parameters. We also have mentioned that the model is not perfect in simulating cloud properties (see line number 601-602.)

Major comments/questions in detail:

Lines 199-200 with Figure 1: Where the value 2355 J kg^{-1} can be read from the figure? And why it’s relevant to be mentioned here? The description of Figure 1, b in those lines does not correspond to what one can get from this part of the figure?

Reply: The value of CAPE mentioned here is associated with the shaded region (in the revised plot) between moist adiabat (dotted red line) and the temperature line (think black line). The purpose of the plot is to show the vertical profile of the atmosphere before the formation of deep convection. The additional information about the Skew-T plot is added in the revised manuscript.

Lines 201-202 with Figure 1: Same as the previous comment. At this level (840 hPa) neither the air temperature nor the dew point temperature was 14 C as stated in the text. Which line in Figure 1, b shows that the temperature at that LCL (840 hPa) was 14?

Reply: We have revised this plot and associated description.
The LCL is specifically mentioned in the revised plot.

Please show the right figure that corresponds to the text or change the text according to what can the reader sees from the figure.

Reply: We have made the changes in the manuscript accordingly.

Lines 202-203: What is the estimated amount of vapor in the entire depth of the troposphere? It seems that this sentence is not complete and this piece of info is missing in the text. Either remove the whole sentence if it's not used further on in the text or type the right value and consider fixing the sentence.

Reply: We have removed this sentence in the revised manuscript.

Lines 199-203: This part needs to be revised/rewritten.

Reply: We have revised this part in the revision.

In figure 1, the resolution of figure 1, b should be improved. It was hard to read and extract information from it with its current resolution and check the values stated in the text. Please consider providing a better version with a higher resolution/readable Skew- T plot.

Reply: We have revised this plot and it's now clearer with better resolution. The region associated with CAPE is shown by the shaded region. The LCL is shown explicitly in the plot. The values of various parameters are checked and corrected.

Figure1, c: Would it be possible to provide the uncertainty in the modeled line as well similar to the observation as the predicted/simulated CCN concentration is deviated from observation? Add some text as well in the manuscript to describe this part of the Figure.

Reply: The uncertainties in the model simulated CCN concentration can arise due to the variations in the mass mixing ratios of various aerosol species that serve as CCN in AC. The coincident observations of aerosol mass and cloud properties are not available during the MC3E campaign. The IMPROVE observations of aerosol mass were available on May 18 and 21 which indicated a 20-30% variation in aerosol mass.

We have added error bars on the AC simulated CCN concentration based on these variations in the input aerosol mass (**Figure 2**).

Lines 271-282: I would guess that those aerosol measurements from IMPROVE were not on the same dates as the MC3E campaign as they seem to be a separate data-set. But can the authors give more info about those measurements (dates, some time series of the measured species especially for PBAP), and more importantly the scaled profiles of aerosol mass concentration that matched actual measurements, which were mentioned in lines 280-281? Those can be added to the supplementary material and referenced here in the manuscript.

Answer: Yes, we agree with the reviewer that the IMPROVE measurements were not on the same dates as the MC3E campaign. The measurements which we used in the simulated case of the MC3E (May 20) were carried out on May 18 and May 21. In the revised manuscript we have added more information about these measurements. The scaled vertical profiles of mass mixing ratios of various species including dust, sulfate, black carbon, sea salt, and total PBAP are also added to the supplementary material (**Figure S2**).

It should be noted that there no direct measurements of PBAPs were carried out during IMPROVE. It was derived from the measured mass of total organic carbon and is described in the text (line numbers 417-430).

Lines 307-310: How those processes are different in terms of temperature? It's better to add (in parentheses) the range of T where each of those SIP mechanisms is more efficient/relevant.

Answer: Some of the SIP mechanisms are strongly active over a particular range. For example, the Hallet Mossop process is highly active between -3 to -18^{o}C. The collisional ice-ice breakup is mostly active between -10 to -15^{o}C. The rate of formation of secondary ice through raindrop freezing is highest at around -15^{o}C. The sublimation breakup is highly active over a variable temperature range of 0 to -18^{o}C, and is a strong function of relative humidity over ice, and the initial size of parent particles.

We have added the temperature ranges at which the SIP mechanism was more active (Line 317-323).

Line 344: As the parametrization PT21 has been used in this study, I suggest adding & present the formulation of PT21 with its used/corresponding parameters here in this manuscript at the end of this section 3.2.

Reply: We thank the reviewer for his suggestion.

We have added a summary of the formulation of PT21 at the end of section 3.2.

Line 347: Where those initial and evolving boundary data for meteorological conditions were taken from? Did the authors use any other climate/regional model to derive them? Or they were taken from observations? If any, this should be mentioned more clearly. Better show some meteorological conditions plots for the simulations in the supplementary material.

Reply: The initial and boundary data for meteorological conditions were taken from observation from the radiosonde network and we did not use any climate/regional model. The large-scale meteorological conditions used for the model simulation were derived using the constrained objective variational analysis method that is well described in Xie et al. (2014). Based on this method, the so-called large-scale forcing including large-scale vertical velocity and advective tendencies of temperature and moisture were derived from the sounding measurements network. During the MC3E campaign, the sounding network consists of five sounding stations centered on a sixth site at the ARM SGP central facility (CF). An area with a diameter of approximately 300 km was covered by this sounding network covers. The details about the sounding data are described in section 2.3. Also, the time height plots of the potential temperature and water vapor mixing ratio from the large-scale forcing data are shown for the simulated case.

In the revised manuscript we have added more information about the large-scale forcing data (Lines 396-409).

Line 354: what was the spin-up time for the conducted simulations in this study?

Reply: The initial 6 hours were considered as the model spin-up time.

Line 371: It would be a good idea to plot an example of those aerosol initial/prescribed profiles together with the predicted aerosol size distribution from AC, especially for PBAP (can be added to the supplementary).

Reply: We have added the initial vertical profiles of various PBAP groups (**Supplementary Figure S2**).

We have also added the size distribution PBAPs (**Supplementary Figure S3**).

Line 374: Now I see the text that explains Figure 1c. either move the text further up or consider moving this part of the figure and separate it from Figure1 and move it here as it can stand alone or join it with the suggested other plots from a previous comment in the supplementary (see the above-mentioned comment on Line 347). The whole of figure 1 can go to the supplementary.

Reply: We have revised Figure 1 according to the reviewer's suggestion. The skew-T plot and vertical profile mixing ratio are shown separately in **Figure 1**. The CCN spectra from the model and observations are shown in **Figure 2**.

Line 379: What was the uncertainty range of the modeled CCN concentration? Consider adding it to Figure 1c.

Reply: The uncertainties in the model simulated CCN concentration is between **20-30%** and are shown in the plot as error bars.

Line 381: Since the uncertainty of the simulated line from the AC model is not provided, I don't agree that they are in a "good agreement", but rather in an "acceptable agreement"

Reply: We agree. We have revised this statement in the revised manuscript.

Line 385: was the SIP and homogeneous freezing turned on or off in the control simulation? Was there any other type of INPs activity (i.e., dust) considered in the control simulation in the new version of AC? Better mention those explicitly if they are turned on by default after adding the PT21.

Reply: The control simulation considered in the study includes all the SIP mechanisms mentioned in section 3.1 as well as homogeneous freezing. These processes were never turned off in the control simulation. The control simulation includes INPs from various sources including dust, black carbon, and various PBAP groups. These INPs sources were never changed for the control simulation.

The SIP and INP sources were changed only in the sensitivity studies and are mentioned specifically in the newly added Table 3.

Line 385: For validation reasons, why the authors did not compare their PT21 with another parametrization for this case study, fx, the older version of PT21?

Reply: We thank the reviewer for his suggestion. The comparison of the new Empirical parameterization for bioaerosols with the old scheme from Phillips et al. 2013 was carried out using a parcel model in our earlier study PT21 (Figure 9). Therefore, considering the limitation on the length of the manuscript we have not repeated the analysis here.

We have attached the figure here for reference.

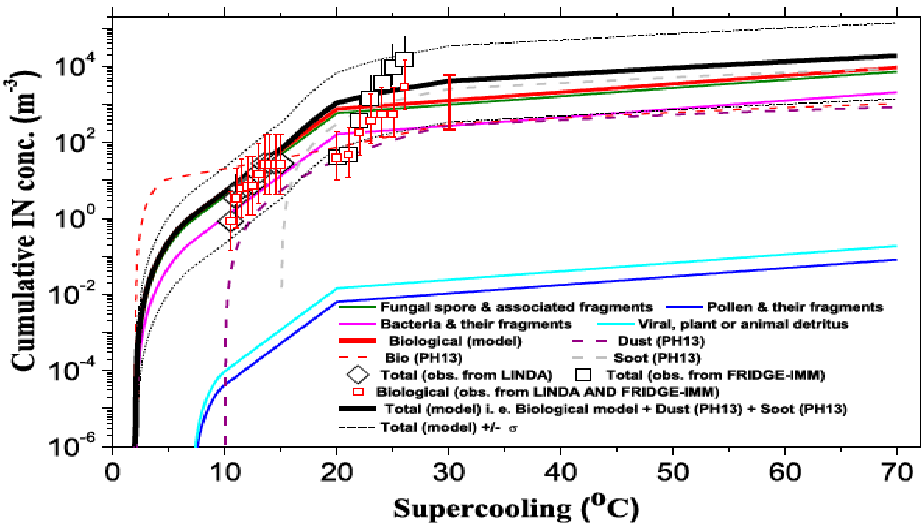

FIG. 9. The number concentration of biological ice nuclei predicted by empirical parameterization as a function of supercooling in a water-saturated air parcel. The number concentrations of dust, soot, and biological ice nuclei predicted by the empirical ice nucleation scheme by Phillips et al. (2013) (mentioned as PH13 in the plot) are also shown for comparison. The error bars of observed IN concentrations indicates the standard deviations estimated based on the uncertainties in the measurements. An error bar in thick red on the biological (model) line indicated uncertainties in the estimated biological IN at a temperature colder than −15°C.

Line 397: What does TWC refer to in the Figure 2 caption?

Reply: It refers to the total water content (LWC + IWC). It is mentioned in the figure caption now.

Line 435-436: Figure 3a How authors can read agreement from that subfigure? does not show an adequate agreement between observation and simulated averages of LWC as the authors wrote in the text. It's clear that the simulated means are deviated by nearly one order of magnitude from the observed means in the convection case below 0 C. Maybe, one can see some sort of agreement in the stratiform case.

Reply: We agree with the reviewer. We have changed the statement and mentioned that the model simulated values of LWC are in acceptable agreement with observations.

21- Line 438-444: same as in the previous comment. What about the points in Figure 3c at T around 15C? Here, the simulated domain averaged points deviate from the observed mean values by ~ factor of 2. Again, this difference needs to be stated clearly in the text and then evaluate overall and say that there is an overall agreement. I would rather use the term "an overall acceptable agreement" rather than "a good agreement" here. I would also suggest adding a calculation of the percentage range of that agreement (do the same for the above comment in Fig 3 a,b)

We have mentioned these differences in the text. Also, we have modified the text by changing "a good agreement" to "a fair agreement".

22- Lines 470-475: How the authors can justify that two-thirds of an order of magnitude bias between observation and modeling could be better than half of an order of magnitude bias?

Reply: We have modified the text.

23- Line 472: Was the underestimation of "measured" or "simulated" ice number concentrations? Because the author said "also" at the start of this sentence.

Reply: We wanted to say the underestimation of simulated ice crystal concentration.

We have corrected this in the revised manuscript.

24- Line 497: from Figure 4c the bias in reflectivity between 3 – 8 km is higher than 8 dBZ, Look at the first 5-6 points. The bias at those points is at least 10 as shown in the figure. So fix that range in the text.

Reply: We agree. We have made the changes in the text accordingly.

25- Figures with the vertical profiles: Since not only observations have uncertainty, but also modeling output especially since the points are mean values, it's a good idea to add the error bars (uncertainty) to the simulated mean values of the diagnostics that are shown in these figures similar to the observations.

Reply: We thank the reviewer for his valuable suggestion. In the revised manuscript we have added error bars on the simulated vertical profiles in the most important simulations involving changes in PBAP.

These error bars are based on the five Ensemble runs that were carried out for the given simulation.

26- Lines 510-511: Again, the term "good agreement" does not fit well here. Same as previous comments. Here, the authors stated clearly the 1-2 hours delayed simulated peak of the precipitation and justified that by the uncertainty of the initial and boundary conditions of the 3-D model, and then they wrote that there is a "good" agreement between the observation and the modeling results. What criteria do the authors define for "good agreement"? Please consider fixing the term "good" here and revise throughout the whole text and use it only when it's relevant.

Reply: We agree with the reviewer. We have revised the statement accordingly.

27- Line 521: Where the total was estimated? was the 0.3 L^{-1} calculated by the model or detected by other observation or modeling studies? If it's calculated by AC, then add the total to figure 5. If it's from other studies, then consider adding a reference?

Reply: The PBAP INPs and total INPs mentioned here from the AC model. We have added the total INP from AC in Figure 5 (**now Figure 6**).
We have also made the changes in the text accordingly.

28- Section 4.3 and figure 5: How the fraction of the number concentration of PBAP INPs was calculated from the scaled profiles of mass mixing ratio that were used as input fed into the model? As the authors mentioned earlier in lines 271-280, they used some observations to estimate those aerosol profiles (Table 2). But they never mentioned or explained how the model calculated the fraction of PBAP INPs from the input mass mixing ratio?

Reply: The fraction of PBAP INP was calculated based on their mass fraction in total PBAP mass. It should be noted the observation of insoluble organics from IMPROVE was used to estimate the total mass of PBAPs as there are no observations of PBAPs available over the geographical location considered in this study. The mass fraction of each PBAP group in total PBAP mass is prescribed based on the PT21 observations. The fraction of mass mixing ratio for various PBAP groups is: FNG= 0.39, BCT= 0.13; PLN= 0.31; DTS= 0.17; ALG= 2.5×10^{-4}.

how was the size distribution of the PBAP in this domain (consider providing these to the supplementary)?

Reply: We have shown the size distribution of PBAP in this domain in supplementary **Figure S3.**

Was there any other assumptions to calculate the PBAP INPs fraction (i.e., the mass of the bacterial cell, fungal spore cell, or their particle densities)

Reply: No assumptions were made to calculate the PBAP INP fraction. It was based on observation by PT21,

The PBAP density assumed was assumed to be 1360 kg/m^3.

29- Figure 6-a: Was primary ice (PRIM) referred to by the first blue bar for all INPs considered in the AC model or only for PBAP?

Reply: The primary ice shown by the blue bar is from all INPs considered in the AC model.

30- Line 552: Use the same name/short name that is used in Figure 6-a for consistency. The authors used HOM for the homogeneous drop freezing and PRIM for the primary heterogeneous ice freezing. Better to name them primary Heterogeneous freezing and primary homogeneous drop freezing.

Reply: We have made the changes in the plot and text accordingly.

31- Lines 565-567: Make sure of the estimations of those percentages and if the authors can calculate them then it's better to show them in a subfigure versus T?

Reply: In the revised manuscript we have shown their relative contribution to various processes as a function of temperature.

In detail, if ice-ice (blue dots and line in figure 6-b and c) contributes 50% and sublimation (green) contribute 7% as stated in the text then the rest (red for active INPs, brown and pink for the other SIP) should contribute 43% to complement the 100% for the total (black) at T = -25 C. Let's assume that each of the rest contributes similarly as sublimation (green) does (7%) although they show a much lower contribution from figure 6 b and c, then they will sum up to 21% and not 43%. Is there some other source missing in this figure that could complement 100%? Or it's just the wrong estimation of the percentages? The same goes for the stratiform case.

Reply: We apologize for the confusion. We have revised this figure (**now Figure 7**) and text accordingly. For better understanding, we now show the relative contributions from various processes as a function of temperature.

Therefore alternatively, I would suggest adding subfigures (6 d and e) where the authors display the percentage contributions of each process to the total ice number concentration (x-axis) vs T (y-axis)? Add the homogeneous contribution as well as figure 6-a shows the largest contribution to the total ice, especially at low T than -35 C.

Reply: We have changed the plot (**now Figure 7**) and we now only show the relative contribution of various processes in determining the total ice number concentration.

32- Lines 573-575: This may create uncertainty in estimating the HM contribution to total ice. Can the author give an estimate for this uncertainty?

Reply: It should be noted that in the AC model HM process is treated with a factor multiplying the fragment emission rate (350 splinters at -5°C per milligram of rime) which depends on the cloud droplet size. This factor is zero for cloud diameter below 16 µm and unity above 24 µm with linearly interpolated in between. Thus, the HM process is not fully absent for cloud diameters less than 24 µm. The vertical profile of cloud drop diameter from the AC model shown in Figure 4f is the average profile and therefore the cloud region with a drop diameter greater than 16 µm is not visible.

We have added this description in the revised manuscript (Line 700-709)

33- Section 5.1: Did the authors consider dust or any other type of INP than PBAP in those simulations including the control one? If yes, what was the ice nucleation parameterization for dust used in these simulations in AC?

Reply: The AC model described in the study includes IN activity of the dust, black carbon, and five PBAP groups. The parameterization for dust as well as black carbon is based on Phillips et al. 2008 and 2013. This parametrization has empirically derived dependencies on the chemistry and surface area of the INPs.

This information is added to the revised manuscript (Line 331-333).

34- Lines 615-616: Please consider revising/rewriting this sentence. Are the authors comparing convective to stratiform here? Less than 50 % of what? In which case, stratiform or concoctive or both?

Reply: We have revised this part of the manuscript.

35- Lines 644-646: Interesting and maybe unexpected result here, however, the justification is NOT relevant here.

Reply: We have modified this part of the manuscript in the revision.

To my knowledge, homogeneous freezing works at T lower than ~ -36 C, so it should be insensitive and inactive at -20 C. Only Heterogeneous and SIP can contribute here at this level (range of T)?

Reply: Yes. In AC, the homogeneous freezing is active at temperatures colder than -36^{o}C as is realistic. The number concentration of ice particles shown from homogeneous freezing shown here is based on an advective tagging tracer explicitly dedicated to tracking the ice formed through homogeneous freezing. The ice formed as a result of homogeneous freezing at temperatures colder than -36^{o}C can be advected to the lower level and affect the ice number concentration at levels with temperatures warmer than -36^{o}C.

To avoid confusion we have modified the text to clarify.

This leads to a couple of questions; how did the authors define homogeneous freezing in AC? Which parameterization is used for homogeneous freezing? What is the range of T for homogeneous freezing in AC?

Reply: There are two types of homogeneous freezing represented: that of cloud droplets near -36°C and that of solute aerosols at colder temperatures. Both schemes are described by Phillips et al. (2007, 2009). For cloud droplets, a look-up table from simulations with a spectral bin microphysics parcel model treats the fraction of all cloud droplets that evaporate without freezing near -36°C, depending on the ascent and initial droplet concentration and supersaturation. The size dependence of the temperature of homogeneous freezing is represented.

Homogeneous aerosol freezing is treated with lookup tables following Koop et al (2000) with dependencies of the critical relative humidity on temperature and aerosol dry size.

The number concentration of ice particles shown from homogeneous freezing shown in Figure 9e is based on an advective tagging tracer explicitly dedicated to tracking the ice formed through homogeneous freezing. The ice formed as a result of homogeneous freezing at temperatures colder than -36°C can be advected to the lower level and affect the ice number concentration at levels with temperatures warmer than -36°C.

We have added information about homogeneous nucleation to the text (line numbers 335-341).

36- Lines 647-650: Can you explain more clearly why this is happening?

Reply: We have added text to explain that it is the same trend with respect to PBAPs altering homogeneous freezing.

37- In Figures 4 a and b, the AC model with its parameterizations was underestimating the observed ice concentration already.

Reply: Only in Figure 4b for the stratiform cloud between -10 and -20°C is there any significant underestimation of the ice concentration in the simulation. This is in view of plotted error bars.

It was not clear if SIP was turned on or off in the control simulation to explain this deviation.

Reply: The SIPs were never turned off in the control simulation as shown in Table 3. This is now clarified with new text at the top of the model validation section **(line 536-537).**

Assuming that homogeneous freezing was inactive at -20 C, can the authors clarify more properly why adding more PBAP could result in less ice concentration at levels higher than -20 C where most PBAP is relevant for ice nucleation?

Reply: It should be noted that even though the homogeneous nucleation is not active at a temperature higher than -20^{o}C, it can affect the ice number concentration at those levels due to downwelling from upper levels. This is clear from the advective tracking tracer for homogeneous nucleation as shown in Figure 9e. The very high pbap case clearly shows the decrease in ice number concentration between -10 to -35^{o}C originating from homogeneous freezing above those levels.

Since homogeneous nucleation with downwelling makes a significant contribution to the total ice number concentration in mixed-phase regions, the overall effect of the very high-pbap case is to decrease in ice number concentration.

It is crucial to view any convective cloud in terms of many deep vertical motions with a broad continuum of values of ascent and descent. One cannot neglect convective descent for the microphysical properties at any level.

38- Lines 640-650: Does AC take into account the dissolving factor/fraction of each type of aerosol considered in AC? If yes what was the assumed value for each type of PBAP?

Reply: AC does take into account the soluble fraction, by mass, of each type of solid aerosol particle. The values of this fraction are 0.15 for dust, and 0.8 for carbonaceous species (Phillips et al. 2009). The soluble mass fraction for all PBAP groups is 0.1.

This information is added to the main text (lines 428-430).

39- Figures 4, 7, and 9: Why the ice concentration from the control run in Figures 4 a,b is different from Figures 7 d and 9 d, especially at T lower than -20 C? Should not be the same control simulation/run in 4, 7, and 9?

Reply: In figure 4 we have validated the simulated cloud properties with the aircraft observation. The observed number concentration of ice particle particles smaller than 200 μm is prone to shattering, even with the use of the shattering correction algorithm. This can introduce a significant bias in the observed ice number concentration. To avoid these biases, we have compared the number concentration of ice particles with a diameter greater than 200 µm from both observation and model. However, in the rest of the manuscript, the number concentration from the model included all ice particles.

This information has been added to the text (Lines 564-572).

I suggest unifying the x-axis range in all the above-mentioned subfigures, especially for ice concentration, and making it from 10^{-3} to 10^{1} cm^{-3}, so it's easier to compare.

Reply: We have changed the axes range accordingly.

Also, add the observation data points from Figures 4 a, b to Figures 7, d, and 9 d as they are also relevant here. Something is missing/ or not explained properly in justifying these results (figure 7 and 9).

Reply: We thank the reviewer for his suggestion but we would like to keep the validation plots separate from the sensitivity tests. The size range of ice particles considered in model validation plots and sensitivity tests is different as mentioned in the above comment. Therefore, we cannot overlay the observed ice number concentration on the model simulated ice particle concentration in the sensitivity tests. We have explained the results from Figures 7 and 9 in a better way.

40- Line 653: Can the AC model distinguish how many ice crystals that are resulted from "downwelling" homogeneous freezing from the other amounts of ice crystals that are resulted from other processes (heterogeneous freezing and different SIPs processes) and aloft up at each level of T, especially at those higher than -20 C, where homogenous freezing should be inactive? if yes, could you please explain shortly how?

Reply: Yes, there are tagging tracers for the components of total ice concentration originating from each of these sources of primary and secondary ice production as well as for homogeneous nucleation.

Note that each tagging tracer is advected and diffused just as for the total concentration so that its plotted average profile reflects the effects from deep upwelling and deep downwelling.

41- Figure 10: Are those diagnostics produced explicitly by AC when all SIP and homogeneous freezing were turned on? Or they were estimated from different simulations where only one process turned on and the other were off?

Reply: The analysis presented here was produced explicitly by AC when all SIP and homogeneous freezing were turned on.

Why the very high bac simulation contributes to more ice budget/concentration (green in 10 a for primary ice condensation-depo cold and warm) than the control, which is contrary to the conclusion that the authors drew from Figures 7 and 9 d where they claimed that the control simulation contributed to more ice concentration?

Reply: It should be noted that Figure 7 (now Figure 8) and 9d shows the changes in total ice number concentration which include primary heterogeneous ice, primary homogeneous ice, as well secondary ice. Figure 10 showed an increase only in primary heterogeneous ice through condensation and contact freezing which has a lower contribution towards total ice number concentration as compared to homogeneous nucleation. The very high-pbap case leads to a decrease in ice formed through homogeneous nucleation with no significant increase in ice formed through SIP. Thus, the overall effect of this simulation is a decrease in ice number concentration.

42- Lines 684-686: Again, same comment as the previous one. It seems somehow that what is shown in Fig 10 a and explained in those lines is contradicting the conclusion drawn from figures 7 and 9 d? Any explanation? Please clarify the explanation and the justification of those results?

Reply: As we said in reply to the previous comment the total ice number concentration shown in figures 7 and 9d included ice particles formed through all possible primary and SIP and homogeneous freezing included in AC. Whereas Figure 10 a showed changes in each component of primary heterogeneous ice nucleation as well as primary homogenous ice nucleation associated with changes in PBAP. The very high pbap case showed an increase in primary heterogeneous ice and a decrease in primary homogenous ice. Since the contribution of primary homogenous ice nucleation is much higher in determining the total ice number concentration when compared with primary homogeneous nucleation, the overall effect of the very high pbap case is a decrease in total ice number concentration.

43- Lines 693-698: this is an important conclusion, but the plot to show that ratio is not shown in the manuscript. I suggest showing the ratio plot between SIP and primary ice without homogenous freezing.

Reply: In the revised manuscript we have included a plot showing the ratio of SIP and primary ice without homogeneous nucleation (**Figures 10c and 10d**)

44- Lines 715-720: I suggest adding a table where the authors summarize the vertically summed and domain averaged values of precipitation, ice concentration, and LWC for the whole period of simulation for the different simulations and scenarios. This can give a better picture of those changes stated in the text.

Reply: We thank the reviewer for his valuable suggestion. We have added a new table in the manuscript that summarizes the changes in precipitation and cloud properties in various sensitivity tests (**Table 4**).

45- Line 730 and figure 12: These short and longwave radiations/fluxes are a result only of cloud-radiation interaction (indirect effect), right? If yes, Can AC also check the direct effect resulting from PBAP-radiation interaction? or did the authors run any simulation to check only the direct effect and how different the values would be from the indirect effect?

Reply: Yes. The short and longwave radiation fluxes are a result of indirect effect only. Currently, AC has a limitation in estimating the direct effect resulting from PBAP-radiation interaction. Therefore, we could not check the direct effect resulting from PBAP-radiation interaction.

46- Lines 756-768 and figure 13: This makes a lot of sense, and that's what one would expect, the concentration of cloud ice will increase by increasing the bacteria or fungal concentration or both. Now, how relevant this change is, is another story that depends on many factors. However, my question is, why this was not the case in Figures 7 and 9 d when PBAP concentration was increased? Any justification? Can one conclude here that there is a competition between the different types of PBAP or INPs when they are all included in the simulation (control), so they prohibit each other efficiency in nucleating ice, whereas this competition is canceled when only one type of PBAP is considered and increased?

Reply: We agree that there is a contrasting trend between the boosting bacterial particles alone (30% more ice number concentration in the high-bct case than in the control) and boosting all PBAPs. However, the changes are small and are not statistically significant because of the spread of each ensemble.

In the revised version, the sensitivity test with only changes in Fungi and Bacteria is removed to make the manuscript more concise. Thus, this part of the result section is also omitted.

47- Lines 781-783: Difficult to recognize which process is turned on or off in each simulation, therefore, hard to follow or assess the rest of the text in this section.

Reply: We have modified this part of the manuscript.

For a better understanding of the processes that are on/off in each simulation, we have added a table that describes all the simulations with active/inactive processes (see Table 3).

48- In lines 782 - 783, NO!!! Are the authors still talking about the same control simulation that has been discussed earlier in the previous sections (which I think it's the case) or it's a different one? If it's the same one, why did the authors wait until here to mention that one or more SIP mechanisms were turned off in the control simulation (this should have been said much earlier)? If this is the case then, What's the point of comparing simulations with no SIP to the control simulation where SIP was turned off as well?? In this context, I would suggest to the authors add a table that summarizes the different simulations' names, types, and different configurations and processes that are turned on/off for each simulation. (See my general comment).

Reply: We apologize for the confusion. In the control run mentioned in the previous sections and throughout the manuscript none of the SIP mechanisms were switched off. There was a typo in the mentioned sentence and is corrected now.

To clarify, we have included a new table (Table 3) which now provides a summary of all the simulations that were carried out. The SIP mechanisms were switched off as a part of sensitivity tests and mentioned explicitly in Table 3.

49- Liners 791-793: Since the authors mentioned this conclusion, why the figure is not shown here to support the text? Consider either adding the subfigure or removing the text.

Reply: We have removed the mentioned text in the revision.

50- Lines 795 until the end of this section: I'm lost, which simulation refers to the inclusion of SIP that the authors are talking about here in the text and Figure 14? Is it the control (that's what I can hardly guess from the legend Figure 14), but this again contradicts what has been said and assumed by authors in lines 782-783?

Reply: As mentioned earlier, the SIP mechanisms were never turned off in the control simulation. There was a typing error in the lines 782-783. In figure 14 we are comparing the simulations with various SIP sensitivity tests with the control simulation in which all SIPs were active. For better clarification, We added a new table (**Table 3**) to the revised manuscript which summarizes the cloud processes that were active/inactive in the mentioned simulation.

51- Line 807: warmer than -25 C as stated in the text or -15 as shown in figure 14 d?

Reply: Correction made

52- Line 809: warmer than -15 C as stated in the text or -25 as shown in figure 14 c?

Reply: Correction made

53- Lines 823-825: Any justification for such a result here, since SIP is known to efficiently enhance precipitation?

Reply: SIP not necessarily can enhance precipitation. Phillips et al. (2017) showed that SIP through ice-ice collision breakup can reduce accumulated surface precipitation in the simulated storm by 20%-40%. The inclusion of SIP can result in numerous fragments of ice particles which can convert some of the mixed-phase clouds to ice only. It results in more snow particles competing for available liquid and they are less prone to growth by riming which results in smaller ice particles. Smaller ice particles can result in a reduction in surface precipitation.

We have added this explanation to the text (Line 911-915)

54- Line 828: I think this section is important and needs some more explanation and justification as it clearly shows the importance of SIP vs PBAP, especially in the convective case at T higher than -30C. For example, why in figure 15b (stratiform), the ice concentration resulting from the control was nearly one order of magnitude higher than the other two simulations with no SIP at T ranging from 0 to ~-13 C, and the opposite happens at lower T than -13C? Adding a sentence or two at the end of this section summarizing the conclusion of this section would be good.

Reply: The average ice concentration above the -13^{o}C levels is dominated by downwelling of homogeneously nucleated ice from above the mixed-phase region in stratiform clouds respectively. Below these levels, SIP prevails. Therefore, including SIP mechanisms increase ice number concentration at temperatures warmer than -13^{o}C in the stratiform region.

We have added this information in the discussion section.

55- Lines 870-872: Figure 4b (stratiform) in the range of T between -10 and -16C the bias was more than a factor of 3 (at least half order of magnitude)

Reply: Correction made

Consider having a closer look at the COMP Obs (pink points) and HVPS Obs (cyan points). This range of bias between simulated ice concentration and observation is almost in the same range as the change resulting from removing SIP (Figure 14, d), where authors eventually concluded that this change is large indicating the importance of SIP in ice production.

Reply: As noted above, the bias in the prediction by the control of ice concentrations is **only** in the stratiform region and between levels of -10 and -16^{o}C and its only by half an order of magnitude at the most. By contrast, the changes in ice number concentration from inclusion of SIP is upto almost one order of magnitude at those levels.

Clearly, the effect from SIP is generally far stronger than any bias compared to aircraft data.

56- Yes, observations may indeed have uncertainties, but all models also have uncertainties. Since the model uncertainties were not shown, so, the AC model can also underestimate the observed ice as clearly shown in Figure 4 in both convection and stratiform.

Reply: We disagree there is any major problem with ice concentration validation against aircraft data. In the convective clouds, there is no statistically significant difference between predicted and observed ice concentration at any levels in view of the error bars.

As noted above, the only significant bias is evident between -10 to -16ºC in the stratiform region. This is less than half an order of magnitude and is clearly mentioned in the text now.

57- Lines 876-878: See the above comment. What about the half order of magnitude difference in the ice concentration and the 1-2 deviation in capturing the precipitation peak taking into account the relatively short time of the simulations (48 hours)?

Reply: We agree with the reviewer. We have made the changes in the text accordingly.

58- Lines 891-893: Yes, it might be that dust and BC concentrations are much higher than PBAP at T -15 C in the chosen domain or at any different domain (also globally), but do the authors think that dust and BC can initiate ice nucleation at this T or higher?

Reply: It should be noted that in the AC model activation of dust and black carbon occurs at temperatures colder than -10 and -15 ºC respectively. However, the number concentration of active INP from dust, BC, and PBAP showed in the paper are based on independent advective tracking tracers. Therefore, the INP from dust and BC activated at much colder temperatures can be advected at warmer temperature levels which gives the impression that dust and black carbon are much dominant INP at temperatures warmer than -15ºC.

Lines 909-911: It's good to mention "In our study". Although such a sensitivity study is important in a high-resolution mesoscale model, however, this study has a few limitations that should be mentioned especially when talking about the shortwave and longwave flux radiation. Those limitations are 1- the small chosen domain representing maybe one or two types of ecosystems and not a whole globe, 2- the limitation in the vertical resolution (model top was at 16 km) and not the whole atmosphere, and 3- it's a mesoscale model and not a climate model to eventually get a global conclusion on the impact of PBAP.

Reply: We have added this information in the text (Line 1013-1017)

Minor comments

1- Line 42-43: How little is the effect on the ice phase in the convective region? Consider providing a value similar to the stratiform region.

Reply: Done

2- Line 46-47: Same comment as above. Provide this no significant in number or percentage?

Reply: Done

3- Line 47-48: Same comment as above. How little is the effect on surface precipitation as well as on shortwave and longwave?

Reply: Done

4- Lines 57-64: although those are well known, consider adding a reference at the end of each sentence in those lines. There are many.

Reply: We have added new reference in the revised manuscript.

5- Line 74: Only insoluble material in the PBAPs? Please provide a reference here?

Reply: Correction has been made. We have added a reference after the statement.

6- Line 90: make sure of Hummel et al 2018 whether it fits here?

Reply: Hummel et 2018 is removed from the reference.

7- Line 102: consider adding a reference at the end of the sentence here.

Reply: This is a general statement and does not need a reference.

8- Line 178: Be consistent, either PBAP or PBAPs (you decide) throughout the whole manuscript

Reply: We would like to keep PBAPs. We use PBAP when used as singular.

9- Figure 1, remove one of the (c) on the right or left side of that part of the figure/plot (CCN Conc vs Supersaturation)

Reply: Done

10- Line 277: Number concentration or mass concentration? Please be more specific.

Reply: It is mass concentration. It is corrected in the revision.

11- Line 341: What does ATTO stand for?

Reply: The Amazon Tall Tower Observatory (ATTO) is a research site located in the middle of the Amazon rainforest in northern Brazil. The PBAP samples that were used in the construction of the empirical formulation were collected at the ATTO site.

The text is modified accordingly.

12- Line 369: add the paper reference here.

Reply: Done

13- Line 459: be consistent with the unit! Either use L^{-1} or cm^{-3}. If figure 4 shows it with cm^{-3}, then try to use/unify that throughout the whole manuscript.

Reply: In the revision, we have used cm^{-3} throughout for describing our results and conclusions.

14- Line 494: remove “illustrates” after “This”.
Reply: Done

15- Lines 520-521: similar to the comment in line

Reply: Done

16- Figure 6-a: The ticks need to be fixed to match the corresponding bars

Reply: Done

17- Line 772: I think the precipitation is shown in Figure 13, f and not So, consider changing that in the text.

Reply: We agree. Changes in the text are made accordingly.

18- Line 913: consider removing the second “affected”

Reply: Done

19- Lines 1073-1078: The same reference is written Put the full citation of part I and remove one of the copied part II citations.

Reply: Correction made

REPLY TO REVIEWER 2:

Review of "The influence of multiple groups of biological ice nucleating particles on microphysical properties of mixed-phase clouds observed during MC3E" by Patade et al.

General Comment:

The present study evaluated the importance of multiple groups of primary biological aerosol particles (PBAP) in the cloud microphysical properties of a mesoscale squall line event that took place in the 20 May 2011(USA) in 20 May 2011. Ground-based observations were combined with in-situ aircraft measurements to correlate observations with model simulations. The used model includes a new empirical parameterization for PBAP derived from observations at the central Amazon. The study is well motivated, it nicely fits in the journal scope and it is a good example on how field observations can be properly integrated into an aerosol-cloud model. Although the current results are very interesting, the document needs to be deeply improved before it can be accepted for its publication in ACP.

Reply: We thank the reviewer for his/her valuable suggestions. We have addressed the comments carefully and have made the changes accordingly.

Major comments

The manuscript is unnecessarily long. Several parts can be transferred to a Supplementary Material in order that the Results section can focus on the most important findings making the document more concise and readable. I think Figures 11, 12, and 13 together with the related text should be moved to the Supplementary Material. These results add little to the manuscript and make the document longer than needed.

Reply: We thank the reviewer for his valuable suggestion. We have tried to reduce the length of the article in revision by removing plots and discussion associated with sensitivity tests where only Fungi and Bacteria loading was changed. A few other plots that are less important and associated text has been removed to reduce the length of the manuscript. The result section is now more focused. A separate discussion section is added.

There is a clear lack of discussion. An extremely brief discussion of the results is included in the Summary and Conclusions section; however, I suggest adding a new Discussion section where the present results are deeply discussed and compared with previous studies providing clear explanations of the findings.

Reply: We thank the reviewer for his valuable comments.

In the revised manuscript we have added a separate Discussion section (**Section 8**) where we discuss our results and compare them with previous studies.

The Summary and Conclusions section needs to be improved by being more concise.

Reply: In the revised version of the manuscript we have improved the Summary and Conclusions.

The PT21 empirical formulation for multiple groups of PBAP INPs were used in the present study to evaluate the role on PBAPs in cloud microphysics in a mesoscale squall line event in the US; however, the PT21 formulation are based on field observations over central Amazon. Therefore, I am wondering what could have been the impact of using tropical parametrizations to understand a mid-latitude event.

Reply: It should be noted that the PT21 scheme is universally applicable to all environments globally. It takes the concentration of each type of PBAPs as its input. The scheme imposes a requirement for the user to somehow specify those input concentrations and of course these concentrations vary widely globally and through time. When only total PBAP concentration is know a 'default size distribution' of PBAP from PT21 can be used (Table B1 from PT21).

Currently, the PBAP measurements over different geographical locations (including the region where we carried out the simulation) are rare, which prevents us from using the region-specific PBAP observations for the present study. We have used the PBAP parameters for ice nucleation activity and **relative** abundance of the five PBAP types based on the observations from Amazon. However, the **absolute** number concentration of PBAPs is constrained to agree with their typical concentration in the atmosphere.

In the revised manuscript we have added some analysis in which the model estimated values of one of the major PBAP bacteria are compared with the observations. The bacterial number concentration from AC is based on the input mass fraction of bacteria and default size distribution parameters from PT21. Our analysis showed that the estimated values of bacterial number concentration are overall in fair agreement with observations. Based on this we believe that AC was able to estimate the PBAP number over the region considered in the current study.

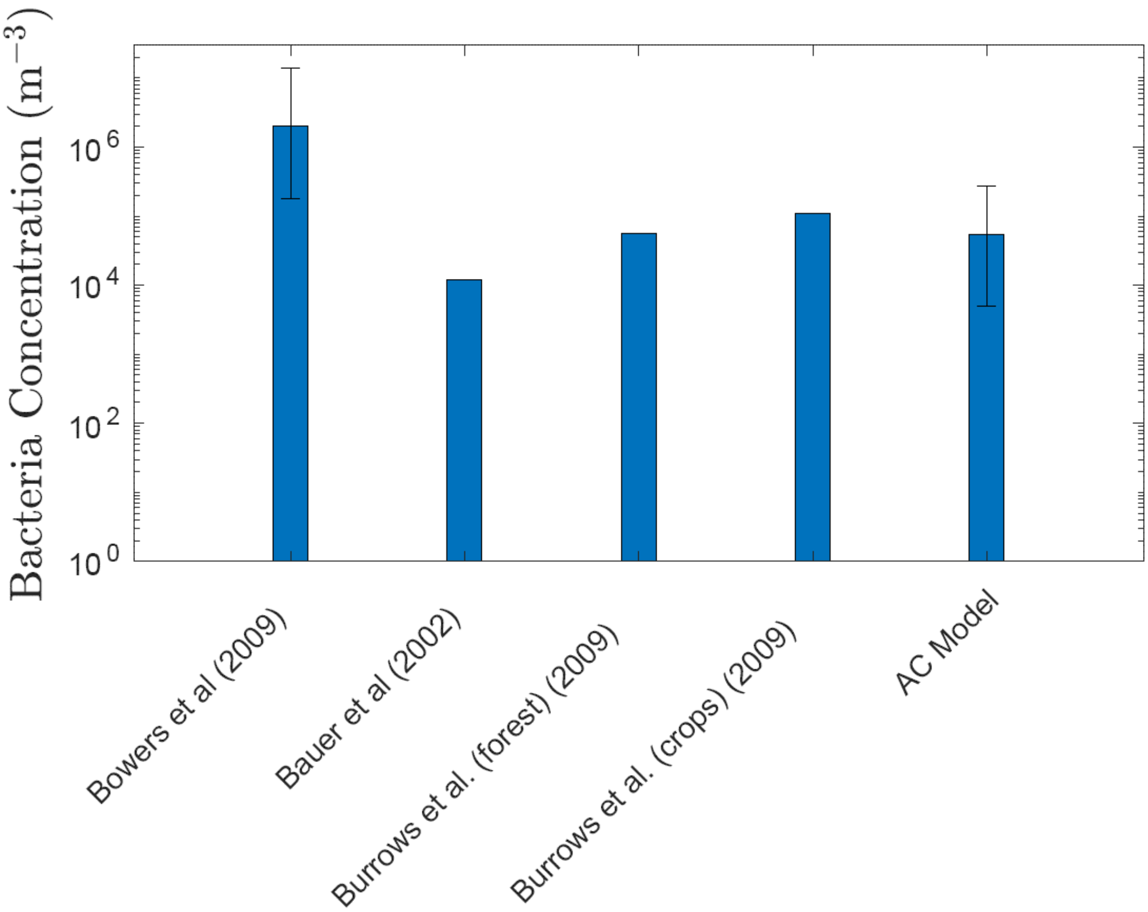

Figure: The comparison of model estimated bacterial number concentration with various observations.

The Introduction section lacks several key references. This part needs to be improved giving credit to previous work, including recent studies.

Reply: We thank the reviewer for his valuable comments. In the revised manuscript we have added relevant key references.

I suggest to avoid repeating the same information along the text. Avoid redundancy as much as possible.

Reply: We have checked the manuscript carefully to avoid the repetition of similar information.

Minor comments

Lines 363-364: “it was parsimoniously assumed that 50% of the insoluble organics were biological in origin”. How realistic is this assumption?

Reply: Unfortunately, there are very few observations available in the literature on this. Observations by Matthias-Maser et al. (2000) found that 25% of the total insoluble particles are biological. Mattias-Maser and Jaenicke (1995) showed that PBAP can amount to 20% and 30% to the total aerosol particles. The observation by Jaenicke (2005) in a semi-rural location showed that cellular particles can contribute up to about 50% of total particles. Based on this our assumption looks realistic.

We have added this information in the text (Lines 433-438)

Lines 364-365: “The total PBAP loading was prescribed partly based on observations of insoluble organics and partly based on the assumed fraction”. Please indicate the work on which this is based on.

Reply: In the AC, estimation of total PBAP loading was based on some assumptions as described in section 3.3. The observations of insoluble organics were used to determine the total PBAP mass. In the AC model, 50% of the insoluble organics were biological in origin. The mass fraction of each PBAP group in total PBAP mass is prescribed based on the PT21 observations. The fraction of mass mixing ratio for various PBAP groups is: FNG= 0.39, BCT= 0.13; PLN= 0.31; DTS= 0.17; ALG= 2.5×10^{-4}.

Relevant references are added in the revised manuscript

Line 381: The CCN measurements were performed at 300 m MSL while the predictions at 500 m MSL. It is well known that the aerosol concentration varies with altitude, therefore, I am wondering what the reason for this good agreement is.

Reply: The boundary layer height at the beginning of the storm was around 1 km and a strong mixing of aerosol particles with less variation in the vertical is expected. The difference of 200 m between the height of CCN observations and the model first level may have little bias as reflected by the error bar on the observed and AC CCN spectrum (see revised CCN plot Figure 2)

Lines 436 and 443: “are in good agreement”. Provide a statistical evaluation to support this, being more quantitative.

Reply: We have modified the text and have changed “are in good agreement” to “in fair agreement”.

Line 524: “is dominated by black carbon”. I am wondering why from the simulations BC is the dominant INP at these warm temperatures as it is well known that BC is not a good INP at such temperatures.

Reply: The activation of dust and black carbon INP starts at temperatures colder than -10 and -15^{o}C. The number concentration of activated INPs in **Figure 6** (in the previous version Figure 5) is based on an advective tagging tracer explicitly dedicated to tracking the ice initiated by various INPs. The ice initiated by dust and BC at temperatures colder than -15^{o}C can be advected to the lower level and affect the INP concentrations at warmer temperatures

Figure 6. I am not sure why homogeneous freezing is separated from primary ice as these type of particles are not from SIP. I suggest changing it to “Prim_Het” and “Prim_Hom” or something like this.

Reply: We have made the changes in the figure accordingly.

Lines 572-573: “The simulated cloud droplet diameter is mostly smaller than 15 μm”. Is this shown somewhere?

Reply: Yes, the vertical profile of cloud droplet diameter is shown in **Figure 4f.**

Lines 574-575: “AC represents the observed dependency of rime-splintering on the concentration of droplets > 24 μm”. Is this shown somewhere?

Reply: We wanted to say that the rate of rime-splintering in the AC model depends on the concentration of cloud droplets with a diameter > 24 μm. We have modified the sentence accordingly. It is not shown in a separate plot.

Line 575-577: Is this based on a previous study. If yes, please add the corresponding reference.
Reply: We have removed this statement in the revision.

Technical comments

Line 33: Please add the used model.

Reply: The Aerosol Cloud (AC) model is our model available at Lund University.

Line 41: …and ice CLOUD microphysical

Reply: Correction made.

Lines 46: Should "artificially prohibited" be replaced by "intentionally shut down"?

Reply: Correction made.

Line 56: Although the "(Forster et al., 2007)" reference is appropriate, I suggest adding an updated one.

Reply: Correction made.

Line 59: Add a reference after "formation".

Reply: Done

Line 61: Add a reference after "budget".

Reply: Done

Line 64: Add a reference after “INPs”.

Reply: Done

Line 67: Add other references together with “(Heymsfield and Field 2015)”.

Reply: Done

Line 68: Add a reference after “climate”.

Reply: Done

Line 72: Add other references together with “(Matus and L’Ecuyer 2017)”.

Reply: This statement is deleted in the revision.

Line 67: Add a reference after “lipids”.

Reply: Done

Lines 78-79: Add the typical freezing temperature of Pseudomonas syringae.

Reply: Done

Line 81: …many years; however,…

Reply: Correction made

Line 82: Add a reference after “debate”.

Reply: Done

Line 85: Define “immersion freezing”.

Reply: Done

Line 102: Add a reference after “atmosphere”.

Reply: Done

Line 109: Add a reference after “clouds”.

Reply: Done

Line 113: Add a reference after “mechanisms”.

Reply: Done

Line 115: Please indicate what the authors mean with “by modifying the order of magnitude of ice particle concentrations”.

Reply: The SIP mechanisms can increase the total ice number concentration by a few orders of magnitude and can affect cloud properties.

We have modified the corresponding statement.

Line 128: Add a reference after “uncertain”.

Reply: Done

Line 129: Replace “(e.g., Hallet-Mossop, 1974)” by “(Hallett-Mossop (HM), Hallett and Mossop, 1974)”.

Reply: Done

Lines 132: Replace “the Hallet-Mossop (HM) process” by “HM process”.

Reply: Done

Line 134: Please indicate what the authors mean with “generated by biologically active landscapes”.

Reply: We mean the landscapes that can generate a considerable amount of bioaerosols e.g. forests, woodlands, etc.

Line 138: Add a reference after “activities” and “atmosphere”.

Reply: Done

Line 140: Delete “(” after the comma.

Reply: Done

Line 156: Define “IN”.

Reply: Done

Line 161: Add a reference after “issue”.

Reply: This is a general statement and relevant reference is mentioned in the next statement.

Line 161: Define “INA”.

Reply: Done

Line 162: Add a reference after “nucleation”.

Reply: This is a general statement.

Line 163: Are there artificial biological INPs?

Reply: We have removed the word “natural” from the mentioned statement.

Line 164 (and along the text): “real atmosphere”.

Reply: Correction made.

Lines 175-178: I suggest deleting these lines.

Reply: Done

Line 178-181: I suggest adding these lines to the previous paragraph.

Reply: Done

Line 184: "field campaign of observations" is unclear to me.

Reply: We have modified the title of the section to 'Description of Observations'

Line 216: "in situCLOUD microphysical"

Reply: Done

Table 1: Add the 2D-C, CDP, HVPS-3, King hot-wire probe, temperature probe, and Static pressure sensor manufacturers.

Reply: We have the names of manufacturers in the table. The information about manufacturers of temperature and static pressure was not available.

Line 248: Add the model of the CCNC.

Reply: Done

Line 252: Please indicate what the authors mean with "the extended facility deployed at CF measured"

Reply: We have changed 'the extended' to 'the measurement'

Line 259: Please add the soundings times?

Reply: Correction made.

Line 265: Fix "Giangrande et al. 2014."

Reply: Done

Lines 273-274: Please add the techniques/methods used to measure black and organic carbon, salt, ammonium sulfate, and dust.

Reply: We have added relevant reference for it in the revised manuscript.

Line 316: Should “IN” be “INP”?

Reply: Done

Line 343: Please double check the name of the diatom.

Reply: Done.

Line 348: “domain” is repeated.

Reply: Correction made

Line 400: “ice CLOUD microphysical parameters”.

Reply: Done

Line 403: Replace “liquid water content” with “LWC”.

Reply: Done

Lines 439-441: “Overall, the mean values of 439 CDNC simulated for convective and stratiform regions are in good agreement with 440 observations.” Delete these lines as the same information is repeated a few lines below.

Reply: Done

Line 459: Should “predicted” be “expected”?

Reply: We agree. Correction made

Line 478: Add a reference after “applied”.

Reply: Done

Line 495: “illustrates” is repeated.

Reply: Correction made

Line 509: Define “PBL”.

Reply: Done

Lines 522 and 523: “L-1” should be fixed.

Reply: Corrections made

Line 535: Please clarify what the authors mean with “budget”.

Reply: We have modified the sentence.

Figure 6: Add units to the y-axis in panel A

Reply: Y- axes is unitless. It denotes the total number of ice particles generated through various mechanisms per 10^{15} particles during the whole simulation.

Figure 7: Swap items a and b in the caption to be consistent with the Figure.

Reply: The item a and b are consistent with the figure. It should be noted that item a and b is followed by the corresponding figure. For example, **(a) Liquid water content**; **(b) cloud droplet number concentration**. We have followed same format throughout the manuscript.

Figure 9: Swap items a and b in the caption to be consistent with the Figure.

Reply: The item a and b are consistent with the figure. It should be noted that item a and b is followed by the corresponding figure. For example, **(a) Liquid water content**; **(b) cloud droplet number concentration**. We have followed the same format throughout the manuscript.

Figure 10. Add the units to the y-axis in both panels.

Reply: Y- axes is unitless. It denotes the total number of ice particles generated through various mechanisms per 10^{15} particles during the whole simulation.

Lines 773-774: Delete them as this was already mentioned.

Reply: Corrections made

Lines 781-782. The stratiform region is also part of Figure 14.

Reply: Correction made