We would like to start by thanking both reviewers for thoughtful and very helpful comments that have improved the manuscript.

The reviewer comments are listed in black below, while our replies are in *blue and italic.*

Please note also the following changes that have been made to the revised manuscript on top of those arising from the reviewer comments:

- By an error in the code, the ERA5 data used before was in fact not regridded, but used the original 0.25 resolution. This has been rectified and the changes are minor.
- Furthermore, the TRMM data has also been regridded to 1 degree for consistency. For the correlations, the change from this was very small, but a slight increase in the strength of the correlations is seen.
- Figure 1 had a mislabeling of the models in the top two panels that has now been corrected.
- We have added a XGBoost hyper parameter tuning section to the supplement. This did not lead to changes in the parameters, but confirms that our previous choices are reasonable.

Table of content:

Referee #1:

General comments

This study uses Earth System Model output, in-situ observations, reanalysis data, back-trajectories, and machine learning to investigate the effects of recent precipitation on the aerosol size distribution at three locations. Specifically, the authors evaluate the models with respect to a hypothesis from the literature that the removal of large particles by precipitation can trigger new particle formation events at these sites. The study represents a novel and valuable contribution to a subject in the scope of ACP: aerosol-cloud interactions and new particle formation processes, the climate effects of which are notoriously uncertain due to the need for complex, unconstrained parameterisations in ESMs.

The authors make very interesting conclusions about new particle formation-precipitation interactions, which lead to important recommendations for future research. However, the results in this article are complex due to the fact that the authors have undertaken a very thorough analysis of the data available, (including several different variables, models, locations, seasons, data products and sensitivity tests). Hence, the article would benefit from some simplification, including more synthesis of how these multiple strands of evidence lead to specific conclusions about individual physical processes in ESMs. I also recommend more discussion of the sensitivity of the results to the use of different meteorological datasets. As such, I recommend that this article should be published following some revisions to address these points. Specific recommendations are given below.

We thank referee 1 (Ruth Price) for these valuable comments that we believe have greatly improved the manuscript.

Specific comments

1. **Different meteorological datasets**: at the high-latitude stations, ERA5 precipitation data are used along HYSPLIT back trajectories to compute correlations in the observations. This differs from the approach in Khadir et al., where GDAS1 precipitation was used. Could the authors clarify why this change was made? There is some indication that results are sensitive to this choice (e.g. Figs. S9–S15, lines 394–395, 688–690). Given this sensitivity, it would be helpful to include a more explicit discussion about the robustness of results to the choice of meteorological dataset, particularly in the conclusions. For example, NorESM and EC-Earth appear to overestimate precipitation at ATTO; could this contribute to the overestimated particle formation and growth rates observed in these models (Figs. 5/6)? Similarly, while Section 2.5 argues that GDAS1 and ERA-Interim dynamics are sufficiently

similar, it would be helpful to show a comparison over a subset of the data to validate that assumption.

Yes, indeed, this was a difficult choice. Basically, the GDAS data extrapolated via HYSPLIT truncates close to zero values to absolute zero, which makes some difference to the correlation analysis – exemplified by Figures in section S5 in the supplement which show the models with truncated precipitation values. For an apples-to-apples comparison between the models and the observations we decided to extract the precipitation from reanalysis data in the same way that was done in the models (nearest neighbor interpolation with similar resolution). In this case ERA5 data seemed as good of a choice as any other and better than using GDAS. The fact that it's a different re-analysis dataset than the one used to create the trajectories makes it more similar to the model treatment. See for example Heslin-Rees *et al.* (2024)*, Fig. S5 in. the supplement for a comparison of precipitation from GDAS and ERA5.*

We thoroughly agree with the reviewer on the points above and have added the following text:

To clarify why the choice to differ from Khadir was made*, the following text was added to the methods (second sentence, section 2.3):*
"This differs from Khadir et al. 2023, where GDAS data was used as extracted by HYSPLIT. This change was done in order to get a cleaner comparison to the models, firstly because we use the same nearest neighbor algorithm to extract the precipitation rate and secondly because HYSPLIT truncates precipitation values close to zero to absolute zero, which was found to impact the correlations (see figures S16-19.)"

To add discussion of the robustness of the results in the conclusions:

We have added the following paragraph at the end of section 3.1 (results):

> *"In this study, back trajectories created with HYSPLIT are used to extract precipitation data from nudged model simulations and for the observations, the same procedure is used to extract precipitation from ERA5 reanalysis and TRMM 3B42 V7 satellite product, both of which are regridded to 1x1° resolution. Our approach for the observations differs from Khadir et al. (2023) in terms of the re-gridding and that Khadir et al. (2023) used HYSPLIT to extract precipitation from GDAS (relevant for Hyytiälä and Zeppelin stations). The difference between using the HYSPLIT/GDAS data and ERA5 data can be seen in section S4 and shows mainly the same patterns, but a stronger signal when using the ERA5 data. The main exception to this is Zeppelin during the Haze season and Slow-build up, where HYSPLIT/GDAS gives clearer patterns and ERA5 gives less significant results. We find that the stronger correlations with ERA5 are likely due to the truncation to zero (mentioned above) done by HYSPLIT for near-zero values (see e.g. Fig. S3, because when we apply a similar*

truncation to the model data, the correlations also go down in magnitude (see section S5).

The fact that the main features are preserved with the change of dataset gives us confidence both in the methodology in general, and in the usage of the HYSPLIT trajectories to extract model data. The deviations for the cold seasons in Zeppelin give some ground to be cautious when interpreting results from these seasons."

We added the following paragraph to the conclusions:

"The methodology of this study assumes that back trajectories produced with one reanalysis dataset (GDAS) can be used to extract 2D precipitation fields from nudged model simulations and other re-analysis datasets (ERA5) without this significantly influencing the results. Using the HYSPLIT trajectories instead of, for example, trajectories based on model meteorology will naturally introduce some error, but at the same this methodology gives a more representative comparison to the observations, where we also expect some error introduced between the "true" trajectory and the one based on re-analysis data. When comparing the analyses of the observations using data from the two different reanalysis datasets, we find mainly the same features, which give us confidence in both the observational and the model analysis. A potential exception is the two cold seasons at Zeppelin (Slow build-up and Haze), where the two datasets differ more, which gives more grounds for caution when interpreting results from these seasons."

We would also like to point the reviewer to line 262 in the new version, where we already mention this: "Although the nudging is not done with GDAS1 directly, previous work (Heslin-Rees et al., 2024; Isokääntä et al., 2022; Talvinen et al., 2025) has indeed shown that back trajectories computed with GDAS1 and ERA-Interim are comparable."

Wrt. the precipitation and impact on growth at ATTO:

Unfortunately Fig. 2 had an error (see comment above), which might have been misleading in this regard. Additionally, we have now regridded the input data for ATTO precipitation, so that the grid size is more similar to the models.

Actually, neither NorESM nor EC-Earth seem to overestimate precipitation (see Figs. S3-5), but they seem to overestimate low level precipitation (see Fig. 2). This could of course mean that there is a bias towards drizzle versus deep convection, which could be linked to the lack of correlation. However, there is still a difference in grid size between the models and the observational product and we would therefore prefer to not speculate at this point.

Wrt. GDAS1 and ERA-Interim dynamics being sufficiently similar

In the observations, the initial study by Khadir et al. used GDAS precipitation and GDAS meteorology to drive HYSPLIT. The following paragraph has been added to the conclusions:

> *"Compared to 'reality' there is an, to some extent, unknown error in the trajectory compared to the 'real' back trajectory of the airmass arriving at the station and in the precipitation experienced along that trajectory. In other words, the observational constraints include noise from trajectories deviating from the 'real' back trajectory and the 'real' experienced precipitation. When models are nudged with ERA-Interim data and then we use the GDAS trajectories to extract precipitation history in the models, we are introducing a similar level of noise to what we have in the observations, at least if we assume the difference between ERA-Interim data and GDAS data is similar to the difference between the observations and the GDAS data. Viewed this way, it may actually be considered a more apples-to-apples comparison to use different datasets for nudging and trajectories. "*

We do however, have some indications that the analysis is not very sensitive to the meteorology dataset. When switching from precipitation from GDAS extracted by HYSPLIT to precipitation from ERA5 (extracted in the same way as for the models) the signal in the correlations gets clearer, except for Zeppelin in Haze and a little bit in Slow-build up. Secondly, if the 'real' model transport was highly different from the trajectories from GDAS, then one would expect the signal to be noisy and to not see correlations. The fact that we see in at least as strong correlations in the models as in the observations is therefore one indication that this is not a major source of error.

In an ideal world, we would compute these correlations both for trajectories computed with ERA-Interim, GDAS and using model meteorology to drive HYSPLIT. This would allow for precise quantification of the contribution of meteorology. Unfortunately, this would constitute a rather massive endeavor and given the arguments above, we believe it is not a good use of resources and outside the scope of the current study.

We have, however, added comparisons of the precipitation rates from different datasets to the supplement, please see Figs. S3-6. We thank the reviewer for these thoughts and hope this sufficiently addresses their concerns.

2. **Presentation of key result**: the article highlights the inability of the models to capture CCN replenishment as a central result (e.g. abstract, lines 655–656), along with possible causes and implications of this finding for model processes. However, Section 3.2, where this is largely discussed, also includes extended treatment of NPF inhibition and particle removal processes. This mix reduces clarity. **The structure of Section 3.2 (and therefore the article as a whole) would be improved if it focussed solely on CCN replenishment (i.e. positive correlations between precipitation and particles <50 nm) to make the key result clearer and more concise.** For example, the paragraph beginning at line 376 contains only one sentence on the replenishment

signal before shifting to NPF inhibition and offline oxidant fields, despite these topics being already discussed in Section 3.3 (e.g. beginning at line 455).

We thank referee #1 for this comment and are happy to accommodate it.

Firstly, we sharpened the first paragraph describing Hyytiälä so the NPF inhibition now takes less space. From line 370 now reads:

> "[…] Very recent precipitation (approximately 0-20 h), on the other hand, is seen to correlate negatively with the smallest particles (below 100 nm), which is consistent with NPF being inhibited by 1) reduced photochemistry that limits oxidant availability to produce NPF precursors (e.g. Jokinen et al. 2017), 2) a loss of critical NPF precursors (e.g. gas phase sulfuric acid and sulfur dioxide) via wet and dry deposition and 3) a high coagulation sink for newly formed particles associated with rainfall and high humidity (hygroscopic growth of particles) and 4) direct scavenging of smaller particles by precipitation (e.g. Slinn, 1984)"

We also added the following before treating NPF inhibition at line 376:

> *"All models show signs of NPF induced by past precipitation, though the timing and the growth differ. The observations show a positive correlation from around 25 hours back in time and peaking around 55. ECHAM-SALSA has a slightly too early positive correlation and peak, NorESM has a positive correlation only after approximately 50 h back in time (later than observed), while EC-Earth is quite consistent with the observations."*

The paragraph discussing growth is also moved up, so now the NPF inhibition is discussed after everything to do with NPF and growth.

The rest of section 3.2 has also been toned to focus more clearly on CCN replenishment.

3. **Section 3.3**: this section would benefit a clearer structure to support the reader's understanding of its contributions.

We agree and thank the reviewer for very useful pointers on how to improve the section.

- *Section 3.3.1*: detailed, speculative discussion in this section is sometimes difficult to follow. For example, lines 473-474 and subsequent discussion reference a nighttime/daytime difference in N10–30 vs precipitation at HYY, whereas it is difficult to see any such strong difference between the observed relationships at HYY in Figure 7b. Moreover, the language used to describe Figure 7 is at times somewhat imprecise (e.g. lines 469–470 "NorESM remains unchanged" could be changed to "the relationship between N10-30 and accumulated rainfall remains unchanged in NorESM"). Overall, since the section does not identify a key result (e.g. line 490: "It is hard to draw conclusions about model skill"), much of this content could be moved to the supplement, with only essential findings retained in the main text.

This is a very good point and upon further consideration we have also come to the conclusion that the division into night time and all data is likely not a good criterion for targeting direct scavenging losses versus NPF inhibition. This is because the existence of particles in this range is in these stations likely always depending on NPF (if there was no NPF, there is likely no particles in the range), so disentangling NPF inhibition and direct precipitation scavenging is therefore not possible.

We have now moved most of the text previously in this section to the supplement and added the following:

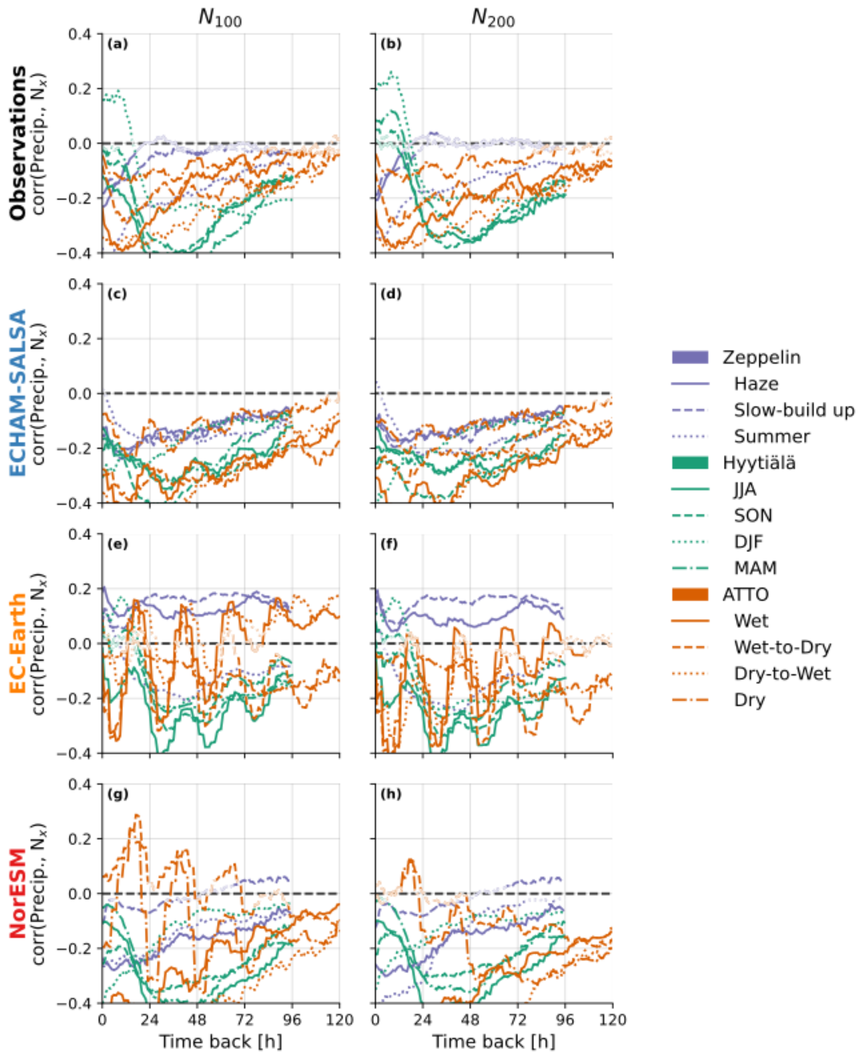
> "Separating the impact of NPF inhibition from impact of scavenging of the smallest particles has proved difficult. We have attempted to investigate the impact of precipitation during night time, as NPF is very rare during night, especially with subsequent growth, see section S2 in the Supplement. However, this approach was found to give highly inconclusive results. This is likely because the particle concentration in the range (10-30 nm) is highly dependent on NPF happening and night time values will still be affected by NPF the previous day for example. In general attempting to pick times when NPF is not impacting the size range means picking times when there are few particles to scavenge.
>
> In sum, we can see in Fig. S1 that models underestimate the negative impact of recent precipitation on the size range 10-30 nm for Zeppelin and Hyytiälä, with the exception of NorESM at Zeppelin. At ATTO, EC-Earth and NorESM are found to underestimate the percentwise increase in particles associated with recent precipitation, while ECHAM-SALSA overestimates it although in absolute numbers (non-normalized) it still underestimates the source (see Fig. S2)."

- *Figure 7:* this figure itself is difficult to interpret because the overlapping lines are hard to distinguish; using separate subplots for "all" and "nighttime" could improve this.

We thank the reviewer for this suggestion and have followed it.

- *Section 3.3.2:* this section introduces seasonal data that had not been discussed earlier in the paper, adding considerable complexity. However, Figure 8 does not allow for individual seasonal relationships to be easily interpreted (it is also not clear why some sections of lines are faint). These issues make it difficult to follow how the conclusions at the end of Sections 3.3.2 are derived. A clearer, more structured presentation that links explicitly to earlier sections would help.

Firstly, wrt. Fig 8 and the others like it, the faint lines indicate areas where the correlations are not significant. We have modified the caption: "Spearman correlations between precipitation rate and (a) N100 and (b) N200 for observations (top left) and models. Each line represents a season at a station. Faint lines indicate where the correlation is not significant (p>0.05)."" Secondly, we have added different linestyles to indicate the seasons in these figures. They now look as below:



We also have added "separated into seasons" to the first sentence in the sections, to highlight and introduce this topic clearer.

The whole section has been adjusted for clarity (see track changes document).

4. **Temperature dependence of particle scavenging**: section 3.4 contains the very interesting result that an observed temperature dependence of particle scavenging is not captured by models. However, similarly to comments above relating to section

3.3.2 and figure 8, it is very difficult to interpret Figure 9 due do the way data from different seasons are included but cannot be distinguished. Please consider revising Figures 8 and 9, perhaps limiting the data to just the seasons that are already presented in section 3.2.

This is indeed a fair request and we have accounted for it (see above for revised figure).

5. **XGBoost analysis**: like section 3.3, this section presents a lot of new information without a clear link to the key results of the article. For example, while the authors state that the XGBoost analysis supports the correlation results, Figure 11a does not show an increase in small particles at longer time lags, which seems to contradict previous findings. It would be helpful to clarify this apparent discrepancy and articulate more clearly how the machine learning results complement the core conclusions.

Fair points. Firstly, Fig. 11a does show positive values, but not as consistently as in the correlation analysis. Note that this figure is not an apples-to-apples comparison to the correlations, because the correlations show us only if a positive relationship exists or not, not how strong an increase this would lead to. In the XGBoost results on the other hand, we are presenting the zero precipitation versus actual precipitation difference, which may be small and noisy, lumps together all precipitation, and does not take into account larger or smaller precipitation rate (just precipitation versus no precipitation). We therefore interpret slightly positive as in agreement with the positive correlation analysis and zero values as not directly in disagreement.

So, the sense in which the results are backed up is that the XGBoost results also find an initial decrease in N10-30 followed by a quick return to close to zero values or even positive values. This differs from the results from N50-100 and N100, which show a return to zero later, indicating growth. We should here have highlighted more the NPF relevant seasons, which is where we expect this signal to agree anyway.

Note that it is expected that XGBoost does not capture all relationships in the data, as its accuracy can be relatively low in some cases (Fig. S41).

We have re-written the entire XGBoost section to highlight agreement or non-agreement with the correlation analysis. Amongst other things, we have added the following paragraph to directly address the comment above:

> "When comparing these results to the correlation analysis, it is important to note that this is not a direct apples-to-apples comparison. A positive Spearman correlation tells us that higher precipitation tends to be associated with higher particle number concentrations in a rank sense, but does not quantify the physical magnitude of that relationship – a weak but consistent effect can produce a clearly positive correlation coefficient. The XGBoost results in Fig. 10, on the other hand, directly estimate how much the actual precipitation is predicted to have changed the number

> concentration relative to a no-rain scenario. A near-unity result therefore does not falsify or contradict a positive correlation, but indicates that while the relationship may be real, its absolute impact on number concentration is modest."

6. **Section 2/table 1**: there are significant repetitions of the information in Table 1 and section 2.1. To make the article as a whole more concise, consider restricting the information to Table 1 and making only brief references in the main text, or further giving details given where necessary.

This is a very good suggestion. We have now shortened and combined the model descriptions under topics rather than under model headlines. Most of the general description has been moved to the supplement and the wet deposition in the models is now compared under headlines of

  - *In-cloud scavenging in stratiform clouds*
  - *Convective cloud scavenging*
  - *Below cloud scavenging*
  - *Resuspension of aerosols when precipitation evaporates*
  - *Phase dependency*

7. **Supplement**: the supplement currently contains a large number of figures, some of which are not referenced in the main body of the article. To make key information easier to find and comprehend, consider removing figures and/or information from the supplement that is not directly referred to in the article.

We have gone through the supplement and found one figure which is repeated, plus that section S6 and S11 was not referenced and not necessary – these have been deleted. Other than that, the SHAP value plots were poorly referenced in the main text. This has been changed by the change of the following sentence in old line 639:

> "Instead, XGBoost likely attributes the positive correlation to source region, highlighted by Fig. S57 showing the SHAP values for each grid cell versus the SHAP values for recent precipitation in Fig. S75."

We have also reorganized slightly the section, so that the XGboost evaluation comes before the SHAP value analysis (in order of importance) to make it easier to handle.

Technical corrections

- Line 3: et deposition should be wet deposition
  - *Corrected, thank you.*
- Line 49: cloud-born should be cloud-borne
  - *Corrected, thank you.*

- Line 120: arctic should be Arctic
    - *Corrected, thank you.*
- Table 1, first row and column: cloud-born
    - *Corrected, thank you.*
- Lines 347-348: "For ATTO, note that the satellite product used for the observations has fewer instances of low precipitation rate compared to the models, and has more higher values instead." My interpretation of Fig 2 is that the TRMM product has many more instances of 0 values of precipitation rate than the models, and fewer instances both of low (0-0.05) and higher values, I struggle to see how Fig 2 shows that TRMM "has more higher values instead". Please clarify this.
    - *Note that this figure has been revised and the TRMM data been coarsened to more resemble the models. We agree with the reviewer and have revised the sentence as follows:*
      "For ATTO, note that the satellite product used for the observations has more instances of zero precipitation, fewer instances of low precipitation, and proportionally more high precipitation rates compared to the models."
- Lines 348-349: "This is likely because to the input data for the observations at ATTO have a resolution…"
    - *Thank you, this sentence was removed in the final version due to changes in the input data for ATTO (see above).*
- Line 429: close bracket missing after (10-30 nm
    - *Corrected, thank you.*
- Lines 453-454: "Additionally, whatever relationship is observed, we see will be a combination" seems like it should be "whatever relationship is observed will be…" or "whatever relationship we see will be…"
    - *Indeed. It now reads "Additionally, whatever relationship is observed will be a combination"*
- Figure 9 caption: suggest it is more logical if it reads "Figure 9. Same as Fig. 8a for cold versus warm conditions at the station separately" rather than "Figure 9. Same as Fig. 8b but for N100 and for cold versus warm conditions at the station separately"
    - *Indeed. We revised it to now read: "Spearman correlations between precipitation rate and N100 for cold (defined as below -5 ° C at the station) versus warm (defined as above 0 ° C at the station) conditions at the station separately. Otherwise the same as Fig. 7" (Note the updated Fig number).*
- Line 674: "the smallest particlethe NPF"
    - *Thank you, it now reads: "if the appearance of the smallest particles after rain is driven by ozone injection with downdraft"*

Referee #2

This paper by Blichner *et al* looks to investigate how much of an impact precipitation can have on the CCN budget. Precipitation is typically seen as a CCN sink and so research into how precipitation can lead to new aerosols that can then grow and potentially become part of the CCN budget once again is important. They explore how precipitation can lead to good conditions for new particle formation and an increase in smaller aerosols due to downdrafts brought about by precipitation events. They explore how different processes affect the particle number size distribution (PNSD) after rainfall. This work is building on ideas suggested by Khadir et al (2023) and introduces three Earth system models to see how well they predict changes to PNSD along back trajectories from precipitation events.

They have selected three different models, nudged to ERA interim analyses, which use different in-cloud and below cloud processing schemes. They also look at how the models treat phase dependency, redistribution of aerosols and aerosol transport differently. This gives them a range of approaches to modelling aerosols and clouds which can then be attributed to different sensitivities that the models exhibit, compared to observations taken at three different sites (two high latitude sites and one tropical). They primarily explore the correlation between precipitation and number concentration and how this differs for different aerosol sizes, with different times away from precipitation event. They discuss their results process by process to gain an understanding of how each affects the aerosol number size distribution, back through time. They also use XGBoost, a machine learning algorithm that is good at predicting outcomes from a small number of input features. This is used to support the arguments their main methods suggest by looking at whether the same patterns seen in the correlation figures can be picked out from the predictions of these machine learnt models. They also do some feature analysis to help pick out potential correlations that shouldn't be attributed to precipitation.

The main results from this paper show that models are underpredicting new particle formation and growth after precipitation, compared to observations. They perform better at the ATTO site, where the increase in small particles is linked to downdrafts instead. It is unclear however whether improved model performance is for the right reasons, with some existing hypotheses impossible within the models available. They also see further verification of previous work that showed particle growth at the ATTO site was much too high in several of the models. XGBoost presented an interesting new way to further analyse these correlations and potentially helped to identify questionable correlations seen in other methods.

This paper is written very well with a clear narrative path throughout the sections, introducing all the important concepts as well as previous work that this is building upon. This paper will fit within the current literature around this topic, exploring some new novel results that provide further understanding of how models perform when looking at the CCN budget after precipitation. The conclusions are clear and show scope for future research, outside of this paper. The title, abstract and results reflect the work that has been done. I believe this paper should be published, once minor comments (outlined below) have been addressed.

General comments:

Thought 1 (Introduction): The discussion of the paper is primarily on a weather timescale (4 days) or so however the paper starts by talking about the impact of aerosols on climate and human health. I know the general discussion after the first few lines is in aerosol dynamics which are at a weather timescale but I think it would be good to introduce aerosol impacts on weather timescales in these first few lines.

This is an interesting point, but we would argue that in discussing CCN (which is what we are currently doing in the introduction), we are in fact discussing aerosol dynamics on a weather timescale. The climate aspect of this paper is due to that its topic is evaluation of aerosol dynamics in climate models, which are designed to investigate climate questions, even though for this paper, the time scales are too short. We discuss climate because accurate representation of aerosol processes is highly important for accurately representing the CCN response to changing emissions (a climate question).

Thought 2 (Introduction into Methods): HYSPLIT and GDAS1 are introduced in both sections without any references to the specific model/method used with regards to HYSPLIT and no reference for GDAS1. I think a more substantial introduction and explanation of these two things would be appropriate as well as a reference to the GDAS1 dataset and HYSPLIT.

Thank you this is a very good point. We have added the full name of HYSPLIT in the end of the introduction (old L132):

> *"We use nudged simulations with high temporal resolution output, combined with trajectories derived with The Hybrid Single-Particle Lagrangian Integrated Trajectory model (HYSPLIT) to produce trajectories and extract precipitation rate along the back trajectories in the models and re-analysis."*

Additionally we have added references and a better introduction in the method section (old L290):

> "We use a trajectory-based approach to investigate the relationship between precipitation and aerosol properties.
>
> Back trajectories were computed for Khadir et al. (2023) using HYSPLIT Stein et al., 2015) and were computed at each station every hour during the PNSD measurement period.The meteorological data used to drive HYSPLIT was from the Global Data 5 Assimilation System (GDAS1, 1 ° x 1 ° resolution; *https://www.ready.noaa.gov/gdas1.php*). Please see Khadir et al. (2023) for details."

Thought 3 (Methods): I think that the models are well outlined however I think the table for comparison is too complicated and a lot of it is repeated in the individual models sections. I struggle, using the table and the individual sections to fully grasp the differences between the models. I believe **a simpler table that just outlines each part and an additional section at the end**, comparing the models in terms of schemes and complexity (linked back to the introduction when wet deposition scheme complexity was discussed) **would be useful to support later discussions of model performance**.

This is a very good suggestion also highlighted by the other reviewer. We have not changed the table, but we have shortened and combined the model descriptions under topics rather than under model headlines. Most of the general description has been moved to the supplement and the wet deposition in the models are now compared under headlines of

- In-cloud scavenging in stratiform clouds
- Convective cloud scavenging
- Below cloud scavenging
- Resuspension of aerosols when precipitation evaporates
- Phase dependency

We hope this addresses the concern of the reviewer.

Thought 4 (results): I think that the figures within figures could benefit from labelling with a), b), c) etc so that when they are referred to, they can be referred to as 2b) and so on to make it clear which figure is being referenced. Also some of the figures have faint lines for some reason (see figure 8). Figure 7 could do with being split into two plots (one for nighttime and one for all) as it is hard to read the results. Overall the spacing between the figures and size of the figures could be tweaked to remove some of the white space too.

These are all good points and the following revisions have been done:

- *Labelling with a, b, c etc.*
- *The faint lines are when the correlations are not significant. This information has been added to the figure caption.*
    - *Caption for Fig. 7 (old 8) now reads "Spearman correlations between precipitation rate and (first column; a, c, e, g) N100 and N200 (second column; b, d, f, h) for observations (top row, a and b) and models (c-h). Each line represents a season at a station, with color indicating the station and linestyle indicating the season. Faint lines indicate where the correlation is not significant ($p>0.05$)."*
    - *Caption for Fig. 8 (old 9): "Spearman correlations between precipitation rate and N100 for cold (defined as below -5 ° C at the station) versus warm (defined as above 0 ° C at the station) conditions at the station separately. Otherwise the same as Fig. 7"*
    - *Caption of Fig. 9: "Correlations between precipitation rate and aerosol mass for observation (ACSM data and Aethalometer measurements, a-d) and models (e-h). Each line represents a season at a station (see colorbar), while each model is*

indicated by a different linestyle (see legend). Faint lines indicate where the correlation is not significant (p>0.05). The correlations are shown for Organics (Org, a and e), sulfate (SO4, b and f), Nitrate (NO3, c), Black Carbon (eBC880/BC, d and h) and sea salt aerosol (SS, g)."

- *Figure 7 has been split into two panels and moved to the supplement.*

Specific comments:

[L3] "Wet" rather than "et"

Thank you, this has been corrected.

[L5] can do general circulation models (GCM) in brackets here rather than later

This is a good point. However, we have here followed the convention that the abstract is a stand alone text and GCM is not used again in the abstract. So we prefer to not define the abbreviation here.

[L10] new particle formation (NPF) in brackets here rather than later

See reply above.

[F1L1] "Illustration" not "Illustratration"

Thank you, this has been corrected.

[F1L6] "points" rather than "point"

Thank you, this has been corrected.

[F1L11] Reference Khadir et al (2023) at this point as this plot type is taken from that paper

Good point.

[L50] "Borne" not "born"

Corrected, thank you.

[L62] A reference to a paper explaining why precipitation formed via ice formation is expected to scavenge less than liquid precipitation formation

Thank you for noticing. On further inspection, this sentence needs some specification. We have revised it to read: "Precipitation formed via ice formation may be expected to scavenge less particles in-cloud than liquid precipitation formation."

This is followed by the explanation with reference to the WBF process and reference to (Browse et al., 2012) and (Zieger et al., 2023) of which (Zieger et al., 2023) has been added in this revision.

[L120] A suitable reference for each of the different sites related to NPF or precipitation

This sentence refers to results from Khadir et al. unless we are misunderstanding the reviewer here. It is therefore our opinion that it would be confusing to add references in this sentence. We have however added another sentence at the end of the paragraph (L126 in the submitted version).

> *"For further discussion of NPF at the two other stations in SMEAR-II and Zeppelin, see e.g.* (Dada et al., 2017) *and* (Heslin-Rees et al., 2025) *"*

[L126] This second hypothesis contradicts the one by Zhu et al. Which one is more plausible or are both plausible in different scenarios? A further comment on this contradiction would be beneficial.

Yes, it would indeed be good to know which one is more plausible. However, we do not believe that there is a scientific consensus at the moment and would prefer to not make such comments without a strong argument behind it. We have, however, made it clear that these are competing hypothesis by revising L124 to:

> "A competing hypothesis is that precipitation injects ozone […]"

[L128] Linked to F1L11 but this should be in figure caption aswell/alternatively

Good suggestion. The sentence "which is an adaptation of a figure from Khadir et al. (2023)" is now moved to the caption of figure 1 in line 5 "Panel b, which is an adaptation of a figure from (Khadir et al., 2023), shows how these […]"

The original sentence now reads:

"This is illustrated in Fig.1b which shows the correlation between the PNSD measured at Hyytiälä measurement station (Finland) in spring […]"

[L131] ",for example,"

Thank you, this is corrected.

[L171, L204, L244] Why these relaxation times? Further detail would be useful.

Relaxation time is at the core of the nudging technique and tells you "how strong" the nudging is essentially (see e.g.(Kooperman et al., 2012)*). A forcing is added to the prognostic equation for example horizontal winds (X) which draws the models value (X_M) towards the prescribed value (X_P) with some relaxation time parameter (τ) as such*

$$\frac{\partial \mathrm{X_M}}{\partial \mathrm{t}} = \frac{X_M - X_\mathrm{P}}{\tau}.$$

To make this clearer, we have added the following sentence to the model description at the start of section 2.1:

"All models are run with nudging (see e.g. Kooperman et al., 2012) to ERA-Interim reanalysis data from the European Centre for Medium-Range Weather Forecasts (Berrisford et al., 2011), meaning that their meteorology is forced towards that reanalysis product and hinders the model from deviating too far from it."

[L191] "area" not "are"

Thank you, this is corrected.

[L209] "scavenging" to "scavenging"

Thank you, this is corrected.

[L229] "includes" not "include"

Thank you, this is corrected.

[T2L4] "Jun-Aug" instead of "Jun-Jul"

Thank you, this is corrected.

[T2L11-13] Might read better if you swap Dry and Dry-to-Wet around as then goes through the year.

Thank you, this is corrected.

[L351] swap S1 and S2 around

Thank you, this is corrected.

[L418] "at least in" not "just at least"

Thank you, this is corrected.

[L427] "S7))" not just "S7)"

Thank you, this is corrected.

[L429] "10-30nm)" not "10-30nm"

Thank you, this is corrected.

[L468] remove anyway

Thank you, this is corrected.

[F7] Night needs to clearly be a star in the legend

Thank you, this figure is now split into two panels, so that should solve the issue.

[L496] "one can see here" not "one can here see"

Thank you, this has been corrected.

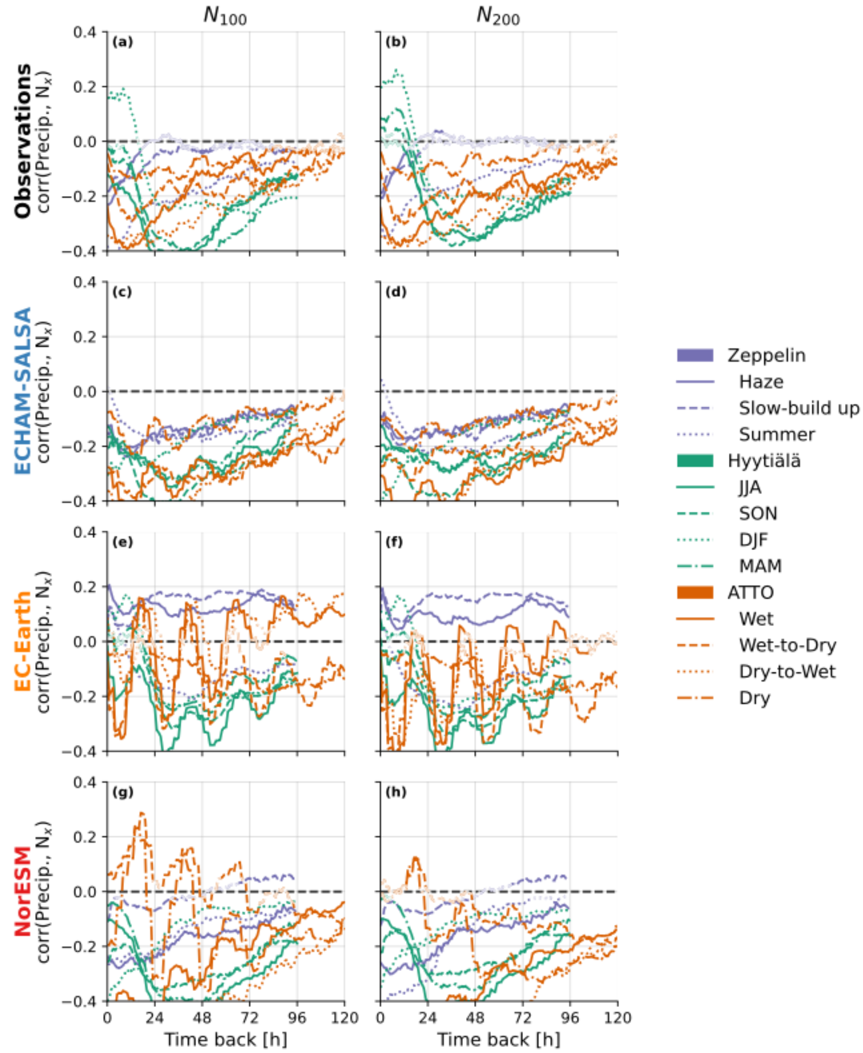
[L497] need something between "stations, the models" like a "whereas" or change the sentence structure

Thank you, good point. However, the sentence has been edited out in the revisions.

[F8] Could you differentiate between the seasons? Also address the white out effect of some of the lines

In Fig. 8 and the others like it, the faint lines indicate areas where the correlations are not significant. We have modified the caption: "Spearman correlations between precipitation rate and (first column; a, c, e, g) N100 and N200 (second column; b, d, f, h) for observations (top row, a and b) and models (c-h). Each line represents a season at a station, with color indicating the station and linestyle indicating the season. Faint lines indicate where the correlation is not significant (p>0.05)."

Secondly, we have added different linestyles to indicate the seasons in these figures. They now look as below:



[L526] "Fig S22" not "Figs S22"

Thank you, this has been corrected.

[L530] "to be more" not "to more"

Thank you, this has been corrected.

[L581] struggles to reproduce two of the models, not NorESM and observations instead of current wording

Thank you, this has been corrected.

[L586] "XGBoost" not "XGboost"

Thank you, this has been corrected.

[L618] "72-80h" not "72-8h"

Thank you, this has been corrected.

[L658] “to be a lower” not “to be lower”

Thank you, this has been corrected.

[L673] this needs fixing to make sense (remove the NPF?)

Thank you, this now reads:

“Furthermore, if the appearance of the smallest particles after rain is driven by ozone injection with downdraft”

[L682] “much too large” to “too many”

Thank you, this has been corrected.

[L692] “Is” not “are”

Thank you, this has been corrected.

[L699] Need to make it more clear that XGBoost agrees with overall correlations but attributes different reasons – some more explanation would be useful

Fair point! We have re-written the entire XGBoost section to highlight agreement or non-agreement with the correlation analysis.

References

Browse, J. *et al.* (2012) "The scavenging processes controlling the seasonal cycle in Arctic sulphate and black carbon aerosol," *Atmospheric Chemistry and Physics*, 12(15), pp. 6775–6798. Available at: https://doi.org/10.5194/acp-12-6775-2012.

Dada, L. *et al.* (2017) "Long-term analysis of clear-sky new particle formation events and nonevents in Hyytiälä," *Atmospheric Chemistry and Physics*, 17(10), pp. 6227–6241. Available at: https://doi.org/10.5194/acp-17-6227-2017.

Heslin-Rees, D. *et al.* (2024) "Increase in precipitation scavenging contributes to long-term reductions of light-absorbing aerosol in the Arctic," *Atmospheric Chemistry and Physics*, 24(4), pp. 2059–2075. Available at: https://doi.org/10.5194/acp-24-2059-2024.

Heslin-Rees, D. *et al.* (2025) "Drivers governing the seasonality of new particle formation in the Arctic," *Aerosol Research Discussions*, pp. 1–47. Available at: https://doi.org/10.5194/ar-2025-11.

Khadir, T. *et al.* (2023) "Sink, Source or Something In-Between? Net Effects of Precipitation on Aerosol Particle Populations," *Geophysical Research Letters*, 50(19), p. e2023GL104325. Available at: https://doi.org/10.1029/2023GL104325.

Kooperman, G.J. *et al.* (2012) "Constraining the influence of natural variability to improve estimates of global aerosol indirect effects in a nudged version of the Community Atmosphere Model 5," *Journal of Geophysical Research: Atmospheres*, 117(D23). Available at: https://doi.org/10.1029/2012JD018588.

Zieger, P. *et al.* (2023) "Black carbon scavenging by low-level Arctic clouds," *Nature Communications*, 14(1), p. 5488. Available at: https://doi.org/10.1038/s41467-023-41221-w.