Authors' reply to the reviewer's comments. Comments by the reviewer are written below in **bold**, our reply in normal text and modifications for the manuscript in *italic*.

One general modification to the manuscript is made, we have changed the text of competing interests (L512) to "At least one of the (co-)authors is a member of the editorial board of Atmospheric Chemistry and Physics" according to the remarks by the editor.

## Despite the authors have answered the reviewer concerns, the reviewer still have a major concern about one of the general comments.

First, this is an editorial decision, but I would like to point that, despite we agree there is a demand of studies on this topic, this work applies a model (already published before) to data (already published before; e.g., Zha et al., 2024) in order to model growth rates (considering vapour properties already used before) and compare them with measured ones. The authors stated that "We also confirmed the major role of SVOCs and sulfuric acid on particle growth, along with smaller but still notable contribution of ELVOCs", however this is not a new finding because this comes from applying the model (this model only depends on vapours concentrations and not on pre-existing particle concentrations) to already published data. In this sense, despite this manuscript provides new useful information, from my point of view, results and conclusions of this manuscript are of more limited scope than in research articles.

The Southern hemisphere high ALTitude Experiment on particle Nucleation And growth (SALTENA) campaign has been notably fruitful, resulting in several publications (Zha et al., 2023b; Scholz et al., 2022; Zha et al., 2023a; Bianchi et al., 2022). These studies, derived from different observational periods and using different instruments, address diverse research topics. Notably, the gas-phase organic compounds detected by the FIGAERO-CIMS have not been covered in previously published papers from this campaign.

Zha et al. (2023b) only published particle-phase FIGAERO-CIMS data for only one day, while our study used gas phase FIGAERO-CIMS data. While that paper focuses on the long-distance transport of oxidized compounds (with 4-5 carbon atoms) in the tropical free troposphere air from Amazonia, our study primarily investigates new particle formation and growth in the boundary layer. While Zha et al. (2023b) focuses on the measurement in January 2018 during the austral summer (wet season), we reported the NPF events in April (the wet-to-dry transition period) and May (austral winter, dry season) 2018. In Zha et al. (2023b), the measurement was performed with a nitrate CI-APi-TOF that predominantly measures ELVOCs. For comparison, it presented in the SI one event during the night of 22 April 2018. In this figure, only the mass fraction of the C4-C5 organic compounds in the gas-phase (again with the nitrate CI-APi-TOF) and particle-phase (with the FIGAERO-CIMS) was presented. In our manuscript we present and use the volatility distribution of the measured gas-phase organic vapors, which is an approach not published before from the SALTENA campaign. We would also argue, that by looking gas-phase measurements alone it would not be possible to determine the important compounds affecting particle growth. More detailed analysis is needed, which is provided in this manuscript by MODNAG model and volatility information of organic precursor vapors.

In our manuscript we compared the modelled particle growth and the observations to test whether the measured organics and sulfuric acid are enough to explain the observed particle growth, which, clearly, is a new finding. The reviewer states that our conclusion is not new because it comes from applying the model (which only depends on vapour concentrations and not on pre-existing particle concentrations) to already published data. Firstly, as explained below, it doesn't need to include all processes affecting the gas-phase

concentration as input data. Secondly, we don't see any concerns regarding the loss of novelty when we applied a well-established and published model to our unpublished data. Hence, we still argue that our manuscript contains enough novelty to be published as a research article rather than as a measurement report and that it contributes to advance our understanding on components taking part in particle growth on high altitudes.

Lastly, we would like to apologize for a typo in our last reply. In sentence "We also confirmed the major role of SVOCs and sulfuric acid on particle growth, along with smaller but still notable contribution of ELVOCs", we meant LVOCs to have a major role in the particle growth, not SVOCs, whose contribution we showed to be almost negligible. This typo was only in our reply for the reviewer comment and in the manuscript (on line 381) this had been written correctly.

However, the reviewer main concern is about the second and third major comments. The authors stated that "model uses measured gas phase concentrations which already are affected by condensation sink". To my knowledge, gas phase concentrations are affected by 1) condensation sink and 2) formation and growth rates. Actually, the precursor vapours are consumed by these three processes 1) condensation in pre-existing particles, 2) formation of new particles and 3) the growth of newly formed particles. Thus, the reviewer here don't really understand the argument that the authors provide about this comment. Why are concentrations only affected by condensation? Furthermore, I agree that "the size dependence from the model simulations would also increase the sensitivity of the results to assumed vapor properties" but if the condensation into pre-existing particles is high, then the comparison with the model is not accurate at all.

The reviewer is correct that all these processes, i.e., formation and growth of the new particles and condensation to larger pre-existing particles, affect the precursor vapors. However, our reasoning still applies, i.e., we only need the measured gas phase concentration for each simulation steps, instead of calculating the changes of gas phase concentration due to multiple source and sink processes for the next simulation step. By using the measured gas phase concentrations through the simulation, we are simulating how a particle would grow in that ambient surrounding gas phase (manuscript line 174-176). Important point to note here is that we do not use the measured gas phase concentration in the model only as an initial value at the time when the particle starts to grow. Instead, in our model for each time step in the simulation we use real-time and time-dependent gas-phase concentration measurements as an input. The measured gas phase data with 30 min resolution was interpolated to the time resolution required by the model for this (manuscript line 220-222). These measured gas phase concentrations have been affected by the processes mentioned by the reviewer and by any other processes that may affect the concentrations. As gas-phase concentrations are taken at each time step from measured data, the changes in gas-phase concentrations are not calculated in the model and are thus not dependent on calculated condensation rate. Hence, although condensation sink to particles of any size or particle formation are not directly considered in our model, they are indirectly counted for by using measured gas-phase concentrations of condensing vapors. This is one of the advantages of our rather simple model, that can capture the growth of newly formed particles despite other factors affecting precursor vapors.

## Finally, about the minor comment of Fig. 1, I still have same question, why error bars are only in one direction? I understand the process that the authors have followed, however, if you have applied different tests or retrievals, why the "good" GR is not the mean/median and is instead one of the "extreme" values?

It is important to notice that the error bars in our figure do not describe an error in any statistical sense, but different GRs obtained from different subsets, (this is explained also in the manuscript on line 318-319). Usually, when this method to calculate the GR is used, only one set of mode peak diameters is selected and used for determining the GR for the event (e.g., Dal Maso et al., 2005; Nieminen et al., 2014). This method

includes a step where the mode peak diameter points to which the line is fitted are selected. For instance, the mode fitting may result sometimes mode peak diameters that do not describe the growing modes properly and thus need to be excluded from the fitting of the line to obtain GR. At this point the researcher makes the decision of which datapoints to include in the line fitting for the GR based on visual inspection. The markers in our Fig. 1 describe the GR values which are obtained from the set of mode peak diameter data points that were evaluated by eye to describe the event best, i.e., these correspond to how GR values with this method are typically reported. However, as we have stated in our manuscript, in our study most of the events were not "smooth" enough to straightforwardly define one set of mode peak diameters that would perfectly describe the event) that seemed to possibly also describe the growing mode and fitted a line to also these, obtaining one to three additional GR values that may describe the growth. To be exact, together there were 14 event days for wich GR was calculated, of which for one day we only selected one set of mode peaks, for six days two subsets and for the rest seven days tree to four subsets.

The main reasons for selecting the addition subsets of mode peak diameters for GR calculation were 1) the difficulty in determining which time period (how long) one should consider for the GR line fitting and 2) the challenge in selecting the correct mode peaks data points to describe the growth when there was fluctuation or e.g. two consequent events during a day. In our previous reply to the reviewers we wrote by mistake that the "best" subset gave always lowest or highest GR. We apologize for this. For two out of the seven events with more than two subsets the "best" subset gives the smallest GR and also for the remaining five cases the GR for the "best" subset is very close to the smallest GR value (difference < 0.3 nm/h), due to which the error bar is mostly not visible under the marker in Fig. 1. When looked closely this can be seen in one of the events (lower of the not-filled circle markers in the Fig. 1). Out of the six events with two subsets, there are two events where the "best" subset gives largest GR (filled blue and green circles at the bottom of the Fig. 1), and four events where the "best" subset gives lowest GR. Altogether this means that there are 11 events where the "best" subset gives lowest or close-to-lowest GR value and only two events where it gave the largest GR value out of the different GR fits for the event. This is reasonable, since the "best" subset was always also the subset that was obtained over longest time period and the rest were taken from some shorter parts of the event. When selecting a shorter period of the event, one is less likely to include periods when growth was so slow that the diameter seems to be changing little or not at all, and thus such shorter period tends to give higher GRs compared to a longer period. For ten of the events one of the chosen subsets was the first monotonic growth period of mode peaks. From this subset the obtained GR was often quite different compared to other subsets, which were relatively similar to each other.

As described in the manuscript on lines 254-258, the "best" subset describes our best estimation of the particle growth and with the error bars we have aimed to highlight the uncertainty of this estimation, due to the "unsmoothness" of the events. We argue that using mean or median value in these cases would give too much weight on the extreme value (now described by the large error bars in the figure) and not describe the best estimate of the GR properly.

References

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