Based on ~6 years of measurements collected at the ATTO station (Feb. 2014 – Sep. 2020), in the Amazonian forest, the study by Franco et al. investigates the occurrence of particle growth events in the sub-50 nm size range as well as the atmospheric conditions which favour the process. In addition to highlighting an interesting data set, this paper is well written, and I would therefore recommend it for final publication in ACP. Since the growth events have already been reported in different studies, I would however suggest to better highlight what distinguishes this new study from the previous ones, as well as the importance/newness of the results associated with it; indeed, even if it is clearly mentioned that there are gaps in the understanding of the process, some sentences in the paper suggest that a number of elements are already known (e.g. P5, L1-3; P11, L26-29). I also have a series of comments below that I would suggest to address before final publication.

P1, L13: I would recommend (actually throughout the manuscript) using scientific notation to report CS values.

P2, L23: Do the authors mean that the occurrence of growth events in Amazonia is on average less frequent than "classical NPF" at other PBL sites? The wording of the sentence is in my opinion a bit confusing as it gives the impression that it compares the frequency of occurrence, between Amazonia and other sites, of a process that seems characteristic of Amazonia (L21 "Amazonian banana plots", L23 "these characteristic events").

P4, L10-12: If I am not mistaken, among the listed references only Kirby and co-workers report results from laboratory experiments; Rose et al. (2018), in contrast, report complementary observations from "the real atmosphere", and Zhao et al. (2020) uses a combination of measurements and model simulations.

P6, L10-11: "sampling from the 60 m inlet at the 80 m high so-called triangular mast". It is not very clear to me, does it just mean that the particle sampling is not done at the top of the mast but at a lower height?

P6, L20: I would suggest to replace "almost" by "more than" since the data set covers more than 6 years.

P7, L24-25: Can the authors clarify what they mean by "the same fitting routine as for the first (dominant) mode"? Do they mean that once the second (and possibly third) mode size range is identified, the mode diameter identification procedure described in 1. (P7, L18-20) is applied? Also, can the authors say a few words about the criteria for deciding whether a PNSD is best described by 1, 2 or 3 modes? Is this test part of 4. (P7-8, L28-L4)?

P8, L15-16: "moving average with moving windows of 25 minutes for time and 20 nm size for particle diameter". I wonder about the choice of values used for the smoothing algorithm:
- the size range of interest extends over 40 nm, between 10 and 50 nm (it is in fact restricted to 10-40 nm for the application of the procedure from Kulmala et al., 2012): is the 20 nm window used for the smoothing average not too wide, and does not impact the monitoring of the process evolution through this relatively restricted size range?
- similarly, is the 25 min window not too large for the description of a phenomenon that (may) have a relatively sudden character?

P9, L15: How was the threshold on R^2 selected (> 0.6)?

P9, Meteorological parameters: Outside the period January 2019 - September 2020, the measurements of the different meteorological variables involved in the calculation of the equivalent

potential temperature (i.e. T, p and RH) were not made at the same height (55, 55 and 81 m, respectively). On the other hand, between January 2019 and September 2020, the measurements of these same variables were made at a significantly higher height (321 m; especially compared to the measurement height of the SMPS). Doesn't this have an impact on the analysis? In particular, I wonder about the possible existence, in the vicinity of the forest canopy, of a very fine scale tropospheric stratification phenomenon, like the one observed by Zha et al. (2018) over the boreal forest?

P11, L19-20, 23-24: Since the diameters that are reported are for average distributions, I would suggest clearly indicating "on average", or “(mean)”, same as for the description of Fig. 5.

P12, L15: In light of the results of this work, which suggest that there are growth events that may not be directly related to characteristic wet season conditions such as rain / deep convection events, I would suggest changing the sentence slightly, replacing “indicating” by “suggesting”. Related to my general comment, this would contribute to give more weight to the results of the present study.

P13, L2: Isn’t the entire measurement period between February (and not April) 2014 and September 2020?

P13, study of CS_{growth}: As proposed, the definition and analysis of CS_{growth} does describe the decrease in CS related to a decrease in the concentration of > 50-100 nm particles visible at the event onset, but CS_{growth} also includes the contribution of the event itself to CS; therefore, CS_{growth} is likely ultimately impacted by the strength of the event. Based on Fig. 10, it seems to be the case in particular during “deep convection events”, and it is more broadly suggested by the similar “trends “obtained for CS_{growth} and GR , which both seem to represent a measure of the strength of the event. If the objective is to study the conditions that favor the occurrence of events, why not look (at least in addition to the analysis of CS_{growth}) at the evolution of the CS related only to particles >50 or even 100 nm, i.e. associated to Aitken / accumulation mode particles?

P17-20, Diurnal trends:

- Why did the authors not also present for all the variables shown in Fig. 7, in the same way as for N_{50}, the median daily variation observed on event days? Even if the number of event days is limited (and the corresponding statistics must therefore be considered with caution) this might help to illustrate what distinguishes in particular these days from non-event days during the wet season.
- P18, L11-12: “This suggests that $CN_{<50}$ are injected into the PBL by rainfall events during the late afternoon and early night and last until mid-morning”: beyond lasting until mid-morning, the particles assumed to have been injected in the late afternoon and early evening show an increase in concentration (already between 15:00 and 00:00 LT but more importantly between 00:00 and 09:00 LT). Can the authors comment on this increase? Is it related to the dynamics of the PBL described in L25-31? If so, the link should be more clearly established. Also, while the decrease of $N_{<50}$ is mentioned in the Summary and conclusions, it is not discussed in Sect. 3.4, whereas I think this would be interesting for a more complete description in the results section dedicated to the analysis of diurnal cycles. Finally, still concerning the $N_{<50}$ analysis, I would not say that the concentrations observed on event days are *significantly* higher. The difference on the medians is most pronounced between 04:00 and 14:00 LT but it does not exceed ~35 cm^{-3} (corresponding to a multiplying factor of 1.6 compared to all data), and it is less than 20 cm^{-3} on the rest of the day. Considering that concentrations are moreover likely affected by uncertainties, I would suggest to slightly balance this observation.

- With the exception of P_{ATTO}, the analysis of the different meteorological variables shown in Figure 7 is relatively brief, and I find it overall decoupled from the analysis of N_{50} and growth events (I find that the link between the different observations is better established in the conclusion section!). For example, if the occurrence of the growth events seems to be often connected to a rainy episode, there seems, in addition, to be a strong link between the onset of the growth process and radiation; this should for instance be highlighted as a support for "daytime events are directly influenced by sunlight".
- This leads me to a broader question: the conditions that favor the occurrence of a growth event are implicitly favorable, in the first place, to the "appearance" of <50 nm particles. However, are there times when the appearance of <50 nm particles is not followed by the growth of these particles? Such days would allow the identification of conditions that are favourable in particular to the growth phase if this is not systematic.
- P18, L9: I assume that the numbers in brackets correspond to quartiles; this should be specified at first use.

P19, L3 - P20, L1-5: With the exception of light, which is by definition specific to daytime events, the list of factors mentioned to explain the difference between daytime and nighttime events is not clear to me. In particular, as further illustrated by the analysis of the different event groups in Sect. 3.5, there is a likely role of weather (and in particular convection/downdraft) in the case of groups G1-G3, i.e. including both daytime and nocturnal events; moreover, the entrainment of particles from the upper troposphere into the PBL is mentioned for nocturnal events, but it is this mechanism that is, in one way or another, at the origin of the transport of particles also for daytime events, isn't it? And isn't the contribution of biogenic sources mentioned for the nocturnal events also valid for the daytime?

P23: For the sake of consistency in the abbreviations used, I would suggest changing "BL" to "PBL".

P23, L14: To avoid any confusion, I would suggest to change Di to another acronym, as Di is already used in Sect. 2.4 in the description of the fitting process (where it corresponds to the diameter of mode i).

P23, L15: "The results show a clear difference": I would suggest to slightly balance this assertion with respect to Tir as, in a first approach, all groups (with the exception of G3 maybe) have relatively comparable median Tir, at least all belonging to the same range corresponding to mixed sky conditions.

P24, L7-8: When they speak of a stable Di, I assume that the authors mean that Di varies little from one event to another; if this is indeed the case, I would suggest not using the expression "remains stable", which, in my opinion, gives the impression of referring to a temporal evolution of the diameter. I would for example suggest "while the events belonging to G1 and G4 display comparable median Di".

P24, L14: Change "15 LT" to "15:00 LT" for consistency in the notation of times.

P24, L20: The value reported for G3 median Di in the text (24.3 nm) is slightly different from that reported in Table 2 (24.6 nm).

Fig. 3: Change "a particle growth event" to "2 particle growth events" in the figure caption.
Regarding the color bar, I would suggest indicating the concentrations in logarithmic scale instead of showing the logarithm of the concentrations, to facilitate the interpretation of the figure. If the authors

wish to keep this representation, however, the unit should be corrected (log10(particle concentration) is not in cm^{-3}). The same comment applies to similar figures.

Fig. 7: Ticks on the x-axis are not located at the same place in all panels, which makes it difficult to read the times from the lowest panel.

Fig. S3: abbreviation “abr” for April should be changed to “apr”.

References:

Zha, Q., Yan, C., Junninen, H., Riva, M., Sarnela, N., Aalto, J., Quéléver, L., Schallhart, S., Dada, L., Heikkinen, L., Peräkylä, O., Zou, J., Rose, C., Wang, Y., Mammarella, I., Katul, G., Vesala, T., Worsnop, D. R., Kulmala, M., Petäjä, T., Bianchi, F., and Ehn, M.: Vertical characterization of highly oxygenated molecules (HOMs) below and above a boreal forest canopy, Atmos. Chem. Phys., 18, 17437–17450, https://doi.org/10.5194/acp-18-17437-2018, 2018.