The study by Baalbaki and co-workers presents one year of measurements performed at a rural background station in Cyprus, with the aim of documenting the occurrence and characteristics of new particle formation (NPF) at this site located in the Eastern Mediterranean and Middle East (EMME) region and influenced by air masses of various origins. The paper is well written and presents new and interesting observations of the process in a poorly documented area. I therefore recommend the publication of this study in ACP, but I suggest to consider the comments listed below prior to publication. These comments mainly concern the main text, but, when necessary, the abstract and conclusion should be modified accordingly.

Comment 1: P1, L19: "*located at the crossroads of three continents*": In addition, I would mention explicitly (but very briefly!) the diversity of the air masses sampled at this site (marine vs. continental, desert vs. anthropized vs. forest areas) that results from this particular location, because I think it is an important specificity of the site that should be underlined already in the abstract.

Comment 2: P3, L97: "*which is the key gas phase precursor for cluster formation*": I would suggest modulating this assertion (and also that on P14, L535), since there are certain environments where this species does not, a priori, play a determining or limiting role in the early NPF process, and this seems to be the case in particular at CAO (P15, L580: "*sulfuric acid was not the limiting factor for NPF occurrence*")!

Comment 3: P4, L113-114: "*it is distant from any major pollution sources*": I would specify here "local sources", since it is indicated a few lines later (L119) that the site is frequently under the influence of air masses which transport pollutants from Turkey and Europe.

Comment 4: P4-5, L141-142: Were the authors able to evaluate the impact of this setup modification on the continuity/comparability of the measurements? Also, in addition to the effect on the CPC activation efficiency that is discussed here, RH of the sample flow also certainly affects the detection efficiency of the PSM itself (as a result of water-DEG interaction, Kangasluoma et al, 2013).

Comment 5: P5, L161, nCNC data handling: I would suggest specifying the number of size bins used in the main text. Also, I wonder why such a high number of classes is needed for this study, and, more importantly, I wonder about the relevance of such fine size classes, which width is of the same order or less than the uncertainty on particle sizing related to the particle chemical composition and/or charge (Kangasluoma et al., 2014; 2016). Wouldn't a single class, which could be used both in the calculation of $J_{1.5}$ and for cluster concentration analysis (e.g. 1.5-2.4 nm), be sufficient and less uncertain?

Comment 6: P5, L180, full PSD:

- Unless I am mistaken, the final temporal resolution of the composite PSD is not indicated, could the authors please specify it? It is indicated in Section 2.3 of the Supplement "*Here, we mainly use 1 hour resolution data for the presented analysis*": if this resolution is indeed used, isn't it too coarse to capture the rapid changes in cluster/particle concentration during NPF events? If the constraint comes from the PSM data, wouldn't the use of a single bin make it possible to increase this temporal resolution with more confidence?
- It is not clear to me at what stage the correction for particle hygroscopicity was applied: to the composite PSD after correcting NAIS concentrations? In this case, it would mean that the NAIS correction was made by comparing ambient PSD from NAIS and dry PSD for SMPS?
- Based on the values reported in the Supplement, it seems that seasonal means of kappa have been used to evaluate the particle HGF. I imagine that beyond the seasonal variability, there may be a variability of kappa related to the origin of the air masses; in the study of Holmgren et al. (2014), it was for instance shown that such variability was not obvious, but has it been investigated in the case of CAO?

Comment 7: P6, L206-207: "*spectrums of total particles (both neutral and charged) are usually less ambiguous to classify than charged particle spectra (ion mode of NAIS)*": could the authors explain why the total particles spectra less ambiguous to classify than that of ions? Is it related to the strength of the events? If so, this makes me wonder why the GR calculation is based on measurements of charged particles (P7, L224); also, why the negative ion mode data were specifically chosen for this calculation (instead of positive mode data, or both)?

Comment 8: P8, L255-256: "*but also includes the formation pathway via stabilized Criegee Intermediates*": I do not think it is the case for the proxy developed for rural sites and recalled in Eq. 6.

Comment 9: P8, L257: Air mass origin analysis: Although it has been described in previous studies, I would suggest to say a few words on the method used for the air mass classification, and especially on the definition of the geographical sectors, which are otherwise numerous!

Comment 10: P9, Fig. 2: Although the variability on the measurements is shown in Fig. 2, it is not described, and seasonal variations appear to be discussed primarily on the basis of monthly medians. I think it would be interesting to complete/modulate the conclusions in the light of this variability, which "unusual" behaviour in some months is also worth discussing (e.g. variability of the cluster concentration in February). I would also suggest adding a grid to make the figure easier to read. These recommendations also apply to Fig. 10, 11 and 12 and their analysis.

Comment 11: P9, L291-292: "*cluster mode and nucleation mode particles had roughly a similar pattern, with the highest concentrations during the spring followed by the autumn and a clear drop during the summer*": as indicated, there are "*rough*" similarities in the seasonal variations of the cluster/particle concentration in these two size classes. However, I do not think that the proposed brief description recalled above is appropriate since it does not seem to me to correspond to either situation. For example, fall/winter levels are similar or even higher than spring levels in the case of cluster concentration, while they are closer to summer levels in the case of the nucleation mode concentration. In fact, I think it would be particularly interesting to discuss these differences since it is indicated right after (L292-293) that concentrations in both size ranges "*can be directly linked to NPF activity*".

Comment 12: P9, L296: "*higher emission during spring*": does the authors refer to the emission of particles or gaseous precursors?

Comment 13: P9, L304: "*with the highest values recorded between 9:00 and 15:00 am and the maximum at 11:00*": The maximum concentrations in the two size classes appear at the same time while we would expect a chronology if they are related to consecutive NPF stages: is this only related to the hourly resolution of the data shown in the figure?

Comment 14: P11, L361-362: multiple events: how are these events taken into account in the statistics shown in Fig. 5? Only the first one observed, or the most intense one of the series, each one individually, or all the events are considered as a whole?

Comment 15: P11, L392: "*high dust loading (translated to a high condensation sink)*": did the authors estimate the increase in CS caused by the presence of dust compared to a dust-free day? These "big" particles have a definite impact on the PM but I would be curious to know their real impact on the CS!

Comment 16: P11, L401: particle formation rates:

- I think a figure (same type as Fig. 2, 10, 11 and 12) would make it easier to visualize the variations of the particle formation rates (Table 1 could be kept in the Supplement). To limit the number of figures in the paper, Fig. 6, 7, and 8 could possibly be transferred to the Supplement;
- Also, I do not understand how the values presented in Tables 1 and S4 were calculated. What does "daily" data mean, since it seems that in each case the reported statistics were calculated within the event duration?

- Finally, could the authors comment on the different seasonal patterns which are observed for the particle formation rates at different sizes (maybe in connection with the differences observed between Fig. 2.a and b, see Comment 11)?

Comment 17: P12, L441-442: "*The seasonal variation of the CS followed the seasonal pattern of the accumulation mode particles*": I pretty much agree with that, and I think therefore that in their present form, the descriptions of Fig. 2.d and 10 sound a bit contradictory:

- P9, L296-297: "*The accumulation mode had its maximum during the summer, except during July*"
- P12, L442-443: "*with highest values calculated for winter and spring, and lowest for summer and autumn*"

More generally, I suggest moving the CS discussion to the next section. This analysis currently sits astride sections 3.3 and 3.4, and I think that CS is more a "*driving atmospheric parameter*" of NPF than a "*specific parameter*" of the process.

Comment 18: P13, L450: I would suggest to explicitly refer to <u>particle</u> formation rates (also on P14, L534).

Comment 19: P13, L457-460: A possible explanation for the fact that the CS is not systematically lower on NPF event days could be that, as observed at some mountain sites (e.g. Sellegri et al. 2019), the sources and sinks (i.e., CS) of NPF precursors share the same origin, and in this case the CS is not necessarily a limiting factor.

Comment 20: P13, L486-487: "*NPF occurred largely at lower wind speeds and local north easterly winds, which is the direction where the main agglomerations and livestock farming lands are situated*":

- The use of box plot does not seem to me perfectly adapted to the analysis of wind direction, especially since the values close to the extremes 0 and 360° correspond to situations that are in fact similar. I think that the use of wind roses such as those shown in Fig. S11 is much more appropriate;
- Considering the variability of wind speed, it does not seem obvious to me from Fig. 11.e that weak winds are particularly favorable to NPF;
- Regarding wind direction, I would suggest to slightly rephrase this sentence. I might be wrong but it means for me that the majority of the events take place in north easterly wind, whereas there are months, and especially the months with the highest frequencies (March-May), when both sectors (i.e. north east and north west) seem to be almost equally represented on event days. Based on my understanding, I would say that NPF occurs in both north easterly and westerly winds but with a probability of occurrence which seems to be definitely higher in north easterly winds;
- What do the authors imply by indicating the presence of anthropogenic activities in the northeast sector? That there could be a local influence of these activities on the occurrence of NPF? Did the authors analyse whether there was a significant difference in the type of event (i.e. bump vs. class I or II) in the northeast and northwest sectors which could support this hypothesis?

Comment 21: P13, L494-495: In light of the reported observations, I would slightly balance this statement and rather say that SO_2 / H_2SO_4 "*cannot explain <u>alone</u> the seasonal pattern of NPF*".

Comment 22: P14, effect of air mass origin:

- L511: shouldn't it be *7* source regions?
- L513-514: shouldn't it be *month<u>s</u>*?
- Although the marine sector does not appear to be one of the most frequent source regions in Fig. 13.b, I would assume that due to the insular nature of the station, "marine conditions" are

part of the history of a certain number of air masses sampled at CAO (as also suggested on P4, L121), and could therefore often influence the occurrence of NPF at this site. I think that a couple of sentences on this subject could enrich the discussion.

Comment 23: P14, L525-527: "*The increase in PM levels during these months could be a limiting factor for NPF. Indeed, accumulation mode particles and thus CS were the highest during the summer (except for July)*". Based on Fig. 10, it is not obvious to me that the CS is higher in summer, and this is not what is indicated on P12, L441-443 (e.g. August vs February-May, also see Comment 17). I also wonder about the sequence of these two sentences ("*Indeed*") since no clear relationship between the CS (based on sub-700 nm particles) and the occurrence of NPF could be evidenced at CAO.

I would therefore suggest to remove the second sentence and simply mention that the actual CS could be higher in summer during episodes of high of PM levels, possibly contributing to lower NPF frequencies and less frequent particle growth during this time of the year.

Comment 24: P15, L555: I fully agree with this hypothesis, which explains why the individual analysis of the different atmospheric variables does not make it possible to highlight a preponderant role of one or the other of these variables. Without necessarily considering the use of "heavy" statistical approaches, have the authors tried to study some combinations of these variables, which behaviour could be more contrasted between event and non-event days and give clues about their combined role in the occurrence of NPF?

Comment 25: P27-28, Figs. 10-12: I would suggest to move the gray bars in the background to increase the clarity of the figures.

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