

Interactive comment on “Influence of vegetation on occurrence and time distributions of regional new aerosol particle formation and growth” by Imre Salma et al.

Anonymous Referee #1

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The manuscript "Influence of vegetation on occurrence and time distributions of regional new aerosol particle formation and growth" by Salma and coauthors reports multi-annual data of new particle formation (NPF) in the Budapest area and seeks causal correlations between NPF frequency and different environmental variables by multivariate statistical analysis. Besides common meteorological quantities and air quality parameters the authors also investigate possible connections to vegetation activity. Overall, data from seven full years between 2009 and 2019 are presented showing a clear annual trend in NPF frequency that is known as well from other sites in diverse climate zones. In this study, however, explicit focus was put on the effect of vegetation in connection with NPF. To this end, satellite-based measurements for the

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start of spring, green-up duration and leaf area index were modelled for the wider Budapest area considering major plant types of the region. While I believe that the manuscript clearly fits the scope of ACP and adds new ideas to the topic, it still needs major improvement before final acceptance. Below is a list of scientific and technical comments that the authors should consider when revising the manuscript.

Scientific comments:

One of my major concerns relates to the data from 2012 which were measured near city. Apparently, these are the only data from this region in this time range. However, when looking at the individual graphs (SI) one can see a clear difference between near city and city centre. The near city location exhibits lowest NO₂ concentrations, lowest T and WS values and highest O₃ which all influence NPF activity. Interestingly, the annual mean NPF occurrence frequency (Fig. 2) has the highest value near city (Y2012). What is the point of showing these data in Figs. 2 and 4? As far as I understand, they're not included in Fig. 3, and were not used anyhow according to the statement on line 722. Lines 694-697 even discuss the problem of varying types of oxidizing agents and concentrations with respect to NPF. I think more reasoning is needed why data have been merged here from two different locations. Probably, these data can be skipped without losing any information.

Lines 505-507 discuss joint influence of variables on occurrence frequency by pair wise correlation. To me the question remains whether only correlation is important or also the absolute value of the variables.

Lines 586/587: Who/what determines the limiting ratios (>1.1 , <0.9 , respectively) that favor or disfavor NPF? Are there references for these numbers or other scientific arguments?

Table 3 (bottom of page 22): How was SD determined? Given the smooth increase of curves in Fig. S1, SD could probably be chosen narrower ($< 10?$). Connected to this: Why are error bars for SoS bigger than for start of NPF (seems to have much

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bigger scatter due to discrete daily appearance)? SoS may vary from year to year but can probably be narrowed down substantially for each individual year which would be much more representative for NPF activity/conditions in the corresponding year. Also related to this point is Fig. 7: the difference between the red and black lines is ~ 20 days, which seems to be in perfect agreement with the difference of SoS and start of the NDVI increase in Figure S1. Wouldn't the cutting point of linear fits through the pre-spring period and spring (increasing NDVI section) give a better estimate for start of vegetation activity?

One remark about the conclusions: in the manuscript the influence of vegetation is solely discussed in relation to spring. The conclusion should also comment on the summer season where vegetation is fully developed and whether the winter minimum in vegetation activity explains the minimum in NPF frequency.

Technical, editorial comments:

Abstract, line 36: in the abstract I'd suggest to write out WS (wind speed)

Introduction, line 84: I'd use parenthesis: "(semi-)continuous"

Line 94: check language: "understanding OF? the role"

Chapter 2, line 118: move "(last five properties)" after the word "vegetation" in line 119.

Figure caption 1, line 189: write out "IGBP".

Line 199: check language, word order

Line 265: the wording "may be indirectly" does not sound convincing. Are there any stronger arguments for the use of the selected parameters?

Section 3, line 336: "overviewed"

Line 342: "units"

Lines 351-357: Quite long sentence, hard to read

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Line 428: “plots”

Line 503: “joint”

Line 535: “climate”

Line 575: “necessarily”

Lines 580/581: I’d suggest to add the actual ratio here to clarify what is meant (as done on line 682 for some variables)

Lines 583/584: check language: ...“ratios for modelled variables”...?

Lines 594/595: Not sure I understand this sentence correctly. What’s the cause, what’s the effect?

Line 602: the table indicates a value of 2.6 (not 1.64).

Line 637: “later”

Line 665: “confirms”

Table 3, bottom of page 22: “SD” appears twice. Maybe add superscripts SoS and GuD to avoid confusion.

Line 718: regarding NPF event occurrence spring peak: add reference to text on page 5 where this is explained.

Lines 720/721: Regarding statement “growth characteristics are different for various vegetation types as just concluded”: I can’t find discussion on this, please refer to position in text.

Fig. 7: font type hard to read

Line 758: “...affect NPF...”

Chapter 4, line 763: “... with AN overall...”

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Fig. S1, line 25: symbol assignment in figure caption (upper/lower triangle) is misleading, reformulate (e.g., triangle pointing upward/downward)

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-862>, 2020.

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