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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Effect of Meteorological Conditions and Atmospheric Composition in the Occurrence and Development of New Particle Formation (NPF) Events in Europe" *by* Dimitrios Bousiotis et al.

Anonymous Referee #1

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A couple of decades ago, a number of studies tried to link meteorological variables and gas phase pollutants with NPF. In some cases, the analysis was concise enough to produce evidence that a certain physical parameter played a role in NPF at a specific site.

Since then, studies - mostly chamber-based - have provided evidence on the ruling mechanisms of nucleation and subsequent growth of newly formed particles. These are mainly related to the concentration of low vapor pressure compounds such as sulfuric acid, or ELVOC as well as agents that could stabilize the former (ammonia,

amines, iodine) suggesting that NPF is dictated mainly by gas phase chemistry rather than meteorology. However, other parameters such as the ones investigated in this study, play a secondary yet important role. Therefore a summary of observations from European, or even better global sites, is always welcome.

During the past 15 years more than 20 compilations of results related to atmospheric NPF have been published, the majority of which are summarized by Kerminen et al., 2018. Even though some of them (eg Kerminen et al., 2018, Lee et al 2019) provide insight on the parameters this study is also focusing on, none has gone been as detailed as the one presented in this work.

Therefore, the compilation of results presented in this work are of interest to the community and would be worthwhile publishing if the manuscript was well written and the analysis provided informative and concise. I am afraid that this is not the case.

After reading the article, I was disappointed not to find any information on seasonality for any of the parameters investigated even though multi year data were investigated. Furthermore the authors fail to deliver any error metric whatsoever (deviation, error, confidence level). The lack of the most elementary statistical analysis was striking.

The other striking feature is the poor use of English and terminology, which I explain thoroughly below The use of English must be improved as there are many sentences that require revision. The major drawback is the generalizations and uncertain phrases used throughout the manuscript. The authors should be concise and specific instead.

As an example in Line 69 it is advised to name the places (exceptions) were NPF is hardly observed. A nice review can be found by Lee et al., 2019 (section 4.8).

Example 2 Line 270: A few sites presented a strong correlation, which in all cases were background sites (either rural or urban).

A few sites (which ones?) presented a strong correlation (nowhere in the manuscript strong, medium weak is defined. The reader has no idea what the author is discussing) which in all cases were background sites (either rural or urban; to the best of my knowledge rural sites are considered as background sites. What do the authors mean?). I assume that the authors are trying to point at urban kerbside sites with this sentence, yet I am not really sure what they mean.

And the paragraph continues

The relation (which one?) found in most cases (how many, percentage?) was positive (does this mean a positive slope? Where is it shown? In which table or graph?) apart from two roadsides (improper terminology) and GREUB, though due to the low (again low is not defined?) R2 these results cannot be used with confidence (and where do the authors draw the confidence line?).

The above lines are just an example of improper phrasing used throughout the manuscript that make it very hard to follow. Similar examples can be found throughout the manuscript.

A major drawback of this work is that many trends/relationships reported are not referred to any table or Figure and hence are hard to follow.

NPF probability sounds to me as if you are trying to predict the occurrence of nucleation events. Based on Line 191 (Equations are not numbered!) a more suited term would be NPF frequency.

The authors should consider adding reference formation and growth rates from other studies in their figures for comparison. I understand that this is not always possible (especially for formation rates) but is for the other two parameters in question.

Table 2 should also include growth rates for this study.

The authors fail to summarize the seasonality of the parameters they are exploring even though they are having multi year data. This is very disappointing.

The statement in Lines 45-46 is not true. Please read Kerminen et al., 2018 for exam-

ple. That work which explicitly states the opposite. I have noted another case (Lines 98-99) in the manuscript where the authors focus on the exceptions (which always exist) rather than the rule giving a very distorted view to the reader.

The introduction is very poor on references.

Lines 107-111 should be rephrased. I cannot make sense of it at all. Lines 82-84. Please mention that increasing temperatures also have a negative effect as they increase the energy barrier the clusters have to overcome to become stable and grow in size.

Kerminen, V. M., Chen, X., Vakkari, V., Petäjä, T., Kulmala, M. and Bianchi, F.: Atmospheric new particle formation and growth: Review of field observations, Environ. Res. Lett., 13(10), doi:10.1088/1748-9326, 2018.

Lee, S. H., Gordon, H., Yu, H., Lehtipalo, K., Haley, R., Li, Y. and Zhang, R.: New Particle Formation in the Atmosphere: From Molecular Clusters to Global Climate, J. Geophys. Res. Atmos., 124(13), 7098–7146, doi:10.1029/2018JD029356, 2019.

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