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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: New particle formation event forecasts during PEGASOS-Zeppelin Northern mission 2013 in Hyytiälä, Finland" *by* T. Nieminen et al.

Anonymous Referee #2

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The Authors present the application of a methodology for new-particle formation events optimized for a site in the European boreal forest and relying on the previous work on stochastic methods presented in Hyvönen et al. (ACP 2005). The usefulness of these methods for field campaign planning is clearly stated in the abstract of the paper. Stochastic methods represent a suitable alternative to chemical models incorporating a more mechanistic representation of new-particle formation. However, they must be optimized for the conditions encountered at the specific sites. In other words, the protocols for new particle formation (NPF) prediction presented in this study are just suitable for Hyytiälä and cannot be extrapolated to other environments. Therefore, the

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results presented in this paper must be treated mainly as an example of the suitability of stochastic models for NPF forecasts. The approach can be attractive for its simplicity (look at the decision tree in Figure 1), but it is actually based on an in-depth data mining work necessary to extract key predictors (Hyvönen et al., ACP 2005). The work presented here shows that, as the key parameters for Hyytiälä can be estimated by weather and chemical weather forecast models, the NPF occurrence can be predicted three days in advance. In the conclusions, the Authors seem to suggest that a similar methodology can be developed for other environments, provided that a sufficiently long record of measurements of the possible key predictors is available. However, this cannot be known with certainty, because it is possible that at other sites none of the simple physical and chemical parameters that are normally measured even at a well-equipped observatory can result to be optimal predictors. I would encourage the Authors to provide a more clear discussion of the applicability of their method outside the boreal forest.

I have some major comments also on the methodology:

a) The work of Hyvönen et al. (ACP 2005) provides a detailed analysis of the best NPF predictors in Hyytiälä concluding that "This resulted in two key parameters, relative humidity and preexisting aerosol particle surface (condensation sink), capable in explaining 88% of the nucleation events. The inclusion of any further parameters did not improve the results notably". Instead here, other variables are taken into account (radiation, air mass origin), while RH disappears from the decision tree (Fig. 1). Why? Is the information on RH implicit in the "cloudy, rainy conditions"? But why not using directly RH instead of parameters difficult to quantify (cloudiness)?

b) If the back-trajectories map the more or less anthropogenic impact on air mass composition, why using them as a separate variable with respect to CS and SO2?

c) For the decision making flow chart in Figure 1, you set thresholds (6.6 μg/m3 of PM, 0.23 ppb of SO2, etc.). How were they chosen?

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Some more major comments about results and conclusions:

a) Figure 1 shows a classification of forecasted events into three categories only. Why Table 3 shows multiple ways to describe the undefined events? Were there different types of undefined events?

b) When reporting the scores of the model, please use clear indexes for the missed, false and total misclassified events, such as in Hyvönen et al. (ACP 2005). Provide these score indexes for NPF and for non-NPF events.

c) The Authors' conclusions about the usefulness of the nucleation parameters NP1 and NP2 for NPF forecasting are unclear.

Finally, some specific comments:

a) Please, add some details on the SILAM model (resolution etc.)

b) Why using 96 h back-trajectories instead of shorter/longer ones?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 2459, 2015.