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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Time-span and spatial-scale of regional new particle formation events over Finland and Southern Sweden" *by* T. Hussein et al.

Anonymous Referee #2

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The article describes the spatial extent of atmospheric secondary particle formation events (nucleation). A huge data base of existing surface-based observations of particle size distributions was evaluated and the results described. The topic and the results are clearly relevant and well within the scope of ACP. However, I have some comments below how the article could be improved.

As a summary, I recommend publication in ACP, once the critical issues have been addressed and the MS been revised accordingly".

1) The literature embedded into the article is not very balanced. Relevant literature on the field from outside Scandinavia seems to be completely ignored. This is in slight

contrast to the obvious global relevance of the new particle formation events. I am not sure why the authors ignored the other literature, since the group can look back on a comprehensive review of particle formation observations by Kulmala et al. (2004).

I recommend some literature that has a direct mention of the spatial scale of new particle formation, using methods that are similar to the authors Approach I and II: Wehner, B. et al., Tellus B, 59, 362–371 (2007), Birmili et al., ACP, 3:361–376 (2003), Charron, A. et al., JGR Atmospheres, 112, D14210, doi:10.1029/2007JD008425, 2007. Also, the group of Colin O‘ Dowd (University of Galway) examined spatially limited coastal nucleation events and how these may evolve into regional nucleation events. There are more observations and classifications of nucleation events available from the US (Atlanta), the University of Manchester group (UK), the University of Kuopio group (Po Valley), and many other recent observations worldwide. I think it is mandatory to study and discuss that literature. Then, I believe that the authors can give a confident statement whether their results can be "generalized to many of the air parcels traveling over the European continent." (see Abstract).

I get a slight impression that the article is over-used as a vehicle to promote as many of the authors‘ own citations as possible, even if they bear only a distant relationship to the present article. An example is the multiple citation of urban background aerosol characteristics (Hussein et al., 2004, 2005b, 2006, 2007). This looks a bit quaint in view of urban new particle formation being explicitly excluded as a topic from this paper. Self-citation is natural and healthy, but here it is exaggerated to an extent that is gives a misleading impression on the achievements made in the field. I am happy to hear the authors‘ opinion on this issue.

2) The results presented in the article are sound, but seem rather phenomenological. Much space is devoted to the case studies — fine, but I miss a more analytical examination of the critical issue *why* particular new particle formation events are limited in space, and others are not.

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I imagine that the spatial extent of a particle formation event (Approach I) might also be due to the same meteorology in multiple places. The authors could relate some simplistic local variables such as solar irradiation (or cloudiness) to discriminate local events (one site only) from regional events (multiple sites). I feel that such an analysis is mandatory for the revision of the paper.

From reading some of the references I assume that growth rates of the nucleation mode might also be available for the data sets. It would be worth looking into correlations of growth rates between the sites in the case of simultaneous events (Approach I). High correlations would confirm that the concentrations of aerosol precursors might be similar over a large area, and contribute to the simultaneous occurrence of particle formation events in different sites.

3) Fig. 5: What is the significance of the blue areas far off the receptor site? In Figs. 4 and 5 something like a borderline of significance, maybe based on a minimum number of observations per geographical unit area, should be indicated.

4) The results are important for future attempts to validate regional and global aerosol models, particularly considering the secondary formation process. I am wondering if the authors could discuss some ideas how their analysis/results might be made available for that purpose.

5) Formal points

Page 143: Equation 1 seems unnecessary. It can be omitted since it represents rather general knowledge and appears in many of the references cited.

Section 2.5, "Air mass back-trajectory": it seems more logical to move this section before 2.4.

"median=27 h" is not a valid expression.

The geographical coordinates of Aspvreten indicated in the paper seem wrong. How are 80 arc seconds possible? This seems to be copy-pasted from Tunved et al. (2003),

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where the coordinates are already wrong.

Use "time span" and "spatial scale" (spatial is an adjective) rather than the version with the hyphen.

We don‘t say "evidences", there is just "evidence".

"Air parcels" is typically used to describe small scale motion of air, up to a few kilometres. The authors certainly mean "air masses?"

Reformulate awkward axis labels such as "Day number in".

Practically all Figures need to be drastically improved regarding bigger and uniform font sizes, bigger dot sizes, thicker lines. At the moment most readers will probably need a magnifying glass to detect writings such as in Fig. 8. The authors might also consider showing smoothed time series when appropriate instead of scattered clouds of raw data.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 135, 2009.

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