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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cold oceans enhance terrestrial new-particle formation in near-coastal forests" *by* T. Suni et al.

T. Suni et al.

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We as the authors would like to thank both referees for their insightful comments and their efforts in trying to improve our paper. Our final comments to the points raised by the referee are given below.

Comment 1: Authors note that the distance from ocean/sea and the cities is very important in this kind of studies. What do you mean under too close to the coast, and what is the necessary minimum distance from the coast and from the cities in order to reach "free from antropogenic influence"? Please give some numbers on it especially for Tumbarumba site. I could not find information on it neither in the text, nor in Fig.1.

Reply: The point we were trying to make is that it is difficult to find a measuring site at

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an equal distance from two different oceans and with a roughly equal amount anthropogenic influence (only one megacity on each coast compared to, let's say, 4 major cities on one coast and only 1 on the other). The concept of a "remote site" includes sites such as SMEAR II in Hyytiälä (about 400 km from Helsinki, but with much more populated areas around it than Tumbarumba). Compared to Hyytiälä, Tumbarumba is also much cleaner (total particle concentration only half of that in Hyytiälä, see Suni et al. 2008; reference given in manuscript).

Comment 2: Fig 3 is not clear for me and I could not follow the explanation of this figure on page 13099 from line 21;): Fig. 3. Median number concentration of 2–14nm ions as a function of trajectory direction. The number concentration is shown at (left) 6 h before arrival (middle) 12 h before arrival and (right) 24 h before arrival to Tumbarumba (11 – 16). Black – negative, grey – positive ions

Reply: We agree that the figure is difficult to understand. The idea is to show that the highest particle concentrations are found on days when the air masses have arrived rather directly from the south-west: the location of the air mass has been in the south-west 6 h, 12 h, and 24 h before the observed formation event instead of the wind circulating around the surrounding land areas. We will clarify this point in the caption in the revised version of the manuscript. The figure is related to total aerosol concentrations measured with a CPC whereas Fig. 1 refers to the frequency of new particle formation measured with an AIS. These are two different measurements showing the same phenomenon. This strengthens our argument that the south-western winds are the source of the most intense particle formation.

Comment 3: From the results (page 13100 from line 28) it is shown that the air masses from southwest are dryer than the air from NE. This is clear evidence for "dry", but not for "clean" air mass. Authors connect these two properties; however, this latter should be proved by data. Could you please include some experimental data e.g. SO2, VOC or others?

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Reply: We do not mean to connect these two properties together. Both wind directions are clean judging from the overall total ultrafine aerosol concentration (only half of what is found in Hyytiälä, a remote site in southern Finland (Suni et al. 2008; reference given in manuscript)). We will clarify this point in the revised manuscript.

Comment 4: The experimental data of VOCs are just presented in a table but unfortunately further exact discussion of them regarding NFP is quite incomplete. This summary is descriptive and rather speculative. I cannot see convincing evidence for their influence on aerosol formation.

Reply: We are not trying to prove the role of VOCs in biogenic particle formation. This point has been shown in several previous studies such as Kulmala et al. (2004), Atkinson (2000), and Calogirou et al. (1999), to name only a few. We will add references to these papers in the introduction. The table of VOC observations is presented because our field measurements were (and still are) the first and only ones ever made in South-Australian landscapes and they feature a number of aerosol-producing monoterpenes (pinenes, limonene, MBO, isoprene).

Comment 5: I feel too much simplified the conclusion that relative humidity is the only parameter that reduces NPF events. Are you sure in it; and no other parameters can influence it?

Reply: Our main point needs to be clarified: we could not find evidence of any other factor that would have varied as significantly as this one and as much in tune with NPF. More VOC measurements need to be made to ensure what their role is, but based on the evidence we have at the moment, the humidity variation has by far the greatest correlation with NPF variation.

Comment 6: To my opinion too much figures/tables are included to the text. Some of them may not be necessary. Please consider reducing the number of figures/tables. In Table 1 the data of Fig 2 and Fig 5 is presented; from Figs. 8-9 and Figs.10-11 (on latent heat: two figures – one sentence) all of them are necessary?

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Reply: Table 2 is crucial for the main point. It gives quantitative numbers for different sectors compared to each other. Figs 8-9 and 10-11 similarly give crucial evidence of the effect of humidity and latent heat flux.

Comment 7: Minor comment: Please include that which seasons are represented by a, b, c and d in Fig. 5!

Reply: This is an unintentional oversight and we will include this information in the revised manuscript. Also Fig 10 is missing the explanation of the seasons.

References given here:

Atkinson, R.: Atmospheric chemistry of VOCs and NOx, Atmos. Env. 34, 2063–2101, 2000.

Calogirou, A., Larsen, B. R., and Kotzias, D.: Gas-phase terpene oxidation products: a review, Atmos. Env., 33, 1423–1439, 1999.

Kulmala, M., Vehkamäki, H., Petäjä, T., Dal Maso, M., Lauri, A., Kerminen, V.-M., Birmili, W., and McMurry, P. H.: Formation and growth rates of ultrafine atmospheric particles: a review of observations, J. Aerosol Sci., 35, 143–176, 2004.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 13093, 2009.