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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global analysis of continental boundary layer new particle formation based on long-term measurements" *by* Tuomo Nieminen et al.

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We thank the referee for the comments on our manuscript. Below we give our response to each of the comments and indicate changes made to the manuscript (referee's comments are shown in italics and our response in normal type).

This manuscript compiles and re-analyzes new particle formation and growth statistics from 36 surface sites. It is great to have all of these data in one places and analyzed homogeneously. I am very happy with the manuscript, and I only have a few minor comments. I feel it is ready to be published in ACP once these comments are addressed.

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Discussion paper

L6-7: Why, specifically, are only March-May and Dec-Feb being compared here? Is March-May the max and Dec-Feb the min? It would be good to say this, because now the choice seems arbitrary.
March-May was indeed the maximum in NPF occurrence frequencies and Dec-Feb minimum, averaged globally at these 36 measurement sites. As suggested by the referee, we have clarified this sentence in the revised manuscript: "We found that the NPF frequency has a strong seasonal variability. At the measurement sites analyzed in this study, NPF occurs most frequently in March-May (on about 30

Throughout: "Westervelt" is misspelled as "Westerveld" is several places.
We have corrected the spelling of the name throughout the revised manuscript.

L88-97 and L509-516: Why is the free troposphere not mentioned here? Nucleation in the FT is hugely important for CCN (Merikanto et al., 2009).
We agree with the referee that free-tropospheric nucleation is an important processes for the global aerosol number. The focus of this study, however, was to obtain a global picture of new particle formation in different environments. The long-term aerosol size-distribution data needed for this is only available from ground-based sites that are located inside the planetary boundary layer. Even the few high-altitude sites included in our study are only part-time in the free troposphere. For this reason we have not discussed nucleation in the FT in this manuscript.

*L408-410: This sentence is strange. It's discussing the factors that determine Jnuc when Jnuc is inferred from dN(10-25nm)/dt, GR, and CoagSink; however, the sentence is written as if Jnuc *depends* on these values. Jnuc depends on vapor concentrations and temperature. It's only inferred using dN(10-25nm)/dt, GR, and CoagSink.*
The referee is correct that J_{nuc} (formation rate of 10-25 nm particles) is calculated from the measured N(10-25) using dN(10-25)/dt, GR (correcting for growth out of the 10-25 nm size range) and CoagSink (correcting for coagulation losses of 10-25 nm particles). However, ultimately J_{nuc} depends on the actual nucleation rate J^* (formation rate of $d^* \approx$ 1.5-2 nm clusters; Kulmala et al., 2013) and the losses occurring during the

condensational growth of these clusters to 10 nm size. As shown by Lehtinen et al. (2007), the dependence of J_{nuc} on J^* can be expressed as

$$J_{\mathrm{nuc}} = J^* \exp(-\gamma \cdot d^* \cdot \frac{\mathrm{CoagS}}{\mathrm{GR}})$$

where the value of the parameter γ depends on the size-distribution of the pre-existing aerosol. To clarify this sentence, we added to the revised manuscript: "This is because during the growth of the initial nucleated particles they are continuously scavenged by coagulation with the pre-existing aerosol."

Reference:
Kulmala, M. et al. (2013): Direct Observations of Atmospheric Aerosol Nucleation. Science 339, 943–946.

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