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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The first estimates of global nucleation mode aerosol concentrations based on satellite measurements" *by* M. Kulmala et al.

Anonymous Referee #2

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This paper attempts to derive satellite remote sensed proxies for the number concentration of particles in the nucleation mode. Observations of nucleation mode particles from a research station in the boreal forest in Finland are used to test a number of parameterized equations. The spatial pattern of the particle number concentration predicted by the satellite products is compared qualitatively against published observations of particle formation events.

The paper represents a novel approach to the use of satellite products to explore spatial patterns in new particle formation. It provides new insight into spatial patterns in nucleation at the global scale that will be of interest to the community. The paper is

well written and after some minor comments have been addressed it will be suitable for publication in ACP.

I have also suggested two additional pieces of analysis that I feel would strengthen the paper. However, there may be reasons that including such analysis is impractical at the present time. I do not feel that this additional analysis is essential before publication and I leave it to the authors to decide whether they would like to include it.

Comments

A weaker aspect of the paper is the qualitative evaluation of the spatial pattern of the satellite derived proxies. The paper would be stronger if a more comprehensive evaluation of these spatial patterns was completed. A suggestion would be to use a global dataset of locations where new particle formation has been observed [e.g., Kulmala et al., 2004] to test the proxies in more detail. In the absence of such analysis I suggest changing the abstract to:

" The global pattern of nucleation mode particle number concentration predicted by satellite data using our proxies is compared qualitatively against both observations and global model simulations."

I agree with the discussion on the relative role of SO2 concentrations in controlling nucleation at the SMEAR station (Figure 1 and P18834). However, as the authors acknowledge this analysis does not demonstrate that SO2 is unimportant in controlling the spatial patterns of nucleation mode particles. I understand that the detailed and long term datasets that are available at the SMEAR station are not available at enough other locations to be able to evaluate the importance of SO2 at the global scale.

Nethertheless, the authors should include a more detailed discussion on the likely impacts of including variable SO2 concentrations in Eq. 9 and how this would change the spatial patterns of the proxy. Likely changes would be a reduction of nucleation over the Amazon and over southern hemisphere continents in general (outside the relatively few

regions that are heavily influenced by anthropogenic SO2 emissions including South Africa, regions of Chile, coastal Brazil and the east coast of Australia) and an increase in nucleation over China.

The authors demonstrate that remote sensed products of SO2 are not suitable to include variable SO2 in equation 9. A suggestions is that the authors could try using a global model product of SO2 in place of satellite observations. The broad spatial patterns of SO2 simulated by global models have been tested against SO2 observations and should be quite reliable. I realise that the authors may rather leave such an analysis for future work, or may not like the idea of merging satellite and model products. In this case I feel that the additional discussion requested above is sufficient.

Minor comments

Please clarify the time resolution of the data used in Fig. 1.

P18829, L11. Change to “most likely AN organic”

P18834, L3. Please clarify for which months AOD data is unavailable.

P18834, L8. Please reword the sentence to : “The main reason for the scatter in the data points in this figure is LIKELY that the proxy assume...”.

P18836, L24. I wouldn’t agree that the nucleation proxy is zero over the Amazon particularly in MAM but also across regions of the Amazon in DJF and SON. I think this is important as the Amazon is one reason where gas-phase sulfuric acid concentrations may be too low to initiate nucleation (see the discussion in Martin et al., 2010) and so may be poorly predicted by the current proxy that does not account for SO2 concentrations.

P18837, L4. Please correct English e.g., add “to confirm whether”

Figure 4. One of the most obvious patterns are higher values of the nucleation proxy in the Southern Hemisphere (SH) than in the Northern Hemisphere (NH). What is the

reason for this? I wonder whether this is due to typically lower AOD than in the NH. Lower SO2 concentrations in the SH (outside of isolated regions of pollution) would likely reduce the nucleation proxy over much of the SH if variable SO2 were included.

Figures 4 and 5: What are the reasons for the missing data (white) over continents?

Figure 5, caption: What do you mean by “however the color scale is not pre-defined”?

References

Martin et al., Sources and properties of Amazonian aerosol particles, Reviews of Geophysics, 48, RG2002, 2010.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18823, 2011.