

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

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We thank the referee for the positive comments. Below we give our answers to the comments of the referee; our response is written in italics.

General comments:

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 obser-
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ventions 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.

We will add a discussion on the issues raised by the referee and emphasize the future need for more detailed analyses in terms of 1) using other long-term data set, and 2) the possibility of merging satellite data with global model simulations.

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."

We will modify the last sentence of the abstract, as suggested by the referee.

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.

We completely agree with the reviewer. In the revised paper, the potential role of SO₂ variability in the quality our predictions will be discussed explicitly in section 3.1 (fifth paragraph), 3.2 (fourth paragraph) and in conclusions (second paragraph).

The authors demonstrate that remote sensed products of SO₂ are not suitable to include variable SO₂ in equation 9. A suggestion 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.

This is a good point that we will discuss in section 4 of the revised manuscript.

Minor comments:

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

The time resolution of the data is 30 min. We have added this to the figure caption.

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

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This will be corrected.

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

The AOD data are unavailable from about mid-November to mid-February. We will add this information to the text.

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.

We agree. We will modify the text in the fourth paragraph of section 3.2.

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

This will be corrected.

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.

The most likely reason is the one suggested by the reviewer.

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

The white areas of missing data are mainly due to the missing satellite-based AOD data. MODIS AOD retrieval over land excludes areas of bright surfaces (e.g. snow/ice

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surfaces, deserts), where the retrieval uncertainty is too large.

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

We meant that the color scale covers the entire range. Since this may confuse the reader, we will remove this statement from the figure caption.

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