Atmos. Chem. Phys. Discuss., 9, C4884–C4888, 2009
www.atmos-chem-phys-discuss.net/9/C4884/2009/

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.

theo.kurten@helsinki.fi

Received and published: 16 September 2009

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 referees are below.

Comment 1: The oceans affect nucleation. . .also by emitting nucleating precursors (DMS and iodine species). In Mace Head, coastal iodine emission totally dominate observed nucleation event, so this kind of a study could not be conducted there even if different oceanic conditions were met. The authors should bring up these points in the introduction and to be careful what the mean by "oceanic air properties" and "oceans with contrasting characteristics".

Reply: We agree with the referee that nucleating precursors are a crucial way for the oceans to affect nucleation at coastal areas. However, our measuring site is located 500 km from any coast. Although we have no direct measurements of the effect of DMS in particle formation in Tumbarumba, DMS can certainly be transported several hundreds of kilometers inland (with a lifetime of approximately 48 hours based on the rate constant given in Seinfeld and Pandis 1998; see also Huebert, 2007). Ristovski et al. (2009) have shown that the sulphate component in the particles collected in Tumbarumba in most cases is only <6% of particles in the nucleation mode, and the majority of the growth is attributable to organic vapours emitted by the vegetation. Furthermore, Modini et al. (2009) found no trace of iodine in particles (17-22.5 nm) collected at Agnes Water, a remote tropical site at the coast of the Pacific Ocean. According to Tunved et al. (2006), the effect of the boreal forest is clearly observable in particle formation at in air masses travelling from the North Sea inland at distances of hundreds of kilometers from the coast. Although the marine precursors are likely to have an effect on the growth of the particles, it seems to us, therefore, that their role must be minor compared to that of organic vapours emitted by the vegetation between the oceans and the Tumbarumba site. These points will, however, be raised in the revised manuscript.

Comment 2. The authors say that i) vegetation types vary greatly around their study region and ii) there is no big difference in photosynthetic activity between north-east and south-west direction. Based on this, they claim that organic precursors are probably the same between these two directions. I do not think this statement is justified. Even if total VOC emissions were about the same, concentrations of vapors participating in aerosol formation may be quite different, since different species are known to emit very different set of VOCs and reaction products from different VOCs produce vapors with different aerosol formation potential. The authors should be very careful what they say here.

Reply: We agree with the referee on this point. The lack of comprehensive VOC mea-

ACPD
9, C4884–C4888, 2009

surements in the area surrounding Tumbarumba is a weakness in our study and in Australian atmospheric research in general. There were no such measurements available in South-East Australia before our studies, and our two field trips around Victoria and New South Wales were the first attempts to fill this gap in knowledge. We could not find a systematic, consistent difference between the east and west directions or among different vegetation types – the only consistent feature was great variability in VOC composition. Adding to this the NDVI evidence, we had to conclude that there were no grounds to suggest the overall VOC load would be clearly different in the east and the west, although the composition of VOCs obviously varied in different vegetation areas. Contrary to this, the difference in RH between the south-west and north-east directions was absolutely clear and easily observable. Humidity was the only variable clearly in tune with the NPF trend.

Comment 3: However, I am not really convinced that the evidence presented in this paper really proves that the air humidity is the controlling factor. The authors should, at the very least, leave the door open for other possible explanations.

Reply: We will stress this point in the revised version of the manuscript.

Comment 4: Abstract is not the right place to motivate the study, unless the motivation can be madein a compact and consistent manner. I do not think that the authors have managed in this respect in the first half of the abstract.

Reply: We will remove the motivation from the abstract of the revised version of the manuscript.

Comment 5: Please back up the first paragraph of Introduction with a few more references. Especially, what it comes to aerosol climatic forcing. Also, there are more recent studies on CCN formation related to atmospheric aerosol formation.

Reply: We will amend the introduction according to the referee's comments in the revised version.

ACPD

9, C4884–C4888, 2009

Comment 6: What does "significant cities" mean (page 13096, line 7)?

Reply: Cities with several millions of inhabitants.

Comment 7: The aerosol charging probability depends very strong on particle size, so the charged fraction depends heavily on the ambient particle size distribution. Therefore, stating that the charged fraction is approximately 10% is very rough oversimplification. Please modify this statement (section 2.2).

Reply: The reference (Hirsikko et al. 2007) will be moved after the sentence "The charged fraction is approximately 10% so there are usually about 10 times more neutral particles than charged ones" to stress that the estimate of the charged fraction is related particularly to the size range of the AIS (3-40 nm).

Comment 8: The quality of figure 10 and 11 is not very good. Could it be improved somehow?

Reply: Unfortunately this is not an easy thing to do. We will attempt to improve the figures in the revised version, if possible.

References given here:

B. Huebert: Do I believe in CLAW, Environ. Chem. 4, 375–376, 2007.

R.L. Modini, Z.D. Ristovski, G.R. Johnson, C. He, N. Surawski, L. Morawska, T. Suni and M. Kulmala: New particle formation and growth at a remote, sub-tropical coastal location. Atmos. Chem. Phys. Discuss. 9, C3784–C3787, 2009; www.atmos-chem-phys-discuss.net/9/C3784/2009/

Z. D. Ristovski, T. Suni, M. Kulmala, M. Boy, N. K. Meyer, J. Duplissy, A. Turnipseed, L. Morawska and U. Baltensperger: The role of sulphates and organic vapours in new particle formation in a eucalypt forest. Atmos. Chem. Phys. Discuss. 9, 17793–17815, 2009; www.atmos-chem-phys-discuss.net/9/17793/2009/

J. S. Seinfeld and S. Pandis: Atmospheric Chemistry and Physics, Atmospheric Chem-

ACPD

9, C4884–C4888, 2009

istry and Physics: From Air Pollution to Climate Change, Wiley&Sons, New York, USA, 1998.

P. Tunved, H.-C. Hansson, V.-M. Kerminen, J. Ström, M. Dal Maso, H. Lihavainen, Y. Viisanen, P. P. Aalto, M. Komppula and M. Kulmala: High Natural Aerosol Loading over Boreal Forests, Science 312, 261 – 263, 2006.

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