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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.

Anonymous Referee #1

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General:

Atmospheric aerosol formation related to oceanic air masses arriving at forests with considerable biogenic VOC emissions has been investigated. The main emphasis in the paper has been put on the role of oceanic conditions in affecting aerosol formation. The idea of this paper is original and worth to be published. The paper itself is relatively well structured and very clearly written. There are, however, a few scientific issues that need to be addressed with more care before I can recommend publication in ACP.

Major comments:

The oceans affect nucleation probably not only by providing clean background and suitable meteorological conditions (temperature, humidity), but also by emitting nucleating

precursors (DMS and iodine species). It is clear that DMS oxidation products are the main precursors for nucleation in the Southern hemisphere, except over a few continental locations. 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".

The authors say that i) vegetation types vary greatly around their study region and ii) their no big difference in photosynthetic activity between north-east and south-west direction. Based on this, they claim that organic precursor are probably the same between these two direction. 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.

After a very speculative discussion before conclusions, the authors explain their findings (differences in nucleation frequency between the two air mass origins) by humidity effects. It may well be that this is true. 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.

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

Minor comments

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.

What does "significant cities" mean (page 13096, line 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).

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

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