

Interactive comment on “Sensitivity of aerosol concentrations and cloud properties to nucleation and secondary organic distribution in ECHAM5-HAM global circulation model” by R. Makkonen et al.

Anonymous Referee #2

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The paper includes a new particle formation mechanism and improved treatment of secondary organic aerosol within the ECHAM5-HAM model. The sensitivity of simulated aerosol properties to particle formation mechanism, particle formation rate and the yield of SOA from monoterpenes are studied. This paper is important as it makes one of the first attempts to estimate the impact of particle formation on cloud droplet number concentrations. It is generally well written and clearly presented. I have one potential concern that the authors should address before publication.

At a number of sites (in particular Pallas and Mace Head) the observed number con-

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centrations that are quoted seem to be too low (Table 2). For example at Pallas, the authors quote a value of 267 cm⁻³, whereas Komppula et al. (2003) quotes values of 700 cm⁻³ and 870 cm⁻³ for the size range 7-500 nm. Therefore, I would expect higher number concentrations than the authors quote (even for $D > 10$ nm). What is the reason for this apparent discrepancy? If the numbers from the Komppula et al. are more appropriate then the activation (A) experiments would give better agreement with the observations than the B experiments and might address my second point below.

P10969 I am slightly worried that in remote continental areas the B simulations give better agreement with the quoted observations than the A experiments. Remote continental regions are probably the best regions in the model to look for evidence of new particle formation - they are far from primary sources of pollution and so uncertainty in the treatment of primary aerosol sources will have relatively little impact. In contrast, in polluted regions where the activation scheme does appear to improve the model, uncertainty in the primary particle source could easily be an alternative explanation.

Therefore the comparison with observations presented here appears to maybe provide some evidence that new particle formation is not playing an important role in controlling regional particle number concentrations. Annual mean particle number concentrations are probably not the best observations to use to evaluate the nucleation mechanism. However, if this new particle formation mechanism is leading to an overestimate of particle number in many locations should we believe the impact the mechanism has on cloud droplet number?

In the conclusion the authors state: "The comparisons to the observations show that activation nucleation is a very promising way to improve the ECHAM5-HAM model closer towards the average values observed over different locations, although the results show high temporal variability with occasionally unrealistically high number concentrations in polluted areas" I think this statement requires some admission that the scheme also appears to overestimate remote continental particle number.

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P10975. Comparison with the results from Bennartz et al. is difficult for the reader at present. I suggest including the results/figure from this paper as well for comparison.

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