

Interactive comment on “A GCM study of organic matter in marine aerosol and its potential contribution to cloud drop activation” by G. J. Roelofs

Anonymous Referee #1

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The author uses the ECHAM5-HAM GCM to examine the potential role of marine organic aerosol on cloud droplet nucleation. The model uses a parameterization described by Roelofs et al (2006) that is based on the work of Hanel (1987). The marine organic aerosol is prescribed in the model in a number of suitably different ways (i.e. the sensitivity tests), including different mixing states and relative compositions amongst the main particle size distribution modes.

The central premise is that the addition of organics to the model that was described by Roelofs et al. (2006) and for which a deficiency in the simulated CDNC existed, increases the CDNC. The main conclusion of the current paper is that the inclusion of

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the organics in the model increases the cloud droplet number concentrations (CDNC).

I believe that the conclusion is correct in the general sense. However, there are important processes/issues that have not been clearly discussed that can also affect the CDNC. There is a notion that the addition of organics to the model solves the problem that existed in the 2006 paper, and that discrepancies with satellite observations are due to the precise quantities/nature of the organic put into the model. To accurately assess the impact of the organic aerosol, you need to have the other relevant processes properly constrained also and that appears unlikely. I outline below some concerns about the treatments and assumptions used here. The paper becomes acceptable if the proper uncertainties are identified.

1) when comparing CDNC with observations, the simulated quantities have to be based on two broad processes: the nucleation at cloud base, and then dilution due to cloud mixing processes. As described in Roelofs et al. (2006), the updraft velocity derived from the TKE is scaled down. The reduced updraft velocity lowers the CDNC, and how does the author know that this is not a larger source of the discrepancy with his 2006 paper? The reference used for that scaling down is not in the peer-reviewed literature, and the author ignores the work of Peng et al (JGR, 2005) that specifically addresses this issue and suggests the use of a larger value of the PDF of updrafts. How is the cloud mixing/entrainment process handled in Lohmann and Roeckner (1996)?

2) section 4.2 - a) The comparisons are done with satellite estimates of CDNC. While I appreciate the value of the spatial coverage provided by satellites, there are no in-situ observations mentioned. Are the satellite results accurate such that we don't need in-situ observations? b) the model output is for the highest cloud below about 5-6 km. What influence does the marine BL influence these clouds, and how do you distribute the marine organic in the vertical initially - is it uniform within the MBL?

3) the organic treatment - a) some clarification about the use of solubility is needed here. By saying that something is soluble, is it intended that it is CCN active. Is there

any inclusion of the effect of slow dissolution of organics? b) The results of O'Dowd are not universal (e.g. Phinney et al., DSR, 2006 summertime measurements over the North Pacific Ocean show a relatively low contribution from organics to particles over the CCN size range, with the largest organic mass likely associated with ship emissions.)

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