

## ***Interactive comment on “MADE-IN: a new aerosol microphysics submodel for global simulation of potential atmospheric ice nuclei” by V. Aquila et al.***

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I find one of the central points of this manuscript without experimental or observational basis, specifically “Black carbon (BC) . . . are among the dominant atmospheric ice nuclei, i.e. aerosol particles that can initiate heterogeneous nucleation of ice crystals.”

The recent literature without exception shows that black carbon is NOT a good ice nucleus (whereas mineral dust, the other aerosol type studied in this simulation, is). The most recent and relevant work to this study, Dymarska et al. Deposition ice nucleation on soot at temperatures relevant for the lower troposphere, GRL, 2006 is not even cited : “For our experimental conditions, ice nucleation never occurred at temperatures above 248 K and below water saturation. Below 248 K, ice occasionally formed in our experiments with no indication of the formation of water droplets prior to ice formation.”

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The authors support their contention that BC and mineral dust are good IN with the references DeMott et al. 1999, 2003; Sassen et al. 2003; Moehler et al. 2006; Kanji et al. 2008. Of these only DeMott et al. 1999 actually concerns black carbon and only in laboratory studies. The remainder concern mineral dust – which is a good IN – from both laboratory and field studies. Moreover, the results of DeMott actually largely contradict what is stated in this paper : that bare and thinly coated soot freeze at essentially the homogeneous freezing point: “Considering expected water uptake on these particles the onset conditions for ice formation were consistent with those expected for homogeneous freezing nucleation”; only in the case of thickly coated particles at lower temperature was a slight heterogeneous freezing (earlier than homogeneous) case observed. In the remainder of the paper there is only one other reference that cites ice nucleation experiments – not field observations - of black carbon, that of Moehler et al., 2005. Moehler’s findings were similar to DeMott’s : “Above 235 K, ice nucleation only occurred after approaching liquid saturation” (i.e., the same as for homogeneous freezing) with sub-saturated nucleation only occurring at cold temperatures (largely colder than considered in this study).

I am aware that BC has been and is often considered a good IN by modeling studies. This is, however, without experimental or observational basis and needs to be corrected. It is also troubling that the most current and relevant references have not been cited and those papers that are cited actually largely show the assumptions of this work is not correct.

My intention here is not to be overly negative of this paper. Indeed it is a well written manuscript on a topic of interest in atmospheric science. What needs to be done is to center this paper on ice nucleation by mineral dust – which the authors correctly show is considered a good IN through laboratory and field studies – and eliminate BC – which does not have any basis. Insistence on BC being a good IN undermines what is otherwise a very nice work.

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