

***Interactive comment on “Impact of dust aerosols on Hurricane Helene’s early development through the deliquescent heterogeneous freezing mode” by H. Zhang et al.***

**Anonymous Referee #1**

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General comments:

Zhang et al. present numerical simulations of Hurricane Helene, in which the heterogeneous ice nucleation parameterization is changed from the standard formulations used in the Morrison scheme to the Khvorostyanov & Curry freezing scheme. For both setups, the input dust concentrations are varied. Extensive comparisons to observations are shown and clear differences are found, in particular due to more heterogeneous freezing in the KC scheme and thus less homogeneous freezing at higher altitudes. However, the comparisons don’t allow for robust conclusions about which setup is more realistic.

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Technically, the paper is well written. The simulations and the data analysis are overall solid. The topic is within the focus of ACP and is of interest to a broad readership. But I recommend major revisions concerning the scientific content of the manuscript, as outlined below. My suggestion is that the model description is clarified and that the comparison between the different simulations is shortened. The figures should be enlarged and reduced in number.

When reading the manuscript, I stumbled in section 3.1 (the description of the freezing parameterization). The model description needs at the very least more explanation and justification. It seems that the number of ice crystals formed through the so-called DHF mode depend neither on the droplet concentration nor on the dust aerosol concentration. This is in sharp contrast to other aerosol-related freezing parameterizations, e.g. Lohmann & Diehl, JAS 2006; Phillips et al, JAS 2008; DeMott et al, PNAS 2010; Hoose et al, JAS 2010. According to classical nucleation theory, which is referred to several times, the freezing probability of droplets is proportional to the surface area of the immersed ice nuclei. Where is this included in the parameterization? If the freezing is not coupled to the dust aerosol concentration, then the title of this manuscript is misleading. From what I understand, dust particles only have an impact on the droplet number concentration – in both the standard Morrison scheme and in the extended scheme. And a variation of e.g. sea salt CCN would lead to the same model results.

In section 4, the model results are compared to satellite observations. The comparison is hampered by a time lag. It is difficult to take away a consistent picture from figures 8, 9, 11 and 12. All Morrison simulations and all KC simulations fall into one group, respectively. Neither of the groups strongly resembles the observations. Would that look different for model results at 1 hour later or earlier? Was the timing chosen according to some objective criteria? Can it be that the pdfs of the rainrate depend rather on the model resolution than on the freezing parameterization? Instead of cloud top temperatures, OLR and rainrate, I suggest add more variables to Figure 7 (in particular LWC, cloud droplet number concentrations and droplet freezing rates) because these

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will allow a better analysis of why the Morrison and KC simulations differ. For the derived properties, my feeling is that not more conclusions can be drawn than the model simulations are sensitive to the freezing scheme but rather insensitive to the number of CCN.

Detailed comments:

- Abstract line 9/10: “lower, increase, more, less”: Explain what you are comparing to.
- P 14343, line 3: “nucleating aerosols”: droplet or ice nucleation?
- P 14343, line 14: “interest in the nucleation ability” (insert “the”)
- P 14343, line 15: part → parts
- P 14343, line 21: “assume a clean environment” - explain what this means. It certainly does not mean “no ice nuclei”.
- P 14343, line 21/22: one could argue the empirical ice nucleation parameterizations include all possible mechanisms.
- P 14344, line 27: “small effective radii”: are these ice crystals or droplets? Please clarify. For droplets, this would be rather large.
- P 14345, line 21ff: What is Gordon? And where? And what has this to do with Hurricane Helene? The last sentence of this paragraph is not clear to me.
- P 14346, line 6: Where exactly are the domains located? They could e.g. be inserted into one of the satellite images. Is the domain shown in Fig. 8 the inner one?
- P 14347, line 7: Contact nucleation is not explained by classical nucleation theory (see Pruppacher & Klett or the papers by Neville Fletcher.)
- P 14347, line 11/12: is deposition nucleation not a function of supersaturation in the Cooper (1986) formulation? It would be weird if not. (See e.g. the deposition nucleation parameterization by Meyers et al.) The Cooper (1986) paper is not easily accessible,

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therefore you might consider giving the equation here.

- P 14347, line 22/23: “at subsaturation over water ... the observed high nucleation rates at relatively warm temperatures (-5 to -12°C)”: which observations are you referring to? Most laboratory experiments show that ice nucleation on mineral dust is very inefficient at warm T and below water saturation.
- P 14348, line 15, “ $N(T,w)=...$ ”: Why is this independent of the number of dust particles? Are the fit parameters specifically for dust or for any other aerosol type? The number of ice crystals formed by heterogeneous nucleation should increase with increasing dust number concentration. This doesn’t seem to be the case here.
- P 14348, line 19/20: “Both the Cooper and the Meyers schemes ...”: The Meyers parameterization of deposition/condensation freezing is a function of temperature and supersaturation. It does not make sense to compare with the Meyers parameterization for contact freezing here.
- P 14348, line 19/20: “... are functions only of temperature”: also the KC scheme is only a function of temperature if this equation is applied.
- Fig 3: Results of the KC parameterization should be shown for vertical velocities which commonly occur in TCs, i.e. in the order of several meters per second. It seems that the KC formulation (at least the equation on page 14348) will give incredibly high IN numbers for these vertical velocities. How do you deal with that?
- P 14349 and 13450: Describe how the Clean case aerosol and the dust are distributed into the two modes, respectively.
- P 14349, line 22: “horizontally distributed”: I assume you mean a homogeneous distribution/constant concentration?
- P 13450: In the simulations with dust, are there also background aerosols?
- P 13450, line 9: “Dust aerosols can be activated as CCN following Abdul-Razzak

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and Ghan”: what do you assume for the hygroscopicity of dust?

– P 13450, line 12: “cloud droplets formed from activated dust particles can freeze through the KC scheme”: This is in contradiction to the earlier statement that the KC scheme treats ice nucleation already below water saturation, i.e. before activation of the dust particles. Another question: are the droplet formed on dust tracked separately from the other droplets?

– P 14350, line 14: “The grid-scale velocities ... were found to be comparable to observations”: This needs to be shown. The vertical velocity is an important variable, as the KC scheme seems to use it as an input. Please provide some plot showing that your model can resolve updrafts in the order of several meters per second. I assume that this is not easy with a model resolution of 5km. The azimuthal averages shown in Fig. 14 are very low.

– P 14350, line 18: “the process of dust particles acting directly as IN”: what do you mean? Deposition nucleation?

– Section 4: I strongly suggest to start the description of the results with discussion the microphysics, i.e. section 4.2.

– P 14352, line 17: the droplet number concentrations are surprisingly small. Please explain why. Possible reasons that I can think of: averaging over up- and downdraft regions, strong collision-coalescence, or too low vertical velocities for activation.

– P 14352, line 23: why are the ice crystal concentrations at >10km in the Morrison scheme (Fig. 7b) not increasing with increasing dust concentration? (Although the droplet concentrations are increasing?)

– P 14353, line 8: How reliable are the ice number concentrations for CloudSat?

– P 14357, lines 2 and 4: I -> 1

– A general comment about the figures: Most satellite images are too small and the

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text inserts/legends are hardly readable.

– Fig. 1, caption: Please add more information. Why are images chosen for 18 UTC when the eastern part of the region is not reached by daylight? “visible” is not quite correct. Meteosat’s “true-color” imageries are reconstructions of approximate true-color views based on a few narrow channels. The right part is probably infrared and in the greenish colors in the left part should be explained. The text below the images is hardly readable. I suggest to crop the images to the region of interest.

– Fig. 10a: What do the colors on this image signify?

– Fig. 14: Please use color to fill the contour lines. Over what exact region are these results averaged?

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 14339, 2011.

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