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Atmospheric Chemistry and Physics Discussions

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 #2

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In this paper numerical simulations of tropical cyclone Helen are performed using the WRF model with Morrison double-moment microphysics scheme. An new ice nucleation parameterization accounting for deliquescent heterogeneous ice nucleation after Khvorostyanov and Curry (KC) is introduced in the model and sensitivity experiments are conducted. All simulations are compared with observations from remote sensors aboard the A-train (MODIS, CALIPSO, CLOUDSAT).

It is found that the vertical distribution of hydrometeors and the dynamical development of the tropical cyclone are sensitive to the choice of ice nucleation parameterization. While the standard Morrison microphysics scheme tends to overproduce ice at upper

levels, the KC promotes ice formation at warmer temperatures (through heterogeneous ice nucleation) and, thus, leads to more ice a lower levels. However, both scheme fail to reproduce the observed cloud statistics to a reasonable degree.

Generally, I do think that we could learn tremendously by comparing model simulations with observations in a way conducted in this paper but prerequisite for the usefulness of such a comparison is that the model reasonably simulates basic dynamical features of the system. Unfortunately, this is not the case for this paper, which also makes much of the microphysical comparison and subsequent discussions meaningless.

Major comments:

1. The KC scheme is coupled to the grid-scale vertical velocity of the model. While this may be physically more realistic it has been avoided in past parameterizations partly because quite often simulated grid-scale vertical velocities do not compare well with the observations and/or are not representative for the horizontal scales that control cloud nucleation. It would be interesting to see how well the simulated vertical velocities in hurricane Helena compare with the observations.

2. The key deficiency of the model at present is that is unable to simulate the storm's dynamics. Getting the model to better agree with the observations in terms of dynamics is a necessity for any further microphysics comparison. Parts of the model and observation comparison have a time lag of 12 hours, which makes them invalid.

3. From figure 8 and 9 it seems that none of the schemes are able to better represent the statistical distribution of cloud top heights (especially ice clouds) in the model. This is turn makes it difficult to argue that cloud microphysics (or here heterogeneous ice nucleation) is key for better predicting hurricanes.

Minor comments:

1. P. 13, L. 1: It would be interesting to see the microphysical properties and the mixing state are for aerosols in the dust layer. Are there any representative measurements

from NAMMA? As has been pointed out in recent papers by Kumar et al. (2009) activation of insoluble dust aerosols may be better described by adsorption activation rather than classical Koehler theory. Would differences in the dust mixing state and activation behavior make any significant difference for the results or the conclusions?

2. P. 13., section 4.1: The part that is missing in the discussion on storm track and intensities is to show what the actual TC track was and how it compares with the model simulations. Similarly, is the kinetic energy of the storm in a range that has been observed? If kinetic energy is computed from 10m wind speeds than QuikScat winds could be used for comparison with observations. How good is the model in capturing basic dynamical features of the tropical cyclone?

3. P. 14, l. 9: From the discussion and fig. 4 it is clear that the numerical simulation of the storm is very sensitive to the initial conditions. How large is the response in integrated kinetic energy to small perturbations in the dynamical initial state and how do aerosol perturbations compare with that?

4. P. 17, l. 8: The authors argue that the Cloudsat radar reflectivity may be affected by attenuation but from figure 5 it does not seem to be the case here.

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