Response to Reviewer #1

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.

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 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.

We sincerely thank the reviewer for his/her thorough read of the manuscript and providing helpful comments that helped to improve the clarity and quality of the manuscript. The main motivations for this study was derived from the NAMMA observations, which showed the dust-affected areas of tropical cyclones have large cloud droplet concentrations and ice crystals aloft in strong updraft regions. However, current mesoscale forecasting models do not take the ambient aerosols into account and usually use a fixed cloud number concentration and the ice parameterization as a function of temperature only. Our study focused on the process in which dust aerosols serve as CCN to form cloud droplets, as the droplets elevated in the updraft, they become IN and help ice nucleation. In the revised manuscript, we added a reference (Skamarock et al. 2008) to the WRF model and more description of the DHF mode (please refer to the reply to detailed comment #12) as the reviewer suggested. Due to the limited length, we provided only brief description of the ice nucleation scheme and Morrison scheme, and more details can be found

in the references provided in the manucript. The equations for ice parameterizations were given in the caption to Fig. 3.

The reviewer's main concern is that "the number of ice crystals formed through the so-called DHF mode depends neither on the droplet concentration nor on the dust aerosol concentration". The DHF mode is actually related to the droplet concentration (and hence dust aerosol concentration) in an indirect way, please refer to our manuscript (P14343, L20): "A fixed maximum value of IN is often specified to prevent unreasonable prediction of ice crystals when temperature is extremely low. For example, the maximum number of ice crystals from the Cooper scheme is set to 500 L^{-1} in the Morrison scheme. On the other hand in the KC scheme, ice crystal concentration is a function of both temperature and vertical velocity. The maximum value of ice crystal concentration is not limited by a prescribed number, but rather by the cloud droplet number concentration that is related to the aerosol concentration." Since the KC scheme is sensitive to vertical velocity as seen in Fig. 3, within strong updraft regions in the dust-laden environment, more ice crystals will form by freezing of cloud droplets through the DHF mode. This is consistent with observations from NAMMA (Heymsfield et al., 2009). Therefore, although the KC scheme is only function of temperature and vertical velocity, because more cloud droplets will be formed in the updrafts in dust cases, more ice crystals will be formed by freezing of the droplets. We have clarified this by adding: "Therefore, in updraft cores of the dust cases where cloud droplet concentrations are higher, more ice crystals will form." after the above sentences quoted from the paper. We agree with the reviewer that a fully-coupled model with aerosol modules (including emission, transportation and deposition) would be ideal and we are currently working on that as our next step.

The reviewer also concerns about the time lag when comparing the model simulations with observations. The time lag is due to the slow movement of the simulated storm. We have tried different combinations of physical parameterizations (such as PBL, cumulus convection scheme and microphysics) and started model from different times. None was able to increase the storm speed. The final locations of the observed and simulated storms with the KC scheme deviated about 3.2 degree in longitude and 1.3 degree in latitude, which is not uncommon for a 3-day TC simulation (Nolan et al. 2009). The slow storm movement was most likely caused by biases in the steering flow, which could be resulted from biases in the initial and lateral boundary condition. The reason that we have to wait the simulated storm move to the same geolocations as the observed one is that part of the simulation domain is land and part is ocean. Distinct processes will occur on these two different surface conditions. We added the following explanation in the revised manuscript: "To evaluate the dynamical structure, simulated 10-m wind distributions were compared against the QuickSCAT data (Fig. 5). The QuickSCAT measurement was taken at 1742 UTC 12 September when the storm has completely moved off the coast of Africa. At this particular time, the eastern part of the simulated storm was still over the African continent due to the relatively slow movement. Given that the surface processes over land may be distinctively different from those over the ocean, simulated wind fields at a later time (2000 UTC 12 September) were used in the comparison, when the storm moved to the same geolocation as the observed one. As seen in Fig. 5, our simulations can reproduce the wind magnitude and distribution reasonably well."

Replies to other concerns in the general comments are listed below.

Detailed comments:
1. Abstract line 9/10: "lower, increase, more, less": Explain what you are comparing to.

Yes, thanks for pointing this out. This sentence was revised as follows: "Compared to the original Morrison scheme, inclusion of the DHF mode tends to promote ice formation at lower altitudes in strong updraft cores, increase the local latent heat release, and produce more low clouds and less high clouds."

2. P 14343, line 3: "nucleating aerosols": droplet or ice nucleation?

To clarify this point, we replaced "nucleating aerosols" with "CCN".
3. P 14343, line 14: "interest in the nucleation ability" (insert "the")

Added.

4. P 14343, line 15: part → parts

Changed.

5. P 14343, line 21: "assume a clean environment" - explain what this means. It certainly does not mean "no ice nuclei".

It was changed to "a clean marine environment".

6. P 14343, line 21/22: one could argue the empirical ice nucleation parameterizations include all possible mechanisms.

We revised this sentence as follows "… and do not consider the possible effects of dust aerosols serving as CCN or IN".

7. P 14344, line 27: "small effective radii": are these ice crystals or droplets? Please clarify. For droplets, this would be rather large.

The cloud top temperature (original Fig. 9) for the region embedded in dust ranged from 230K to 260K. Therefore, it is possible that this area contains both liquid and ice clouds. The maximum cloud effective radius for this region is below 20 μm, which is a reasonable size. There are larger values about 50 μm at the edge of cloud areas south of this region. According to the MODIS retrieval algorithms (Platnick et al., 2003), effective radius retrievals around and above the 20 μm are not reliable. There could also be dust particles that contribute to the bias in retrieval. Therefore, this sentence has been removed in the revised manuscript since there could be potential problems in the retrieval and the cloud effective radius is not the focus of the manuscript.

8. P 14345, line 21: 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.

Gordon was the hurricane that formed prior to Helene. It moved off the coast of Africa on 1 September 2006 and went into a similar situation like Helene, which was surrounded by Saharan dust. Gordon was mentioned here to show that although dust seemed like approach the storm from only one side, it could re-distribute into other parts of storm. To clarify, we revised the text as follows: "Samples of anvil clouds away from the dust source were not collected during this particular flight into the pre-Helene tropical depression. However, during another NAMMA flight into the pre-Gordon MCS (which moved off the coast of Africa on 1 September and was also surrounded by dust in a similar manner as Helene), dust particles were found in the ice crystal residual samples from anvil clouds away from the dust source (Dr. Cynthia Twohy, personal communication). This suggests that storm circulation may bring dust and hydrometeors containing dust to a much larger area."

9. 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?

Yes, the color-filled area in Fig. 8 (new Fig. 9) shows the inner domain 2. This information is added in the caption: "… The color-filled area in (b) and (c) also shows the size of inner Domain 2."

10. P 14347, line 7: Contact nucleation is not explained by classical nucleation theory (see Pruppacher & Klett or the papers by Neville Fletcher.)

What we meant is that there are basic four types of ice nucleation modes. To clarify, we revised this sentence as follows: "Previous studies suggested that IN may lead to ice formation through condensation, immersion, contact and deposition freezing modes (Vali, 1985; Pruppacher and Klett, 1997)."

11. 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, therefore you might consider giving the equation here.

The equations were given in the caption in Fig. 3. In the Morrison scheme, it is a function only of temperature.

12. 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.

This sentence was referred to the DHF theory, not just dust. To clarify the meaning of the DHF mode, we modified this part as follows: "Khvorostyanov and Curry (2000) proposed a new generalized theory of heterogeneous ice nucleation for internally mixed aerosols. Khvorostyanov and Curry (2004, 2005) extended this theory and developed a framework suitable for cloud-scale and large-scale models, which will be referred to hereafter as the KC scheme. A freezing mechanism for internally mixed aerosols containing both soluble and insoluble material was included, which was referred to as the DHF mode. The DHF mode suggests that ice nucleation may start on the surface of the insoluble substance of an aerosol particle embedded in a cloud droplet that has formed on the soluble substance of the same particle. The process can start from water supersaturations and subsaturations (but above the deliquescent threshold of the soluble fraction of the aerosol). In other words, the deliquescence of internally mixed aerosol can form an ice particle under conditions of subsaturation with respect to water at supercooled temperatures."

13. 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.

This generalized parameterization was developed for cloud-scale and large-scale models by Khvorostyanov and Curry (2005). Although the KC scheme is a function of temperature and vertical velocity, because more cloud droplets will be formed in the updrafts in dust cases, more ice crystals will be formed by freezing of the droplets. Please also refer to our reply to the general comments.

14. 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.

Here, we are comparing the KC scheme with the current ice nucleation schemes being used in the Morrison scheme. The Meyers' deposition/condensation freezing mode, which is a function of supersaturation, was not used in the Morrison scheme. The Morrison scheme used the Meyers' parameterization for contact freezing nuclei.

15. 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.

The KC scheme is a function of temperature and vertical velocity (an implicit dependence on supersaturation). Please refer to the equation on P14348, line 15.

16. 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?

The KC scheme will produce large amount of ice crystals in strong updrafts. Therefore, the maximum value of ice crystal concentration is limited by the cloud droplet number concentration that is related to the aerosol concentration. Please also refer to our reply to the general comments for more details.

17. P 14349 and 13450: Describe how the Clean case aerosol and the dust are distributed into the two modes, respectively.

The size distribution for each mode follows lognormal size distribution (P14349, line 14): "Aerosol particles have a lognormal size distribution with two modes."

The equation was described in Morrison et al. (2008):

$$\frac{dN}{d\ln r} = \frac{N_t}{\sqrt{2\pi}\ln\sigma} \exp\left[-\frac{\ln^2(r/r_m)}{2\ln^2\sigma}\right]$$

where s, r_m, and N_t are the standard deviation, geometric mean, and total number concentration, respectively. We added this reference and "lognormal size distribution" when describing the dust size distribution for clarification purpose: "Dust size distribution followed the lognormal size distribution and was assumed to have two modes…"

18. P 14349, line 22: "horizontally distributed": I assume you mean a homogeneous distribution/constant concentration?

That is correct. To clarify, we modified this sentence: "The horizontal distribution of dust particles was assumed to be homogeneous…"

19. P 13450: In the simulations with dust, are there also background aerosols?

Yes, there are. We mentioned this in the manuscript (P14349, line 28): "Below and above the dust layer, aerosol properties in the Clean simulation were used to provide the background conditions."

20. P 13450, line 9: "Dust aerosols can be activated as CCN following Abdul-Razzak and Ghan": what do you assume for the hygroscopicity of dust?

The dust is assumed to have a hygroscopicity of 0.7. We set this value high to account for the scenario that dust could be good CCN. The NAMMA observations (Twohy et al. 2009) showed that cloud number concentrations in dust affected clouds are much higher than typical marine clouds, and about 79% of residual particles from liquid cloud droplets in selected clouds embedded in the SAL were composed of dust. Many dust aerosols are coated with sea salt or sulfates. This suggests that dust aerosols can be good CCN. Moreover, the Köhler theory does not account for the scenario that insoluble particles such as dust aerosols can interact with water vapor and adsorb multiple layers of water molecules under subsaturated conditions (Gustafsson et al. 2005; Vlasenko et al. 2005; Hatch et al. 2008). Therefore, the Köhler theory may underestimate the nucleation ability of dust under certain circumstance. In the future, we would like to examine a new parameterization developed by Kumar et al. (2009, 2010) that accounts for this process.

21. 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?

To clarify the KC scheme and DHF mode, we added more detail to the description (please also refer to reply to comment #12): "Khvorostyanov and Curry (2000) proposed a new generalized theory of heterogeneous ice nucleation for internally mixed aerosols. Khvorostyanov and Curry (2004, 2005) extended this theory and developed a framework suitable for cloud-scale and large-scale models, which will be referred to hereafter as the KC scheme. A freezing mechanism for internally mixed aerosols containing both soluble and insoluble material was included, which was referred to as the DHF mode. The DHF mode suggests that ice nucleation may start on the surface of the insoluble substance of an aerosol particle embedded in a cloud droplet that has formed on the soluble substance of the same particle. The process can start from water supersaturations and subsaturations (but above the deliquescent threshold of the soluble fraction of the aerosol). In other words, the deliquescence of internally mixed aerosol can form an ice particle under conditions of subsaturation with respect to water at supercooled temperatures."

Briefly, the KC scheme used in the model is a derived parameterization, which fits the calculations from improved nucleation theories that includes the DHF mode. The KC scheme itself includes scenarios with water supersaturation as well. Comparison of the KC scheme with in situ and laboratory measurements, and several other empirical ice nucleation parameterizations shows good agreement (Curry and Khvorostyanov 2010) (P14348, line 13)

The droplets formed on dust are not differentiated from other droplets as aerosols are not traced individually.

22. 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.

We agree with the reviewer that the vertical velocity is very important in determining the droplet nucleation. When we established the control "Clean" simulation, we compared the peak simulated updraft with observations, and found out that the simulated ones were comparable to observed peak updraft intensities in TCs (Fig. A). The size of updraft core (with w > 1 ms^{-1}) as illustrated in the original Fig. 6 (new Fig. 7) of the manuscript is also comparable to observations made in TCs (Black et al. 1996). Therefore, we used the grid-scale velocity to drive the nucleation parameterization. Some discussion has already been included in the manuscript: "The grid-scale velocities were used in the KC scheme to derive the nucleation rate, which were found to be comparable to observations made in TCs [Black et al. 1996; Heymsfield et al. 2010]." (P14350, line 13). Given that the manuscript has already a large number of figures, we feel that inclusion of a new figure (Fig. A) is not needed.
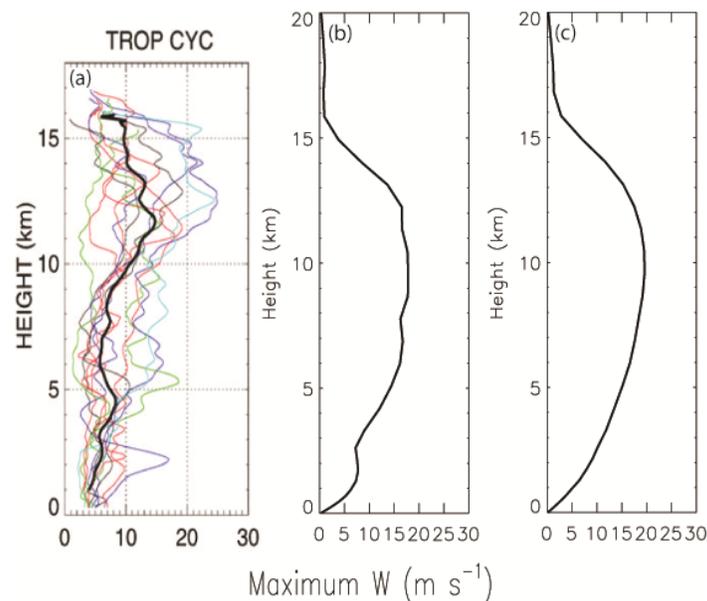

Figure A (a) Vertical profiles of the maximum updraft velocity estimated by the ER-2 Doppler radar over multiple TCs (Fig.12a of Heymsfield et al. 2010, JAS); (b) simulated maximum updraft velocity from Clean simulation at 0000 UTC 14 September; and (c) simulated maximum updraft velocity from Clean averaged over the 60-hr period.

23. P 14350, line 18: "the process of dust particles acting directly as IN": what do you mean? Deposition nucleation?

That is correct. To clarify, we replaced "IN" with "deposition nuclei".

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

We feel that an overview of the storm development in the beginning of this section is important in understanding the life cycle of Helene. We also referred to the storm intensity multiple times when discussing the microphysics. Therefore, we feel that starting with the storm intensity discussion is necessary for the layout of the manuscript.

25. P 14352, line 17: the droplet number concentrations are suprisingly 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.

The small numbers are due to averaging over all regions with non-zero cloud droplets. The cloud droplets are larger in updrafts as seen in the original Fig. 6k and 6l (new Fig. 7k and 7l).

26. 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?)

Fig. 7b is from the original Morrison scheme, in which the heterogeneous nucleation is a function only of temperature. The Morrison scheme with KC scheme shows more sensitivity to aerosol concentrations (Fig. 7d). The heterogeneous nucleation of ice crystal is related to temperature and updraft speed and capped by the cloud number concentrations. At height greater than 10 km, homogeneous nucleation plays a role as well. The transfer of ice crystals to other hydrometeor categories is also a complicated process that not directly related to the initial aerosol value. Therefore, the number of ice crystals at this level is not necessarily increasing with increasing dust concentrations.

Also, in response to reviewer's recommendations, we have added cloud water content and cloud number concentration in the original Fig. 7 (Fig. 8 in the revised manuscript):
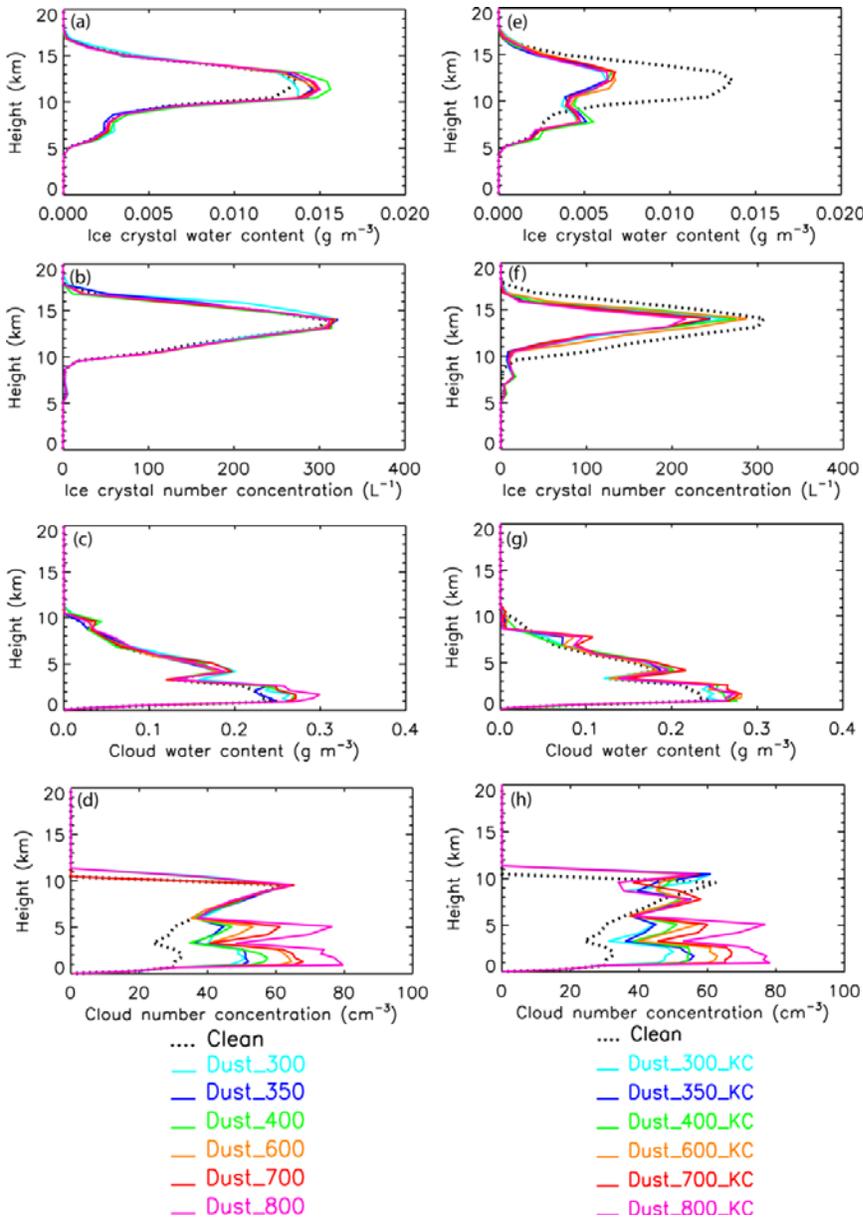
Figure 8 Averaged ice crystal water content, ice number concentration, cloud water content and cloud number concentration over the entire storm area for simulations with the original Morrison scheme (left panels) and KC scheme (right panels) at the time when the CloudSat comparison was made (Fig. 7).

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

The CPR on CloudSat measures the radar reflectivity at 94 GHz. To derive the ice properties, many assumptions must be made, such as the ice particle habit and particle size distribution. Algorithms were also derived assuming clouds having negligible drizzle or precipitation (hence relatively weak attenuation). Therefore, biases are to be expected. Most studies compared CloudSat-derived IWC to either simulated or observed values. Wu et al. (2009) found that the estimated single-profile of CloudSat IWC measurements varies from 0.4 mg/m^3 at 8 km to 1.6 mg/m^3 at 12 km. The CloudSat-derived ice number concentration has an uncertainty ranging from 0 to 250/L. Due to the large uncertainty, we did not compare the CloudSat-derived ice properties quantitatively with the simulated ones. What we emphasized

was that the CloudSat did not detect large amount of ice above 10 km as the original Morrison scheme has predicted, and according to Protat et al. (2009), the higher the altitude, the better the CloudSat retrieval will be.

28. P 14357, lines 2 and 4: l -> 1

Changed.

29. A general comment about the figures: Most satellite images are too small and the text inserts/legends are hardly readable.

We have also noticed that the figures shown in the manuscript for online discussion have very low resolution despite the fact that the our uploaded figures have very high resolution. For the original high resolution figures, one can open the printer-friendly version of paper by clicking the bar on the right side. We also enlarged the text inserts in the figures. We are sorry for the inconvenience that have caused for reviewers.

30. 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 truecolor 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.

The 18 UTC was chosen due to the fact that dust is best revealed at this time when sun lights from the west side. The reviewer is correct that technically, these are not "visible" imageries. These images are now named "pseudo natural color" imageries, which is consistent with the new name for this product. Again, we are sorry that the text is hard to read in the online discussion paper. The main information from the text below the image is about the time when the image was taken, which we have reiterated in the figure caption.

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

The colors denote the area with various OLR. The color bar was located above Fig. 10c. The warmer the clouds, the higher the OLR. For simulations with the KC scheme, there were more low clouds and hence more area with high OLR.

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

We filled the contour lines with color as suggested by the reviewer (please see below). The azimuthal average is the average of values with the same distance from storm center since the storm is near-circular in the end. The x-axis is the distance from storm center. We added this information in the figure caption to clarify.
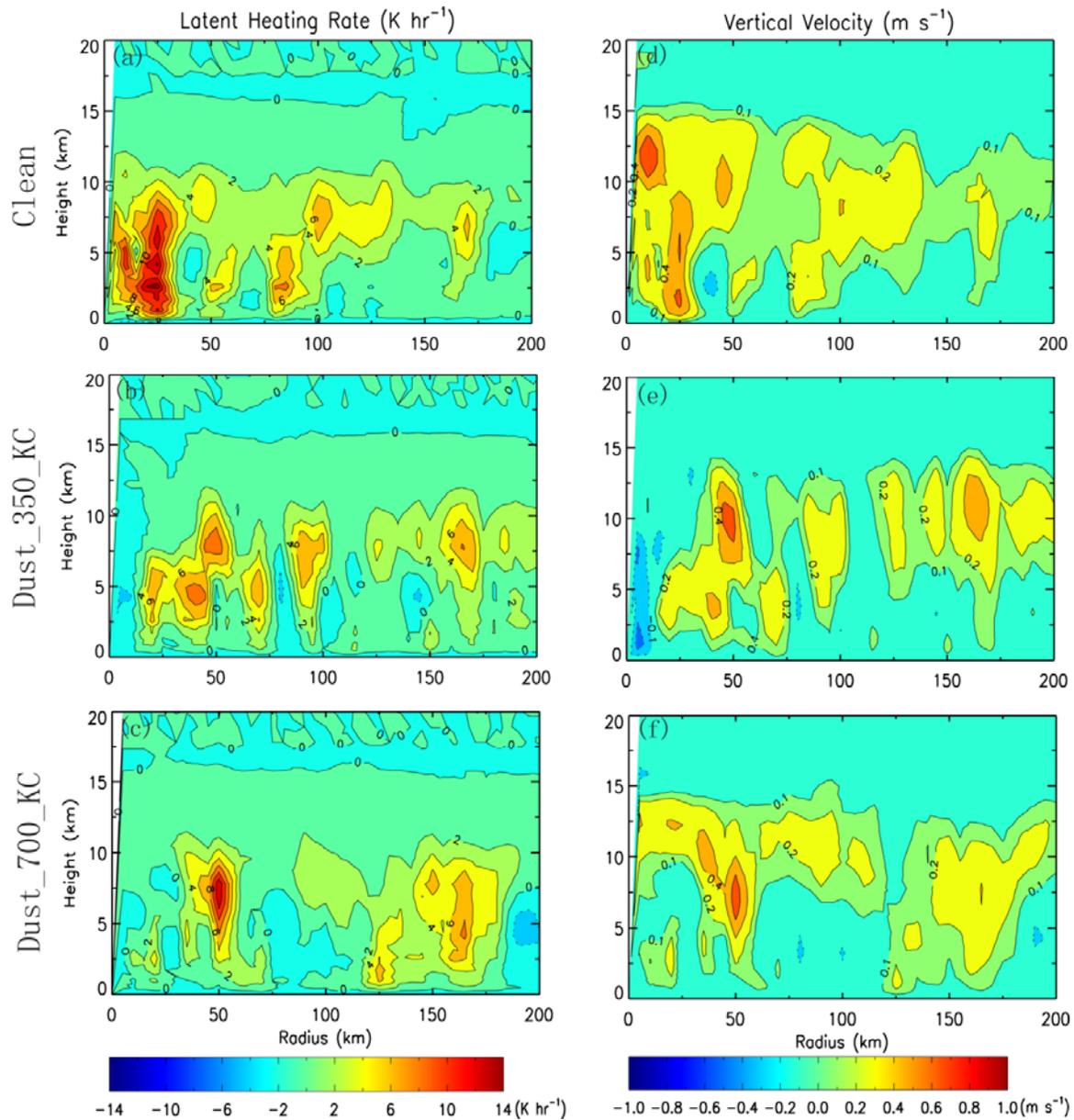
Figure 14. Azimuthally averaged latent heating rate (left panels) and vertical velocity (right panels) for the Clean, Dust_350_KC, and Dust_700_KC simulation at 0000 UTC 14 September. The x-axis is the distance from storm center.

Response to Reviewer #2

General comments:

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.

We deeply thank the reviewer for his/her thorough read of the manuscript and helpful comments that have improved the clarity and quality of the manuscript. The reviewer has raised two main concerns:

(1) Both schemes fail to reproduce the observed cloud statistics to a reasonable degree.

Compared with the CloudSat, MODIS, and TRMM, there are still differences between the observations and simulated results as one would expect. However, the differences are still within a reasonable range as seen in the paper. This is why we need to improve the microphysics parameterizations in the current WRF model. We have tried other microphysical parameterizations, and found that the Morrison scheme give the best estimate for this case. The microphysics schemes in WRF and other forecast models do not consider the effect of aerosols. Our results show that modification in just the ice nucleation scheme makes a difference in cloud characteristics, latent heating budget, and radiation properties, which have altered the overall storm evolution. We also address in detail similarities and differences in the dust impacts acting as CCN and IN among simulations with different ice nucleation schemes. Our results provide important insight into the overall extent of the dust effect and associated uncertainties. We hope more research will be done in this area to provide an improved treatment of dust as CCN and IN and improve the TC forecast.

(2) The model did not reasonably simulate the basic dynamical features of the system.

As suggested by reviewer, we added the comparison of 10 m wind speed between the QuickSCAT measurement and simulations in the revised manuscript (Fig. 5). As seen from the Figure B below, our simulations can reproduce the wind magnitude and distribution reasonably well. The observed final MSLP on September 14 00Z is 1007 hPa. Our simulations at this time produced a MSLP within 986 hPa to 1005 hPa, which is a reasonable range. Therefore, we argue that the model reproduces well the basic dynamics of the system. The storm is moving relatively slow compared to the observed one. We have tried different combinations of physical parameterizations (such as PBL, cumulus convection scheme and microphysics) and started the model from different times. None was able to increase the storm speed. The slow storm movement was most likely caused by biases in the steering flow, which could be resulted from biases in the initial and lateral boundary condition. The storm final locations from the simulations with the KC scheme deviated about 3.2 degree in longitude and 1.3 degree in latitude. Since deviation of

a few degrees in 3-day TC simulation is not unusual (Nolan et al. 2009) and much more variations can be expected in simulated intensities, most TC forecast models now use the ensemble approach.
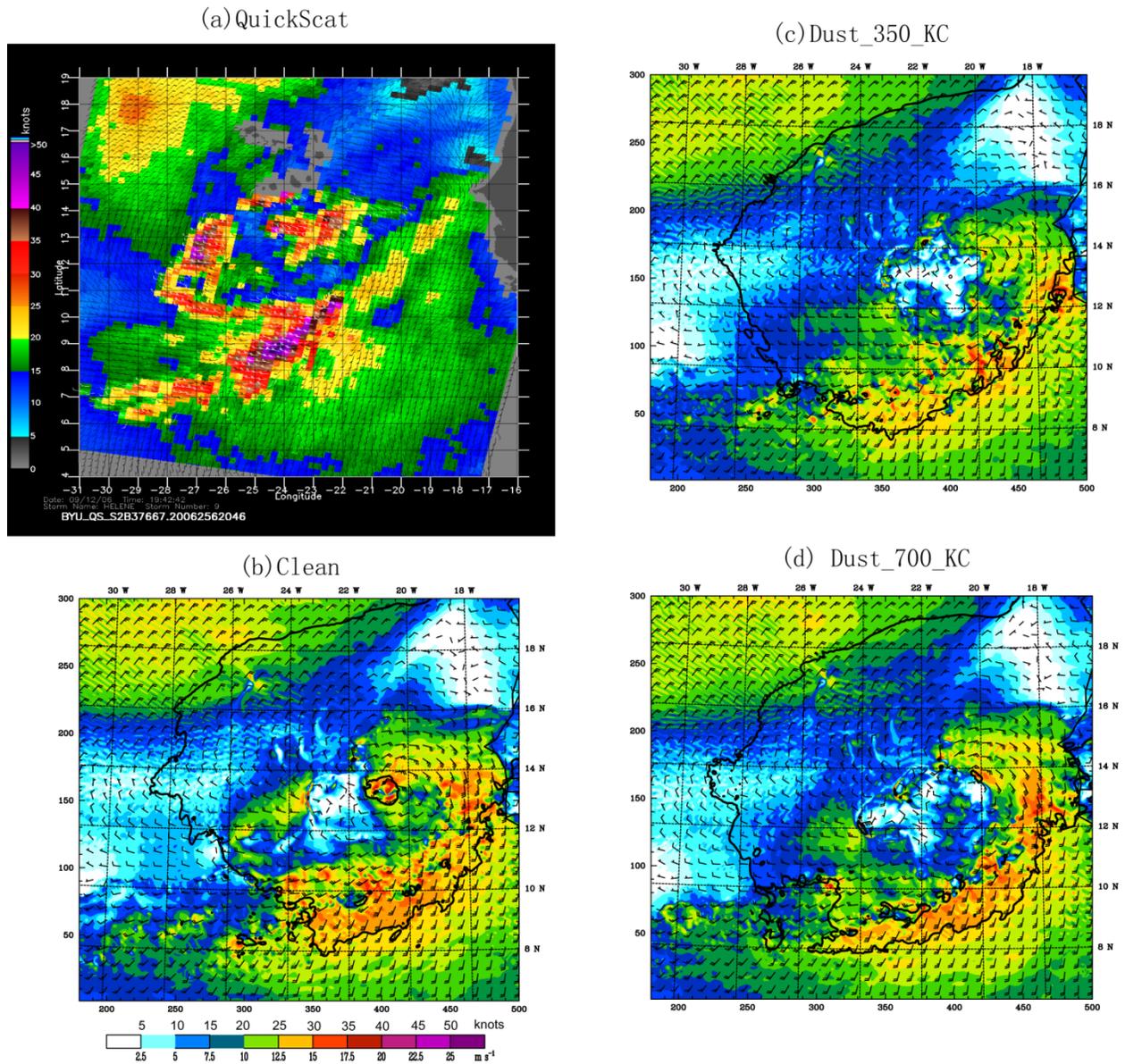

Figure B (Figure. 5 in the revised manuscript) 10-m wind distributions: (a) QuickSCAT data at 1742 UTC 12 September, (b) Clean, (c) Dust_350_KC, and (d) Dust_700_KC at 2000 UTC 12 September.

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.

Yes, we are aware that the vertical velocity is a key to the parameterizations. Please refer to our reply to comment number 22 from reviewer number one.

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.

To address the reviewer's concern, we added the comparison of simulated wind fields with QuickSCAT observations to show that the simulations can reasonably capture the dynamics (Fig. 5 in the revised manuscript). We removed the comparison of precipitation with TRMM observations (original Fig. 12 with a time lag of 12 hours), which will not affect our conclusions. The reason that we have to wait the simulated storm move to the same geolocations as the observed one is that part of the simulation domain is land and part is ocean. Distinct processes will occur on these two different surface conditions. We added this explanation in the revised manuscript: "To evaluate the dynamical structure, simulated 10-m wind distributions were compared against the QuickSCAT data (Fig. 5). The QuickSCAT measurement was taken at 1742 UTC 12 September when the storm has completely moved off the coast of Africa. At this particular time, the eastern part of the simulated storm was still over the African continent due to the relatively slow movement. Given that the surface processes over land may be distinctively different from those over the ocean, simulated wind fields at a later time (2000 UTC 12 September) were used in the comparison, when the storm moved to the same geolocation as the observed one. As seen in Fig. 5, our simulations can reproduce the wind magnitude and distribution reasonably well."

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.

We did not suggest that cloud microphysics is the key for better predicting hurricanes. In our paper, we mentioned that the most important factors for TC development are the storm initial intensity, the thermodynamic state of the atmosphere (such as temperature/relative humidity profiles and wind shear), and the heat exchange with the upper layer of the ocean (Emanuel, 1999) (P14360, line 12). Cloud microphysics is just one aspect that we can improve upon toward better predicting hurricanes. Even though more and more forecasting models are adopting an ensemble approach, all assume a clean background condition. In our conclusion, we stated that "Our study shows that aerosols should be considered as another variable that can affect individual TC's development" (P14360, line 16).

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 in 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?

There was a counterflow virtual impactor (CVI) on board NASA DC-8 aircraft, which assessed the percentage and size of dust particles actually incorporated into clouds. The CVI rejects interstitial aerosol while collecting and evaporating droplets and ice crystals. Therefore, individual non-volatile residual nuclei are retained. The measurements from CVI were presented in Twohy et al. (2009). The size distributions of dust aerosols measured from a suite of instruments were reported in Chen et al. (2011). These two papers were cited in the manuscript. Information about mixing state for aerosols was not available. Kumar et al. (2009) showed that the Köhler theory does not account for the scenario that insoluble particles such as dust aerosols can interact with water vapor and adsorb multiple layers of water molecules under subsaturated conditions (Gustafsson et al. 2005; Vlasenko et al. 2005; Hatch et al. 2008). Dust is also found to be coated with soluble materials such as sea salt and sulfates. We believe that different activation schemes will affect the cloud number concentration and hence the ice nucleation process to a certain extent . However, the conclusions drawn from the comparison of the original Morrison scheme and the KC scheme should still be valid.

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?

The simulations started from a cluster of thunderstorms over the African continent, which was not a vortex with a central location and MSLP. We cannot plot the track and intensity for this period. The storm became a tropical depression on September 12 12Z which was the time when NHC started to record its central location and MSLP. The storm only moved a few degrees after that. Therefore, we used the final locations and MSLP to represent the track and intensity (P14352) and Fig. 10 (Fig. 11 in the revised manuscript) to show the final location and structure of the storms. We thank the reviewer for the suggestion and we included the QuickScat analysis in the revised manuscript.

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?

From sensitivity simulations that we have mentioned in the paper and some additional simulations, variations to small perturbations is comparable to those induced by aerosols. Changing the planetary boundary layer (PBL) scheme also yielded similar variations. The high sensitivity of numerical simulations of TC to initial condition is known as previous studies by Sippel and Zhang (2008, 2010) have demonstrated (both papers were cited in the manuscript). Even though the simulations are sensitive to the initial conditions, our conclusions about the comparison of KC scheme and the original Morrison scheme are still valid. We are working on an ensemble of simulations that will specifically target at the initial condition issue.

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.

Protat (2009) discussed the attenuation of CloudSat. They showed that the rate of attenuation is about 1 dB/km between 9 to 7 km, then increases to 2 dB/km from 7 to 5.5 km and a maximum of -5.5 dB/km at 5.5 km. The attenuation is mainly due to supercooled liquid water in strong updrafts and snow, aggregates and graupels with high densities. A few dBZ may not be detectable on the figure 5. Consider dBZ is the decibel of Z, a 3 dBZ difference corresponds to change in power ratio by a factor of two. The derived ice properties could be greatly affected.