We would like to thank both reviewers for their careful and thought-provoking comments and suggestions. The dialogue has helped us clarify several issues in the revised manuscript. Our responses to the reviewer comments are provided below in blue.

Responses to Reviewer 1

Major comments:
1) The authors have made a noble effort in reproducing the basic state of Kurihara and Tuleya (1981). But is that what they really want to do? The KT81 basic state jet has peak easterly flow at the surface, as does their initial wave disturbance, and these features are well-reproduced in this study. But the real easterly jet has its peak wind speed at 700 hPa (see KT81 Fig 2a), and it is (now) well known that real easterly wave disturbances have their peak flow near this height too (sometimes lower, sometimes higher). If these results are intended to make a "quantum leap" in dynamical understanding of TC genesis, why not use a basic state and perturbation that is much closer to reality?

We chose the same basic state as in KT81 for two reasons: 1) We regard KT81 as a classical study of tropical cyclone genesis. We would like to reproduce the experiment by KT81 to establish the model performance and provide new insights into the pregenesis evolution as seen within the context of the marsupial framework. 2) This mean state captures some basic features of the jet over the West Atlantic.

While it is certainly true that the easterly jet has its maximum zonal winds at 600-700 hPa over West Africa and the East Atlantic, over the West Atlantic the jet is much weaker and closer to the surface. Figure 1 shows the zonal flow averaged between 60-87.5W (western part of the Atlantic Main Development Region) in September from NCEP-NCAR long-term mean. The jet maximum occurs at 925 hPa, and the idealized mean state is broadly consistently with this structure.

On the other hand, we agree that the idealized jet in KT81 differs from that observed in other aspects, such as the broad easterly flow in contrast to the compact jet core in observations. Further numerical experiments with different jet structures, such as the one in Thorncroft and Hoskins (1995) or the one in Hall et al. (2006), are planned but lie outside the scope of the present paper.

2) There are several aspects of the parameterizations and nesting that raise concerns. a) First, regarding the 28 km simulation with BMJ cumulus parameterization, is there no microphysics scheme active at all? This is not clear. If the warm-rain scheme is on, see (b).

The microphysics scheme is turned on for the 28 km simulation.

b) For the high-resolution simulations with warm-rain microphysics, do these also have BMJ on the large grid? That would be somewhat concerning, because there is a mismatch between these two schemes. The BMJ scheme makes precipitation by relaxing the moisture and temperature profiles back to some pre-determined sounding (in some energetically consistent way). But that sounding is based on an atmosphere that does have ice...while the warm rain scheme operates as if ice does not exist.

The microphysics scheme is turned on for all the grids. We agree that the warm-rain

microphysics is not completely `realistic' as it excludes ice physics. That is one of the reasons we did the sensitivity test with ice microphysics. The results are largely similar in many respects.

Finally, we would like to remind the reviewer that the latent heat of fusion is approximately one eighth that of the latent heat of condensation. Therefore, based on simple energetics considerations we would not expect a large mismatch in the BM parameterization by neglecting ice.

c) In both the warm-rain and ice physics simulations, the inner nested grids are not "turned on" until 46 h into the simulation. The authors are to be commended for providing this important detail, and describing the "adjustment" that occurs after the high resolution nests are activated. However, this adjustment occurs at a critical time during the genesis process, especially for the warm-rain simulation. More on this below.

See the response below.

3) The authors point out that these simulations do not appear to have the same sequence of events leading up to genesis as that described by Nolan (2007), most notably an increase in humidity at middle and upper levels before substantial surface vortex intensification. To make this point, a plot of area-averaged RH at 500 hPA is shown in comparison to plots of area-averaged vorticity at 900 and 500 hPa. There are several problems with this:

a) It is stated that the increase in vorticity at 900 precedes the increase in RH at 500. But, RH at 500 does indeed increase substantially from 65% to near 80% from hours 48 to 54 (it may have come up from an even lower value but this can't be seen from the figure). This rise in RH stops for almost 12 hours. But more importantly, both this rise and the interruption are suspect because they occur during the adjustment time after the inner grids have been introduced.

In Fig. 7, we showed the time series of 850 hPa maximum wind speed (a), surface pressure (b), and 850 hPa maximum relative vorticity (c). A temporary increase of the intensity from Hour 26-48 can be seen in all the three experiments. Please note that contrary to the reviewer's assertion, this increase starts *before* the inner grid is introduced. Therefore, this increase is not solely associated with the adjustment after the inner grid is introduced.

b) It seems strange to use RH at 500 hPa, because from the time-height diagrams it is clear that this is the level with the lowest values and slowest increase in RH. Nolan (2007) did not specificy this or any level as the most important, referring mostly to the general saturation of the core at middle and upper levels. Furthermore, it is evident that RH at 500 does reach very high levels after 72 hours. When comparing to Figure 7, this time precedes both the time when the surface pressure begins to fall and the beginning of the rises in maximum 850 hPA wind and vorticity; the pressure fall would be the only "observable" measure of genesis for most cyclones. Although they do not increase faster than the low-level values, note also that substantial increases in mid-level vorticity do occur before "observable genesis" for both the warm rain and ice physics cases.

There are several points to address in this response.

First, the reviewer appears to be using a new definition of "genesis" here as measurable surface pressure falls. This definition is problematic from a dynamical viewpoint. Basic Rossby adjustment considerations suggest that because typical scales of tropical convective systems are small in comparison to the internal Rossby deformation radius the pressure will adjust to the wind field and not the other way around.

Second, if the relative humidity at 500 hPa stays low, the middle and upper levels are not saturated, so the time series of 500 hPa RH is representative. We also checked a layer averaged between 400-600 hPa (see Fig. 2)., and it shows a similar temporal evolution. This should allay the reviewer's concerns about our choosing the 500 hPa level.

Third, we consider tropical cyclone formation as a gradual process, instead of an abrupt or discontinuous change, as suggested also by Ooyama (1982, J. Met. Soc. Japan). In fact, Ooyama on P3 of this paper states the following:

"It is unrealistic to assume that the formation of an incipient vortex is triggered by a special mechanism or mechanisms, or that genesis is a discontinuous change in the normal course of atmospheric processes."

Meteorological observations have long suggested that under normal conditions of the tropical atmosphere the formation process requires a finite amplitude precursor disturbance. This fact is not inconsistent, however, with Ooyama's viewpoint since the ensuing atmospheric processes may act gradually once the finite amplitude disturbance has been created.

As deep convection moistens the air column and spins up the low-level circulation by the attending low-level convergence, it is possible that the formation of a TS vortex occurs after the column achieves saturation. However, we believe it is important to report here that the increase of RH or mid-level saturation does NOT PRECEDE the increase of low-level vorticity. The mid- to upper level saturation is likely a consequence of persistent convection and not a sufficient condition for genesis (although near saturation of the air column certainly creates a favorable condition for further development). We think it is important to emphasize this somewhat subtle but nonetheless important difference.

Fourth, we are interested in the formation of a tropical storm in this study. The rapid fall of the surface pressure is more relevant to the intensification to a hurricane, while formation is manifested as the steady increase of vorticity from Hour 60.

c) There is a mismatch in the RH figures between the warm-rain and ice-physics cases. RH reaches very high values at high altitudes for the warm-rain case, where it does not for the ice-physics case. My guess is that RH is being computed w.r.t. water only in both cases. For the ice case, it is clear that the atmosphere is nearly saturated up to 600 hPa by t = 120 hours, and it may also be as high or higher above if the right RH formula is used. This again is well before "genesis" would be estimated from Figure 7.

RH is calculated w.r.t. water above 0C and w.r.t. ice below 0C as shown in Eq (6) in the

paper.

“Frankly speaking, my advice to the authors would be to abandon the warm rain simulation. It does not offer sufficient simplification (such as allowing the use of analytical techniques) to be justified in the face of the important differences in the structure of convection. If the results of the ice-physics case are basically the same, why not just use them?”

We respectfully disagree with the reviewer on this point. While the warm rain solutions do not offer any obvious advantage from the perspective of obtaining analytical solutions, it is certainly a simpler configuration and furthermore offers the most basic picture of the genesis sequence. We would like to remind the reviewer of the quote by Professor Ian James from his textbook *Introduction to Circulating Atmospheres*: “Absolute complexity is an admission of failure.” We would like also to remind the reviewer that the basic picture of the genesis process simulated here with the ice microphysics scheme is not fundamentally different from the warm rain experiment. We therefore adhere to Occam’s razor and choose to retain the warm rain experiment in this study.

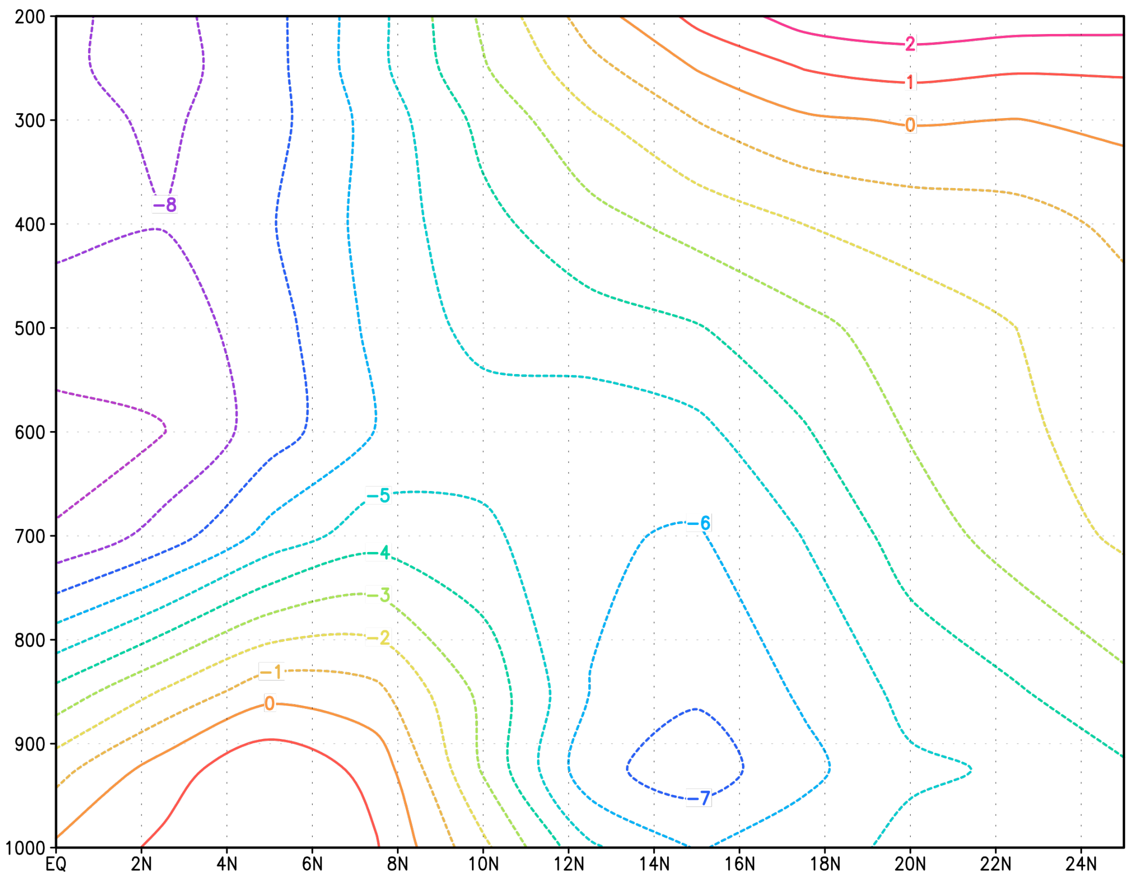

Figure 1 Vertical cross section of zonal wind (m/s) between 60-87.5W in September from NCEP-NCAR Reanalysis long-term mean.

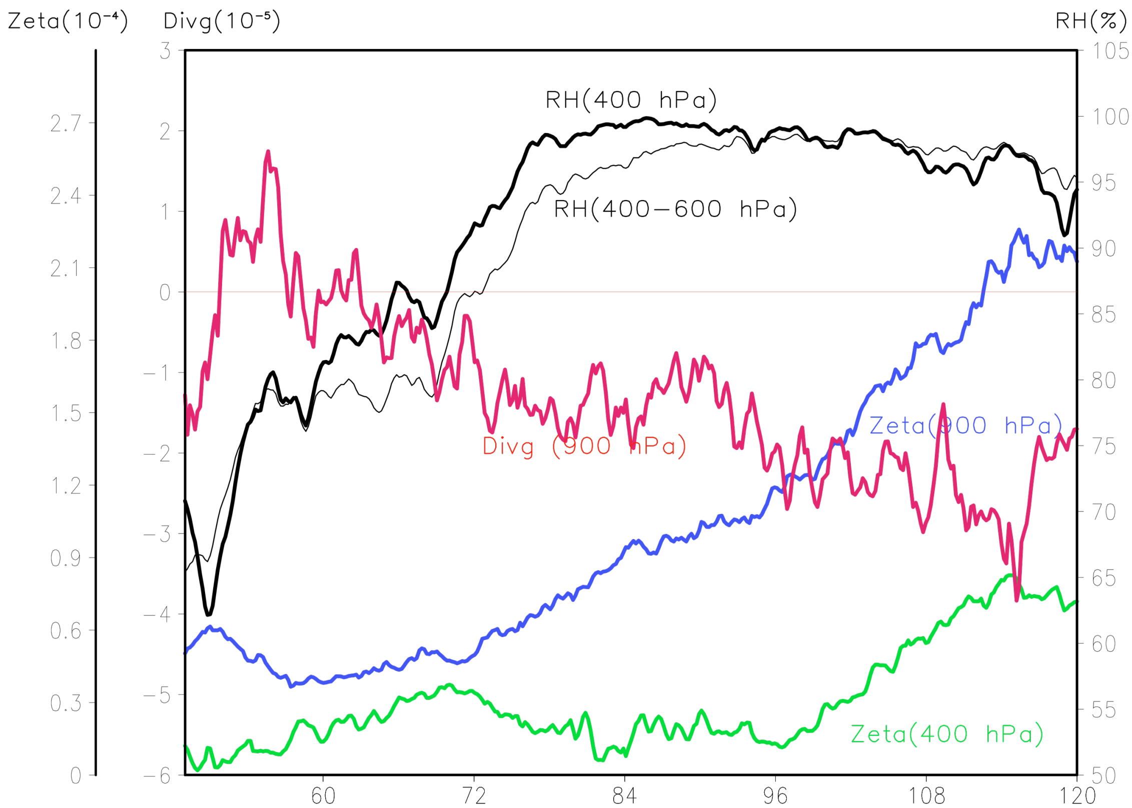

Figure 2 Time evolution of 400 hPa relative humidity (units: %), 400-600 vertical averaged relative humidity (thin black line), 900 hPa divergence (units: 10−5 s−1), 400 hPa and 900 hPa relative vorticity (units: 10−4 s−1) averaged in a 2"°—2" box following the gyre-pouch center from Hour 50 to Hour 120. The RH is calculated with respect to ice below 0°C and with respect to water above 0°C .

Responses to Reviewer 2

Most of the reviewer's minor suggestions have been incorporated in the final version of the paper.

With regard to the comment about the Okubo-Weiss (OW) parameter shown in Figure 9a, the color key suggests that the region where this parameter is positive (in our convention, implying a rotational instead of deformational flow) is larger than the individual centers located within this region. We apologize for the color scheme that tends to emphasize the centers. There is a more important point to be made regarding the calculation of OW in complex flows containing vortical and convective elements, such as in this example. Convection creates horizontal fine structure in vorticity, hence in OW, on scales of the convection itself (meso-γ) and its interaction with neighboring elements. These small-scale variations tend to obscure the spatial distribution of OW on scales of the proto-vortex (meso-β) and tropical wave critical-layer cat's-eye or "pouch" (meso-α). The spatial variation of OW within the cat's-eye is such that rotational flow exists near the pouch center, whereas deformational flow exists near the stagnation point connecting adjacent cat's-eyes, and around the circumference of the pouch. Convection in the latter regions may be just as strong, yet unable to contribute to proto-vortex aggregation, owing to its unfavorable location as diagnosed by OW. Judicious spatial smoothing of the horizontal velocity data prior to computation of OW helps to expose the spatial variations of OW that influence the location of genesis, without the distraction caused by local effects of convection on vorticity.

With regard to the comment that hurricanes cannot be taken for granted -- admittedly a vague statement on our part (!) -- the context points to tropical cyclogenesis, and (in our opinion) a shortcoming that exists in contemporary literature on hurricanes and climate change. This literature does not consider genesis, nor the factors influencing genesis, in the discussion. Rather, the focus has been on hurricane intensity (which may increase in a warmed world) and changes in storm frequency (which may decrease). Much of the discussion is based on a tacit assumption that a link must somehow exist between intensity and frequency, as if stronger storms *imply* fewer storms. Yet we are compelled to regard intensity and frequency as independent factors, or degrees of freedom, in the tropical storm/hurricane record. This statement is based on two facts: (i) the influence of a mature storm on neighboring developments is restricted to a distance less than 1000 km (Schumacher et al., 2009, Weath. & Forecasting) and (ii) genesis places strong constraints on environmental variables within the tropical wave pouch (Dunkerton et al., 2009, ACP). A properly formulated and exhaustive study of tropical cyclones in the warmed world will consider changes in the mean flow, wave excitation and propagation, formation or otherwise of critical-layer pouches, and changes of relevant environmental variables – in each of the respective basins for hurricane formation. Such a study remains to be done. No consensus on this matter can be said to exist until such a study is carried out.