Replies to Reviewer Comments – 2005-058

"A numerical sensitivity study of the extratropical transition of Hurricane Michael (2000)"

Replies to comments are embedded in red text.

Replies to Decision Letter:

I am now in receipt of all three reviews of your manuscript entitled "A numerical modeling study of the extratropical transition of Hurricane Michael (2000)". On the basis of these reviews and my own evaluation, it appears that the manuscript may be acceptable for publication in *Weather and Forecasting* subject to **substantial** major revisions. Acceptance is contingent upon your ability to satisfactorily address the concerns and suggestions of the reviewers. Copies of these reviews are enclosed.

Two of the reviewers commend you on an interesting study. However, all reviewers raised some concerns that deserve attention. Reviewer C is particularly critical and recommended rejection. The main issue is the lack of new insight of how a bogus vortex can improve operational forecasts in general. Both Reviewers B and C have pointed out that you need to address the physical mechanism(s) of how the specific bogus vortex affected Hurricane Michael and how and why this procedure could apply to operational forecasts in general (**more attention has been paid to this aspect…see replies below**). Other modeling issues need to be addressed, e.g., it is possible that your results are influenced by using a one-way nesting approach. The upper-level outflow is clearly larger than the inner domain as indicated by the satellite images. Comparing a two-way nesting simulation will be needed to address this as pointed out by Reviewer A (**focus is on the 12km domain…but we have also tested an even larger domain. Note also that MC2 is not designed for two-way grid interaction**). I request that you carefully consider the comments and concerns of all three reviewers as you revise your manuscript.

Replies to Reviewer A:

Review of the manuscript *A numerical modeling study of the extratropical transition of Hurricane Michael (2000).*

This paper describes a modeling study of the extratropical transition of Hurricane Michael. It describes deficiencies in the numerical guidance at the time, and describes a numerical simulation with a vortex bogusing strategy that the authors argue would have improved numerical guidance of the ET of this system.

Comments:-

This is a nice little study of the impact of bogusing a vortex into a numerical model prior to extratropical transition taking place to ensure that proper structure is present in the initial conditions. The material is appropriate for Weather and Forecasting. I do have a few questions and suggestions to make.

1. Page 7, bottom of page. Which frames of Fig. 2 are you referring to here – be more specific. **Changed to "Fig. 3a" (Fig 2 is now Fig 3)**

2. Page 8, first few lines. I suggest a discussion that doesn't take the figure panels so out of order (**fixed**). For example, Fig. 2e is followed by a discussion that is supposed to happen sequentially after, but references to panels 2d through 2i are made. Do you show the strong baroclinic zone? If not, then mark (not shown) **done**.

3. Page 9. What do you mean by "piloted"? (**as in 3 km grid driven by (piloted by) 12 km output at lateral boundaries**)

4. Page 9, 2nd para. I think you do more than just **two** integrations. I count 16 in your final table. You may use just 2 in your intense analysis, but you should be more specific when you describe this here (**text has changed to reflect not just 2 experiments**).

5. Page 11, last para. Is this a one-way interaction? Would you get better results using two-way interaction? **Most of the results (of the sensitivity experiments) are based on the output from the large 12-km grids which encompass the storm and its environment. Also, the MC2 is not set up for two-way grid interaction. A footnote to this effect is added to the end of section 3.**

6. Page 13. What do you base the statement "..key to a decent numerical forecast.." on? **Removed the statement**

7. Page 14, first para. I think you need to rephrase this. The SSTs do not change, not because the model is not coupled, but because you do not update the SST field. **changed to: "The SST remains fixed during the model integrations".**

8. Page 15. You should place the explanation of how you calculate MSW immediately after your first mention of MSW. (**done**) Your discussion of how you decided MSW in the model highlights the need to make model output variables comparable to that observed or measured in the atmosphere.

9. Page 17, first para. I think you used analyses for your boundary conditions for your simulations? In real time forecast fields provided by ETA or some other system would have been used as your boundary conditions with an almost certain degradation of your forecast. **A new subsection to section 5 entitled "sensitivity to driving fields and grid size" is added to address this.**

10. Page 19. Why don't you sample the model grids through the same region that the dropsonde fell and see if the profiles are representative. If they are, then you do not need your caveat. **Removed the caveat (little change with direct vertical sounding)**

11. Figure 6. I can't see the differences in the tracks – try making them more different! (**done – changed to color and improved resolution**)

12. Figure 9. Choose your grey scale to highlight the important features such as the warm/cold fronts you discuss – I can barely see them in the figures. I also cannot read the numbers or colorbars. **Made colorbar labels larger, and marked warm frontal feature in panel c of the figure.**

13. Figure 9. I do not like the way you have written this caption. It is much too difficult to tell which part belongs to a), b), c). Rewrite this and any others you have done like this in the following manner (suggestion). "Figure 9: a) Manually drawn

(subjective) sea level pressure () valid at 18/19; b) 3-km model control simulation () valid at 19/19 showing sea-level pressure () and surface () wind speed () every 4 m/s; and c) same as b) except with model-simulated surface (40m) temperatures (shaded ever 2C). The estimated position of etc….. **Corrected caption of this figure with suggested format, and modified other captions to maintain consistency... i.e. used the form *(a) description…***

14. Figure 13. the lines are too similar, I cannot pick which is which. **Made some modifications to the lines to better differentiate between them**

Replies to Reviewer B:

Review of paper "A Numerical Modeling Study of the Extratropical Transition of Hurricane Michael (2000)"

Recommendation: Accept with minor revisions

General comments

This is an interesting and well-written paper that investigates the effect of changing resolution and inserting a synthetic vortex to the forecast of the extratropical transition of hurricane Michael (2000). Sensitivity experiments are also performed by changing SSTs to climatology, applying perturbations to the synthetic vortex and changing the convection and microphysics parameterization schemes in the model. The motivation of this study was the poor operational forecast of the extratropical transition of Michael owing to the poor representation of the hurricane in the initial conditions. Increasing the resolution to 3km and inserting a synthetic vortex to represent the hurricane in the initial conditions lead to a great improvement of forecast (both in the track and intensity of the transitioning tropical cyclone). Also the results from the ensemble forecast suggest that cloud processes are important in governing the intensity during the extratropical transition re-intensification period of Michael. However, the authors don't provide a physical explanation as to why this is the case. I think this paper will be of interest to forecasters because it shows the potential of the bogussing method to improve the forecast of extratropical transitions by applying it to a specific case study. The paper would gain by having a broader overview on other cases or other models which already use this method. **A new section in the introduction has been added, summarizing other papers and operational dynamical models employing this and related techniques.** If there were not any studies/models that use this approach they should state that it is a novel approach. More detailed explanations linking the results from the sensitivity experiments to the physical mechanisms involved in the extratropical transition would also broaden the interest of this paper to a wider audience. **The primary focus is to demonstrate where, when and how the modeled storm responds to various parameters which would have a bearing on predictive uncertainty. We only present brief interpretations of some experiments. To delve into the interpretation**

(the “why”) of the many experiments we conducted, would require further analysis, perhaps as follow-up papers.

Major comments

(i) Comments on page 13, paragragh 3:
The idea that the hurricane is the dominant cyclone in the extratropical transition event as described in the abstract, introduction (page 5), in page 13 (paragraph 3) and page 18 (last line1st paragraph) could be misleading. The hurricane is only the dominant cyclone at the beginning of the merging process because the baroclinic low is in its incipient stage. This plays an important role in determining the flow that will advect the systems which in turn will affect the track of the two systems. However, eventually the dominant system will be the baroclinic low (involving upper, as well as lower levels) and not the hurricane. The baroclinic instability dominates the system's growth and the convection associated with the hurricane decays. This should be mentioned at the end of the third paragraph in page 13, emphasizing that the hurricane is only the dominant system at the initial time of merging **(elaborated: … dominant cyclone during the morning and afternoon of 19 October 2000..”)** After all, when you say that the vortex associated with Michael was "captured" by the upper-level low shown in Fig. 2L", it implies that baroclinic low is the dominant system, as expected in the extratropical environment. **Reworded sentence: “The model reproduces the deceleration of the storm after landfall with an eastward motion during the day on 20 October as it was drifting with the deep-layered low shown in Fig. 3l.”** In the summary section, the last paragraph in page 25 should be rephrased consistently taking into account the comments above. **I specified that the hurricane was the dominant circulation center during the early stage of ET at different points in the paper, where it applied.** Was Michael classified as hurricane when the merging occurred? **Yes**

(ii) Comments on page 24, first 2 lines: Could you suggest why cloud processes are important in governing the intensity during the extratropical transition re-intensification period of Michael? Could you link this to the role of the upper-level outflow anomaly? This has been observed to play an important role in some extratropical transition events. For example, Agusti-Panareda et al (2004) showed that the role of the outflow in the extratropical transition of Irene (1999) was to increase the deepening rate and change the direction of the track. **As stated above, the aim of the paper is not to delve into the interpretation of the sensitivities, but to briefly suggest possible reasons or identify that there is sensitivity (important to be cognizant of these issues for forecasting). Removed sentence “This suggests that cloud processes…”**

(iii) Comments on the utility of the ensemble forecast experiments. The variability in the MSLP and MSW from the ensemble system as shown by the one-standard deviation values from the ensemble in Fig. 13 does not seem to account for the discrepancy between the forecast and the best track data. This seems to suggest that the perturbations to the synthetic vortex and changes in model schemes are not enough to explain the errors in the forecast. This should be mentioned in the paper (e.g. second paragraph in page 23 and in the summary in page 27). Could you also suggest other sources of error? **Added paragraph: Note that the variability among members of the ensemble is not enough to explain the rapid intensification period observed between 15 and 18 hours. This is not a surprise since it is well known that numerical models have difficulty forecasting sudden changes in the intensity of tropical cyclones, given that the processes governing these changes occur at scales unresolved by the model (Krishnamurti et al. 2005).**

Minor comments
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1. References

(i). The method of using a synthetic vortex has been applied before to tropical cyclones. The UK Met Office, for example, is using a synthetic vortex to represent tropical cyclones in the operational forecast model. It would be useful to have some references to other operational forecast models that use a synthetic vortex when you mention that the insertion of the synthetic vortex leads to a greatly-improved hindcast of the extratropical transition event (page 5, line 3). **Added a new section to the introduction describing various operational agencies employing vortex insertion / TC bogussing in their models.**

(ii). In 3rd paragraph of page 6, "The small group of numerical modeling case studies focusing specifically on ET" (page 6, 3rd paragraph) should include:

Klein P.M, P.A. Harr and R.L. Eslberry (2002): "Extratropical Transition of Western North Pacific Tropical Cyclones: Midlatitude and Tropical Cyclone Contributions to Reintensification", Mon. Wea. Rev. 130, 2240-2259.

This paper describes the large case-to-case variability in the re-intensification stage of extratropical transitions by considering the midlatiude contributions (how favourable the mid-latitude environment) and the tropical cyclone contribution (whether it plays a significant role).

Agusti-Panareda, A., C.D. Thorncroft and G.C. Craig and S.L. Gray (2004): "The extratropical transition of hurricane Irene (1999): A potential vorticity perspective", Q. J. R. Meteorol. Soc., 130, pp. 1047-1074.

This paper describes the extratropical transition process of Irene (1999) in terms of potential vorticity. The role the tropical cyclone is examined by removing the different anomalies associated with the tropical cyclone in the initial conditions. The role of the upper-level outflow maintained during the extratropical transition by latent heat release is found to play a crucial role in the rapid re-intensification and track followed by the transitioning tropical cyclone.

Although I did not reference these papers specifically, I added to the introductory section some material describing other ET modeling studies using idealized TC vortices in their initial conditions.

2. Although the authors refer to the study of Abraham et al. (2004) for a more detailed description of the synoptic history of Michael (2000), clarifications/more detailed explanations should be added in order to make the text self-explanatory.

(i) In page 8, line 3: How much did the cyclone in intensify between 06/19 and 18/19 (hPa/hour)? **It deepened at a rate of -2 hPa/hr during rapid intensification. Added a new figure showing time traces of SLP and MSW.**

(ii) In page 8, line 7: How different was the "hybrid storm" that made landfall at 2230/19 from a purely extratropical or purely tropical cyclone? The only explanation is that "the storm was rapidly loosing tropical characteristics". **Added cyclone phase space analysis to help describe the transition more objectively (new figures/discussion).**

(iii) In page 8, line 5: Where is the baroclinic zone in the figure 2? **Added "not shown"**

(iv) Page 8. As the extratropical low is a crucial feature in the forecast of this extratropical transition could you mark the position of the extratropical low in Fig. 2? It would be useful to see the relative position of the two low pressure systems before they merge. Where is the extratropical low in the satellite image and analysis plots? **Annotated the figure by marking location of Michael and baroclinic low in the satellite panels.**

(v) In page 8, last 3 lines: Comments on the merging of the two low-pressure systems:
What is incorporated in what? What was the region of upper-level forcing? **More specific rewording: "By 18/19 the sea level pressure center of the baroclinic low (indicating the surface circulation) was becoming less distinct while the primary circulation became centered at the location of the hurricane."**

(vi) Comment on footnote 1: where do you see the dry air (early 19 Oct 2000)? The advection of dry air cannot always completely indicate the onset of the extratropical transition. Is the advection of dry air into the storm a result of an interaction with a baroclinic zone? What was the date in which the NHC declared Michael as extratropical? **Footnote no longer necessary with the addition of the cyclone phase analysis.**

(vi) As the relative location is important for the merging and translation of the two low-pressure systems, it would be good to have the track of the hurricane together with that of the baroclinic low in Figure 5. **A segment of the hurricane track has been added to the figure.**

3. Comments on model simulations

(i) Last paragraph of page 13: Why is the vortex-initiated control simulation not done for the 24 km resolution ? **Such a simulation was never contemplated…primarily because 24 km horizontal resolution is quite coarse for hurricane simulation, and we wish to work toward developing a higher resolution limited area model applicable to forecast applications.**

(ii) Page 15: "The model does not capture the rapid intensification". Do you know why the model is not simulating the rapid deepening rate of the cyclone? **This is the case in terms of MSLP, but the 3-km run shows rapid increase in winds over a short time (although not at the right time). It is very well known that models have difficulty simulating intensity of TCs – especially rapid intensification. Although not included in the paper, a reference on this is:**

Krishnamurti, T.N., S. Pattnaik, L. Stefanova, T.S.V. Kumar, B.P. Mackey, A. J. O'Shay, and R. J. Pasch, 2005: On the hurricane intensity issue. *Mon Wea. Rev*., 13, 1886-1912.

(iii) Page 17, 2nd paragraph: What is the main difference between the GEM regional forecast model and the MC2 model? Why didn't you use the GEM regional forecast model to do all the experiments with the synthetic vortex? **The primary difference in the models is the horizontal resolution, and the fact that GEM is on a global grid. The dynamical codes are somewhat different. However, a new subsection to section 5 (*b. Sensitivity to driving fields and grid size*) has been added where the MC2 model is piloted (driven) by output fields from GEM forecasts.**

(iv) Page 18, 3rd paragraph, 3rd line: The tight gradient in temperature in Fig. 9c seems to be further north than the manually drawn surface front in the analysis (Fig. 9a). **True. Modified text: "The warm front in the analysis in Fig. 11a is represented reasonably well in the simulation (but a bit further north) in Fig. 11c as denoted by the tight gradient in temperature and marked in the image." (Fig. 9 is now Fig. 11)**

(v) Page 19, first line: Is Michael still classified as hurricane at 18/19 by the NHC? **Yes. …and still not completely extratropical as denoted by cyclone phase space.** The equivalent potential temperature anomaly shown in Fig. 11 at 19/19 indicates that the vortex is decoupled from the surface (as mentioned in page 20, line 13).

(vi) Page 19, last 3 lines: "the model does correctly indicate that strong winds extend through a deep layer of the atmosphere on the east side of the rapidly-moving storm". This deep layer in the model is too deep in the model as the wind speed decreases rapidly above 2000 m in the dropsonde. **True enough. Removed 'correctly'. It is still true that the model produces a deep layer of strong winds…in this case…even deeper than reality.**

Replies to Reviewer C:

I. Recommendation:

Reject. The authors revisit the problem of poor operational forecasts of Hurricane Michael (2000) in Canada. **We have not published any papers specifically addressing the forecast problem associated with this event.** First, they establish that these forecasts were poor for Michael as part of a synoptic overview. Second, they conduct a new numerical simulation in which a bogus vortex is added prior to running the model. Not surprisingly, they get a better forecast (but what does this prove?). **It shows that vortex insertion prior to ET gives a foundation upon which to conduct sensitivity studies and test the applicability of this particular model (MC2) as a prognostic tool.** They also conduct some rather ad hoc ensemble forecasts and SST sensitivity forecasts. The bottom line from this effort is that no new scientific findings and insights are advanced and no new operational procedures and techniques are presented. **We believe that this sort of detailed numerical modeling study of ET (in hindcast mode) helps us to establish a feel for challenges related to forecasting ET, and we feel this type of study is appropriate for WAF. ET is a known forecast problem (Jones et al. 2003), so we feel this research helps work towards improving our ability to forecast these types of storms in the future.** Accordingly, a reluctant recommendation to reject the manuscript is made.

II. Particular Comments:

1. Section 1 is cursory and routine, and reads more like an advertisement for previous research than a road map for a new investigation. **New section added with thorough backgrounder on modeling approaches.**

2. Why is Hurricane Juan (2003) mostly ignored in section 1? If I am remembering correctly, the operational model forecasts were poor for that storm too. Why were they poor and how might that relate to the poor Michael (2000) forecasts? **References on work related to Hurricane Juan using vortex insertion have been included in section 1.**

3. Section 2 is also very cursory with lots of hand waving. Surely a phase diagram (Hart and Evans 2003) would make for a nice addition here. Statements are offered without proof (e.g., track of the baroclinic low relative to the tropical cyclone; that the storm was a "hybrid", whatever that means). **This section has been expanded/improved with inclusion of wind and pressure traces from the best track, and a cyclone phase space analysis.**

4. In section 3b what does "an observationally consistent, TC-like vortex" mean when there are no observations available? **Rephrased: "The initial atmospheric fields are modified by inserting a synthetic TC vortex constructed prior to running the model"** Furthermore, what evidence is there that the vortex is symmetric (please don't answer the question by saying that it looks to be symmetric on the basis of the satellite imagery)? **Of course, no tropical cyclone is perfectly symmetric. In the absence of detailed aircraft data at the time we are initiating the model, an assumption of general moist symmetry is made.**

Note also that most agencies including the GFDL are initiating their dynamical TC models with a very idealized vortex which is not a perfect representation of reality.

5. How do we know that 12 h is "sufficient time" for the model to adjust to the hurricane in section 3b? **It is somewhat subjective, but we elaborated by adding: "(e.g. spurious behavior in wind and pressure fields ceases after a few hours of integration)"**

6. In section 4a the possibility of binary interactions and whether they would be consistent with the so-called observed movements seems not to have been considered. **No, we do not consider binary interaction per se…but concentrate on the primary issue that the hurricane became the dominant center early in the transition. In Abraham et al. (2004), more discussion on the observations associated with the interaction is presented.**

7. In section 4b, why would the storm move east if it was "captured" as the authors suggest. A binary interaction should feature cyclonic rotation of the players. **Reworded the sentence discussing the "capture": "The model reproduces the deceleration of the storm after landfall with an eastward motion during the day on 20 October as it was drifting with the deep-layered low shown in Fig. 3l"**

8. To assert that the bogus vortex experiment was a success in section 4c seems premature. At the very least, forecasts from other centers (UK Met Office, ECMWF, NCEP, US Navy) should be examined and discussed. **Section 4c was 'pared down' and reference to forecasts was included in a later (new) subsection in section 5, which now includes a discussion of operational predictions for this event. The MC2 hindcasts are discussed in the context of the operational guidance.**

9. Assertion of continuing warm-core structure in section 4c in conjunction with the discussion of Fig. 9c is not at all obvious. **Reworded: "The storm would be considered as purely extratropical if the tight temperature gradient (front) extended into the center (as is the case with the baroclinic low in Fig. 11c). A similar pattern in temperature is present above the boundary layer (not shown) indicating that this pattern is not confined to the surface."** Have we learned anything new about the ET process as opposed to the model? **There are not necessarily any new findings with regard to physical processes…but the aim of the paper is to analyse the particular case in the framework of a numerical model, which we believe would be of interest to the WAF audience.** Why doesn't the vortex tilt downshear in Fig. 11? **It does tilt downshear, but only discernibly so in the lower levels, and only for a certain period before landfall. Added some more explanation, and also specified in section 6 that the eastward tilt was discernible below 500 hPa: "As ET continues, the warm core (below 500 hPa) changes from having a westward tilt, to an eastward tilt prior to landfall, then to a northwestward tilt after landfall. This is not easily discerned from the panels in Fig. 13, however, it is clear that the deep tropospheric tilt is westward to northwestward, highlighting the extratropical nature of the cyclone."**

10. Section 5a seems strictly ad hoc. Is this the best way to generate ensemble members? I doubt it. A lengthy, inconsequential discussion follows. **The ensemble has been expanded to include some tests of grid size and piloting boundary conditions on simulated storm evolution. The title of the paper has also been changed to reflect that this is a *sensitivity study*. We feel the choice of members represent realistic situations, relating to typical measures of uncertainty of the storm initial conditions and model settings.**

11. The SST sensitivity results from a section 5b are just not believable. **Why are they not believable? In any case, we have concluded that the landfalling intensity was not overly sensitive to the (relatively) small departures of the SSTs from climatology. This is not surprising since the environment is most conducive to baroclinic intensification of the storm as opposed to tropical-type intensification (through oceanic heat transfer). Added some text to this effect.**

12. Section 6 offers speculations as conclusions while failing to offer any new scientific or operational insights. **Results of the comparison with operational forecasts are added to the conclusions. Also, keeping in mind that this is a sensitivity study, we feel the summary and conclusions presented are consistent with the theme of the paper.**