*Response to comments of **Anonymous Referee #1** on*

Sensitivity of simulating Typhoon Haiyan (2013) using WRF: the role of cumulus convection, surface flux parameterizations, spectral nudging, and initial and boundary conditions

Delfino et. al.

RC1: 'Comment on nhess-2021-400', Anonymous Referee #1

Reviewer 1's COMMENTS	Authors' RESPONSES
General comments	
It is an interesting and well-written article that investigates the impact of (a) two different cumulus convection schemes (Kain-Fritsch and Tiedtke), (b) three surface flux formulations, (c) spectral nudging and (d) initial and boundary conditions from ERA deterministic and Ensemble of Data Assimilations system, on the WRF simulations of super Typhoon Haiyan (2013) in Western North Pacific. The model results are compared against the International Best Track Archive for Climate Stewardship, satellite data and ERA5 re-analyses.	Dear Referee, Thank you very much for the overall positive feedback on the submitted manuscript and for giving us the opportunity to submit an improved version of the manuscript. We appreciate the thoroughness and objectiveness of the comments and have addressed the specific concerns raised. And all changes are highlighted in the revised manuscript. All line numbers refer to the revised manuscript with tracked changes. Please see below our specific responses and refer to the attached revised manuscript and supplementary file for more details.
The use of English is very good. The figures/tables are clearly produced and necessary. The abstract is concise and the conclusions are supported by the results.	
It is suggested to accept this article for publication after some minor corrections are performed.	
Suggested corrections:	
Section 2.4: (a) Did you use one or two-way nesting? (b) Please justify the location of the southern boundary of the inner domain so close to the track of the tropical cyclone. Errors from the boundary conditions are expected to influence the simulation. (c) Why did you extend the inner domain so much north of the track? Please justify it in the manuscript. Was it necessary in order to simulate appropriately the subtropical ridge? (d) Please clearly state whether all the model results of this article are based on the output of the inner domain.	(a) Two-way nesting was used to allow interaction between the outer and inner domain. This has been indicated in the manuscript (Page 6, Lines 236-239) (b) Southern boundary – the overall approach of this study is to have a common domain for multiple TC cases in this region (other TC cases not included in this paper, but are the focus of a follow-on paper, about to be submitted) to understand and have a more general set of conclusions on the response of TCs to future warming. We conducted several sensitivity experiments on different domain configurations and specific experiments with adjusted southern boundaries were also conducted (but for a different TC case that

	tracked further south) and it was found that the current domain configuration was optimal in terms of simulated tracks and intensity. Indicated in the manuscript (Page 6, Lines 241-244). *Kindly see Supplementary Figure 1 for more details.* (c) The northern boundary of the inner domain was also designed to consider multiple TC cases (and for further experiments, not included in this paper) that made landfall to the north of the Philippines and to appropriately simulate the subtropical ridge/Western North Pacific Sub-tropical High and Northeasterly winds. Indicated in Page 6, Lines 241-244 in the revised manuscript. Model results indicated in the manuscript are outputs of the inner domain and this has been indicated in the revised manuscript (Page 6, Line 238)
Lines 167-170: How do you explain your result that the simulation with the longer lead-time was the best?	Experiments with different lead times have been conducted prior to the selection of 04 Nov 00 UTC as the initial time (longer lead-time). Other experiments include 04 Nov 06 UTC, 12 UTC, 18 UTC; 05 Nov 00 UTC, 12 UTC; 06 Nov 00UTC, 12 UTC; and Results of these experiments showed that this chosen initial time with longer lead-time is able to simulate the observed track and intensity better than later times. The longer lead-time was used to allow for the simulation of the early stages of development of Typhoon Haiyan, as also used by Nakamura et al. (2016) for Typhoon Haiyan under present-day and future-climate simulations and associated storm surge. The model initialized at 04 Nov 00UTC and 07 Nov 00UTC have simulated tracks closer to observed (IbTRaCS). In addition, when comparing the simulated and observed intensity (minimum sea level pressure and maximum wind speed), it can be noted that in the time series of the simulated intensities, the model takes longer to develop than the observed. This is often seen in regional modeling / limited area modeling, seems to indicate that the model often requires a spin-up period, for example a 36-hour spin-up period was also implemented by Cruz and Narisma 2016 in simulating Tropical Storm Ketsana, to reduce the effect arising from imbalances between the simulated results due to the model physics (microphysics, planetary boundary layer, cumulus) and the initial and boundary conditions (Chu et al., 2018). This behavior was found to be related to the planetary boundary and surface layer parameterizations in WRF (Maldonado et al., 2020) and the time needed for initialization can also be affected by the size of the domain and terrain conditions (Chu et al., 2018). Thus,

	we considered the time between 04 Nov 00UTC to 05 Nov 12 UTC as the spin-up period (first 36 hours of simulation and at this period Haiyan was observed to be just developing from a tropical depression to a tropical storm) and the results presented in the manuscript covers the analysis period between 5 Nov 18 UTC to 8 Nov 18 UTC to cover Haiyan's mature stage. We have added a few lines in the revised manuscript (Page 7, Lines 253-255). *Kindly refer to Supplementary Figure 2 for more details.*
Line 182: Was the cumulus convection scheme employed in both domains? Please state it clearly.	Yes, we have used the cumulus schemes in both 25km outer and 5km inner domain. We have indicated this in the revised manuscript (Page 7, Line 268).
Lines 289-290 and 297-298: the mean DPE of KF simulations is not the same in the former and latter lines. The same happens for the TK simulations. Please make the necessary corrections and update lines 562-563 accordingly.	Thank you for pointing this out. The indicated DPEs in Lines 289-290 (Page 11, Lines 390-391 in revised manuscript) were the mean throughout the simulation period and not the analysis period. We have removed this line and retained the correct figures in Page 11, Lines 390-391 and in the conclusion (Page 24, Lines 687-688).
Figure 3, x-axes: is it the simulation time or the 72-hour verification time (as it was stated in line 171)?	Apologies for the confusion. We have revised the time axis of Figures 3, 4 & 5 to reflect the analysis period between 18 UTC 5 November 2013 to 18 UTC 8 November 2013. All experiments were initialized at 00 UTC 4 November 2013.
Line 319: in Figure 4 the control simulation (KFsnOFFsf0) has a minimum mslp of about 940 hPa (not 934 hPa) and maximum wind speed less than 50 hPa (not 53.69 m/s).	Thank you again for pointing this out. Same with the issue on the DPE and we have indicated the correct figures in the revised manuscript (Page 12, Lines 422-423). It now reads: *"The control simulation (denoted as KFsnOFFsf0) has a MSLP value of only 939 hPa and maximum wind speed of 43.47 meters per second (ms-1). Compared to the minimum central pressure of 895hPa and 73 ms-11-min sustained wind speed in the observations, this is a difference of 38 hPa and 29.53ms-1, respectively."*
Figure 4: For consistency with the symbols of the other experiments, it is suggested to change the pattern of TKsnOFFsf1 to dotted line. In the current figure it is difficult to distinguish it from TKsnOFFsf0.	As suggested, we have revised the figures for better representation of the different experiments. Kindly refer to the updated Figure 4 (Page 13) in the revised manuscript.
Lines 349-350: in figure 6 the RMSE of KFsnOFFsf1 is about 10 m/s and its correlation is between 0.8 and 0.85 (i.e. lower than 0.89).	Thank you for spotting this. We have indicated the correct figures in the revised manuscript (Page 15, Lines 458-459). It now reads: "*Of all the simulations, the simulation with the combination of KF and sf1 without nudging have the lowest RMSE (22 hPa MSLP and 9.59 ms^{-1} maximum winds) and highest correlation coefficient of 0.78 and 0.82 for MSLP and maximum winds, respectively."*

Lines 351-352: in figure 6 the RMSE of TKsnONsf0 is about 15 m/s and its correlation is about 0.69.	Thank you for spotting this. We have indicated the correct figures in the revised manuscript (Page 15, Line 459-461). It now reads: *"While the simulation with the poorest performance i.e. highest RMSE (37 hPa and 14.17 ms^{-1}) and lowest correlation coefficient (0.60 and 0.69 for MSLP and maximum winds, respectively) is the simulation with the combination of TK, sf0, with spectral nudging turned on."*
Line 409: The simulation with the closest landfall time is not shown in Table 3, but it can be derived by Figure 11 (as far as the experiments without spectral nudging are concerned).	Thank you for spotting this. We have indicated the correct figures in the revised manuscript (Page 17, Line 518).
Line 464: Please justify your choice to present only the runs without nudging in figure 11.	For improved readability, we have chosen to present the experiments without nudging to represent the TC-associated rainfall in the different experiments. Similar rainfall patterns were found in the experiments with nudging as shown in Supplementary Figure 4.
Line 488: the steering flow bias has not been shown in figure 12.	Thank you for pointing this out. We have removed this line in the manuscript (Page 21, Line 600).
Figures 12 and 14: Did you interpolate the WRF output to the coarser ERA5 grid? Which interpolation method did you use? Please include this information in the article.	Yes, the 6-hourly WRF output was interpolated to the coarser ERA5 grid using First-order Conservative Remapping through CDO's remapcon function. We have specified this in the revised manuscript (Page 21, Lines 611-613; Page 22, 645-647; Page 23, Lines 665-667)
Figures 12, 13, 14: (a) Please justify the use of the KFsnOFFsf1 and TKsnOFFsf1 experiments instead of all the KF and TK runs. (b) are these figures based on 6-hourly ERA5 and WRF output?	(a) KFsnOFFsf1 and TKsnOFFsf1 were used in this section to represent the experiments with KF and TK runs, primarily to save on space but more importantly, similar results were found in the average of the experiments using KF and TK as cumulus convection scheme. *Kindly refer to Supplementary Figures 5,6 and 7.* (b) Yes, these are based on 6-hourly ERA5 and WRF output values. We have indicated this in the revised manuscript (Page 21, Lines 611-613; Page 22, 645-647; Page 23, Lines 665-667)
Lines 501-502: Please clarify in the article whether the vertical wind shear was computed (a) from time-averaged u and v winds at 200 and 850 hPa (i.e. firstly calculating the time-averaged u and v at each grid-point and then using them to calculate the vertical wind shear), or (b) by averaging the instantaneous values of the vertical wind shear (i.e. firstly calculating the	The vertical wind shear was re-computed by averaging the instantaneous values of the vertical wind shear (i.e. firstly calculating the instantaneous vertical wind shear at each grid-point and then calculating its time-average value) (kindly see revised Figure 12). We specified this in the revised manuscript (Page 22, Lines 619-620)

instantaneous vertical wind shear at each grid-point and then calculating its time-average value).	
Line 536: (a) do you mean that KF shows a higher relative humidity along the track? Otherwise, it disagrees with the previous discussion in this paragraph. (b) for clarity it is suggested to draw the tracks of the simulated and actual tracks on both panels of figure 14.	(a) Thank you for pointing this out. We have corrected this observation in the manuscript (Page 23, Lines 657-658). (b) We think this is a great suggestion so we have revised the figure to show the simulated tracks. Please refer to the new Figure 12 in the revised manuscript.
Technical corrections:	
Line 152: "… and model physics (Isaksen et al., 2010)."	Revised in the manuscript (Page 6, Line 230)
Line 158: "… different parameterization …"	Revised in the manuscript (Page 6, Line 239)
Line 165: It is a 180-hour period (not 174-hour) from 00 UTC 4 November to 12 UTC 11 November.	Revised in the manuscript (Page 6, Line 248)
Line 175: "… is bounded by 100-170 degrees East …"	Revised in the manuscript (Page 7, Line 260)
Line 251: "… maximum 10m winds to evaluate …"	Revised in the manuscript (Page 10, Line 352)
Line 268: "… relative vorticity maxima …"	Revised in the manuscript (Page 10, Lines 368-369)
Line 286: "… without nudging (snOFF) …"	Revised in the manuscript (Page 11, Line 387)
Line 312: "… of the DPE (km) …"	Revised in the manuscript (Page 12, Line 414)
Line 473: "… the KF scheme shows …"	Revised in the manuscript (Page 20, Line 582)
Lines 496, 527, 543: KFsnOFFsd1 and TKsnOFFsd1 must be corrected to KFsnOFFsf1 and TKsnOFFsf1, respectively.	Revised in the manuscript (Lines 610, 645, 665)

References

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Cruz, F. and Narisma, G.: WRF simulation of the heavy rainfall over Metropolitan Manila, Philippines during tropical cyclone Ketsana: a sensitivity study. Meteorology and Atmospheric Physics, 128(4), 415–428. https://doi:10.1007/s00703-015-0425-x, 2016.

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