*Response to comments of **Anonymous Referee #2** 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.

RC2: ' https://https://doi.org/10.5194/nhess-2021-400-RC2', **Anonymous Referee #2**

Recommendation: **MAJOR REVISION**

Reviewer 2's COMMENTS	Authors' RESPONSES
General comments	
The authors utilized WRF-ARW to simulate Typhoon Haiyan and investigate the role of cumulus convection (KF and TK schemes), surface flux parameterizations, spectral nudging, and initial and boundary conditions (ERA5 and EDA). They concluded that the TK scheme and spectral nudging improve track simulations with lower mean DPE than the other model configurations. On the other hand, KF scheme and varying the surface flux options improve the intensity.	Dear Referee, Thank you very much for highlighting the importance of our work, the useful feedback on the submitted manuscript, and for giving us the opportunity to submit a much improved version of the manuscript. We have addressed the major and minor concerns raised. All changes are highlighted in the revised manuscript and line numbers refer to the revised manuscript with tracked changes.
This type of study will definitely be of a great addition to works that optimize a model's configuration of TC simulations in the Philippines, but in its current form is not yet ready for publication. Major parts of the paper should be rewritten due to the following major concerns**:**	Please see below our specific responses and refer to the attached revised manuscript and supplementary file for more details.
Major Concerns:	
1. (Line 55~Line 105, Line 125…) Although a future plan for conducting pseudo-global warming simulations was mentioned, WRF-ARW was used in the paper as a numerical weather prediction (NWP) model to simulate a weather event (TC Haiyan). However, the literature review (introduction) seems to interchange regional climate models (climatological simulations) with numerical weather prediction models (short-term weather events) resulting in mixed and improper	Thank you for pointing this out. The overall approach of the study is that we have used WRF configured as NWP to get the best configuration for hindcast TC case simulations and eventually use that configuration to simulate the TC cases with future climate forcings. The results included in this paper are from the former i.e. as a sensitivity study using Typhoon Haiyan as the TC case. We have revised the manuscript to make the distinction clearer i.e. studies with NWP event-based hindcast simulations to build a foundation on sensitivities to

citations of papers that use RCMs and NWPs. Event simulations are different from climatological runs. Although WRF and other NWPs can also be used as RCM, they are usually modified to efficiently work for climatological simulations (e.g. CLWRF, RegCM -- RCM version of MM5, NHRCM – RCM version of JMA/MRI NHM). NHRCM, and not WRF, is the model used by Cruz et al., 2016 in Line 132.	model parameterizations and settings. We have also cited some studies using WRF as LAM with future climate forcings as initial and lateral boundary conditions in support of the rationale behind the bigger study. Significant revisions were made in Pages 2-4, Lines 55 – 170 in the revised manuscript. Apologies for this mistake. Cruz et al 2016 should read Cruz and Narisma 2016. We have revised this in Page 1, Lines 71-72 and included in references of the revised manuscript.
The paper literature review should focus on studies that conduct TC short-term simulations using models (e.g. WRF, NHM) that are considered as NWP and not RCM. The literature review also fell short in terms of discussing studies that tackle the other sensitivity parameters such as spectral nudging, surface flux, and ICBC. The reviewer hopes to see a clearer revised Introduction with an additional review on the said parameters.	We have included additional discussion in the introduction, particularly that of surface flux options e.g. from a study by Kueh et al., 2019 using WRF (Page 3, Lines 88-102 of the revised manuscript). Additional studies on ICBC (Islam et al., 2015; Mohanty et al., 2010; Shepherd and Walsh, 2016) and spectral nudging (in WRF as NWP Mori et al., 2014; Kueh et al., 2019 and as RCM Shen et al., 2017; Cha et al., 2011) have also been added in the introduction section (Pages 2-3, Lines 88-123).
2. The objective and analysis of this paper are very promising but the initial forcing is also very critical to consider it as a sensitivity analysis. Kindly clarify if the researchers downscaled only one mother domain (D1) for all D2 sensitivity runs? If not, then it will be inappropriate and difficult to compare the sensitivity of TC track and intensity to parameterizations if the initial forcing (D1) for each experiment have different model physics. This might explain the different (or larger differences of) values of intensities at t=0 in Figure 4. The reviewer strongly suggests to reconsider rerunning all simulations using only one D1 simulation as forcing to all D2 experiments.	Thank you for these clarifications. There is only one mother/outer domain (D01) and child/inner (D02) domain and the same domain settings were used in all the sensitivity experiments (as shown in Figure 1 of the submitted manuscript). The same physics parameterizations were also used in both outer (D01) and inner (D02) domains. We have explicitly indicated these in the text (Page 6, Lines 235-245) and in Table 3 of the revised manuscript. Since we are using two-way nesting and there is feedback from the outer to the inner domain and vice versa, it is important that the same physics parameterization is used in both domains. This is the used in WRF with multiple and nested domains (Werner and Wang, 2017; Dudhia 2015), as there could be issues with two-way nesting when physics parameterization differs across the nest boundaries (e.g. in precipitation fields of the mother/outer domain) (Dudhia 2015) and used in past studies (e.g. Wang and Wang, 2014; Islam et al., 2015). The physics parameterization, particularly the cumulus scheme, was changed in each sensitivity experiment in both domains. 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 (t=0). The different values of intensities at the start of the analysis period (18 UTC 5 November 2013) is expected since there has already been interaction between D01 & D02. The same initial conditions were used for D01 and D02. There is no difference in the simulated intensity (MSLP = 1005hPa; max winds = 17 m/s) at t=0 (04 Nov 00 UTC) for both mother/outer domain (D01) and child/inner domain (D02) for all sensitivity experiment. Kindly refer to Supplementary Figure 3 for more details. Given this clarification, there is no need to rerun the simulations.
With this 2nd major concern, it will be difficult to give meaningful comments on the results and discussions.	Given what we have explained above, there is no reason for the 2nd concern.
3. (Line 155-163, 166). Kindly provide supplementary materials for the results of the other domain configurations that led the authors to select the control run model setup. These supplementary materials are very important to justify the model setup of the control run.	Thank you for this suggestion. We have included some figures in the supplementary material. Initial simulations have been done to check model performance using different domain configurations and horizontal resolution i.e. (a) single domain (at 12km horizontal resolution); (b) two domains (at 12 and 4km horizontal resolution); (c) same as (b) but with bigger inner domain; (d) three domains (12, 4 and 1.3km horizontal resolution); and (e) two domains (25, 5km) horizontal resolution. Domain configuration (e) was used for the sensitivity experiments which simulated the lowest minimum sea level pressure and maximum winds, and in consideration of computing resources and other TC cases that were simulated in the project. Kindly refer to Supplementary Figure 1a for more details. Experiments with different lead times have also been conducted prior to the selection of 04 Nov 00 UTC as the initial time (longer lead-time) as well as 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) Kindly refer to Supplementary Figures 1b and 2 for more details. For the choice of cumulus parameterization in the control run, we have chosen KF for the control run since it's used by PAGASA in its NWP configuration; the default surface flux option (isftcflx = 0) and no spectral nudging so that we can easily assess the sensitivity to these physics parameterization

	and alternative model options. Other parameterizations were based on previous work on Typhoon Haiyan i.e. Li et al., 2018.
Minor suggestions	
(Line 113): Correct the year "2012" to "2013".	Thank you for spotting this. Revised in Page 5, Line 190 in the revised manuscript
(Line 125): Kindly reconsider "NWP" instead of "RCM".	The overall approach of the study is that we have used WRF configured as NWP to get the best configuration for hindcast TC cases simulations and eventually use that configuration to simulate the TC cases with future climate forcings. The results included in this paper is from the former i.e. as a sensitivity study using Typhoon Haiyan as the TC case. We have revised the manuscript to indicate that we used WRF as a LAM so as to avoid confusion.
There is no "Powers 2016" in the references.	Apologies for this. Powers 2016 should read Powers 2017. We have revised in Page 5, Line 204 in the revised manuscript and already indicated in the references.
(Line 132): Cruz et al., 2016 uses NHRCM and not WRF to make temperature and rainfall projections in the Philippines.	Apologies for this mistake. Cruz et al 2016 should read Cruz and Narisma 2016. We have revised this in Page 4, Line 210 and included in references of the revised manuscript.
(Line 155-170): Kindly provide a table for your control run's model setup as indicated in this section. Make sure to clarify if you performed one-way or two-way nesting, specify the input forcing, temporal and spatial resolutions (dt,dx,dy,dz), model physics, and so on.	We have used two-way nesting (between the outer domain D01 and inner domain D02) with horizontal resolution of 25km for D01 and 5km for D01; and 44 vertical levels with model top of 50hPa. We have explicitly indicated this in the manuscript and added a table for easier reference. Please refer to Table 3, Pages 9-10 of the revised manuscript.
(Line 180): "These cumulus schemes are used because PAGASA uses KF …". Does PAGASA also uses TK? Does the writer mean "The KF cumulus scheme was used because …"?	PAGASA uses KF, and TK is used for tropical ocean applications. We have indicated this in Page 7, Lines 266-273 in the revised manuscript.
(Line 185): There is no Sun et al., 2019 in the references.	Thank you for spotting this. Should read and have added Sun et al., 2015 in the text and references.
The discussion on TK is too short and vague. The author should also provide short discussion of the main output of the cited references. Same comment for Lines 194-195, 205.	We have revised and added in the discussion on cumulus parameterization particularly on Tiedtke scheme, and added a brief description on the outputs of the cited references. Kindly see Page 7, Lines 274-284.
(Line 206): Check repeating phrases in the sentence with "Charnock's (1995)".	Thank you for pointing this out. We have revised this in Page 8, Line 303.

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