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Comment on nhess-2021-400

Anonymous Referee #1

Referee comment on "Sensitivity of simulating Typhoon Haiyan (2013) using WRF: the role of cumulus convection, surface flux parameterizations, spectral nudging, and initial and boundary conditions" by Rafaela Jane Delfino et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-400-RC1, 2022

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 reanalyses.

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.

Lines 167-170: How do you explain your result that the simulation with the longer lead-time was the best?

Line 182: Was the cumulus convection scheme employed in both domains? Please state it clearly.

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.

Figure 3, x-axes: is it the simulation time or the 72-hour verification time (as it was stated in line 171)?

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).

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.

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).

Lines 351-352: in figure 6 the RMSE of TKsnONsf0 is about 15 m/s and its correlation is about 0.69.

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).

Line 464: Please justify your choice to present only the runs without nudging in figure 11.

Line 488: the steering flow bias has not been shown in figure 12.

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.

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?

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 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.

Technical corrections:

Line 152: "… and model physics (Isaksen et al., 2010)."

Line 158: "… different parameterization …"

Line 165: It is a 180-hour period (not 174-hour) from 00 UTC 4 November to 12 UTC 11 November.

Line 175: "… is bounded by 100-170 degrees East …"

Line 251: "… maximum 10m winds to evaluate …"

Line 268: "… relative vorticity maxima …"

Line 286: "… without nudging (snOFF) …"

Line 312: "… of the DPE (km) …"

Line 473: "… the KF scheme shows …"

Lines 496, 527, 543: KFsnOFFsd1 and TKsnOFFsd1 must be corrected to KFsnOFFsf1 and TKsnOFFsf1, respectively.