Thank you for your useful comments. Our responses to your comments and the comments of the other two reviewers will greatly improve the manuscript. Below are our responses (in blue) to each comment in turn.

While responding to comments from all reviewers we found a bug in our code in the way we treat asymmetry. As described in the original manuscript, we first remove an estimate of the asymmetry due to forward speed from the input best track V_{max}. The portion removed is a function of the TC translation speed, $V_a=1.173V_t^{0.63}$, following Chavas et al., (2017). We then add back an estimate of the asymmetry to the spatial 10m wind field diagnosed by KW01, again following Chavas et al., (2017). And in addition, we apply a factor that varies with radial distance from the storm center (the factor is equal to 1 at the radius of maximum winds and then decays with increasing radius) following Jakobsen and Madsen (2004). The bug was that the code missed adding back the V_t-dependent factor and only added back the radially-dependent factor. This caused us to add back the full value of V_t at R_{max} which caused too strong asymmetry, particularly for fast moving storms over Japan for example. In response, original figures 2, 3 and 4 will be corrected. The analysis in original figure 7 is for along-track winds which will not be affected by this bug fix.

Chavas, D. R., Reed, K. A. and Knaff, J. A.: Physical understanding of the tropical cyclone wind-pressure relationship. Nature communications, 8(1), p.1360, https://doi.org/10.1038/s41467-017-01546-9, 2017.

Jakobsen, F. and H. Madsen, H.: Comparison and further development of parametric tropical cyclone models for storm surge modeling. Journal of Wind Engineering, 92, 375-391, https://doi.org/10.1016/j.jweia.2004.01.003, 2004.

The manuscript proposed a new modeling system for generating tropical cyclone (TC) wind, which consists of a parametric radial profile model, the non-linear boundary layer model (KW01), and the terrain effects. The authors presented a case of hurricane Maria and Wilma and verified the model using landfalling storms. Then, the authors discussed the impact of terrain on the changes of TC winds over the South East US, Taiwan and Eastern China, and Eastern Australia. Overall, I like this approach and can think of many applications of this model. One criticism I have is that the advantage of using KW01 was not shown or discussed with evidence. In Section 2.3, The authors described KW01 as the key advancing component of the modeling system. At many other places, the author says that their system contains more dynamical processes than other existing tools. However, none of the results shown here can isolate the positive impact of using KW01. I suggest conducting additional simulations using Willoughby's wind with an empirical factor applied to get winds at10m height, plus the terrain effect. By comparing these new simulations with the Willoughby+KW01+terrain, we can see the advantage of (or differences caused by) KW01.

The suggestion to better demonstrate the advantage of using KW01 is a good one. We agree that this is needed, given that we state that the use of KW01 is the main advance of our modeling approach. In the revised manuscript we will extend the analysis of Hurricane Maria over Puerto Rico to better demonstrate the advantage of KW01, and its interactions with surface roughness and topography.

The original analysis (shown in the original Fig. 2) compared Willoughby, Willoughby+KW01, and Willoughby+KW01+terrain. The new Fig. 2 will include two new simulations:

1) A simulation using Willoughby+KW01+surface roughness without topography. This simulation will isolate the effect of surface roughness from the effect of topography.
2) A simulation using Willoughby+KW01+ topography but with surface roughness equivalent to that of the ocean over the entire domain. This simulation will isolate the effect of topography from the effect of surface roughness. We will compare our topographic effect with topographic multiplication factors used in Yang et al. (2014).

We will include difference plots to show the impact of each modeling component in turn.

In addition, we will add a comparison of each simulation to the available surface station observations to get a sense of the added value of each modeling component. We will also include a figure showing a snapshot of the model output wind field as Maria makes landfall, to show any evidence for strong deviation of winds around local hills.

Regarding adding a simulation that applies an empirical factor to Willoughby, we explored the possibility of calculating wind multiplication factors that account for terrain and topographic effects. A recent summary of wind multiplication factors used by international and national wind engineering codes found large differences in the level of complexity in the approaches and assumptions (Yang et al. 2014). As described in Tan and Fang (2018), calculating the topographic multiplication factor is non-trivial and requires a number of assumptions about the overlapping influence of nearby peaks within complex terrain and how this varies with wind direction. The resulting wind field would then be very sensitive to our assumptions. Developing expertise in constructing topographic multiplication factors is beyond the scope of this study, and so we choose to respond to this point by adding a description of the differences in the wind multiplication factor approach and our approach.

In the absence of inland observations an alternative approach to model evaluation is to compare with a simulation using a full atmosphere numerical weather prediction model. We were able to acquire data from a Weather Research Forecasting model simulation of Maria. Our intention was to use this simulation to compare the terrain response of KW01 with the NWP model simulation. However, we found that WRF has a problem with dropping the 10-m winds too much over land and immediately at the coast. Fixing this problem was beyond the scope of this paper.

Tan, C. and Fang, W.: Mapping the wind hazard of global tropical cyclones with parametric wind field models by considering the effects of local factors. International Journal of Disaster Risk Science, 9(1), 86-99, https://doi.org/10.1007/s13753-018-0161-1, 2018.

Yang, T., Cechet, R.P. and Nadimpalli, K., 2014. *Local wind assessment in Australia: Computation methodology for wind multipliers*. Geoscience Australia. 2014/33.

Below are a few minor comments and questions

1. Page 4, line 2. While I understand the advantage of using KW01 instead of a simple empirical model, this sentence sounds vague. Please elaborate more on what the additional dynamical effects are.

We will include a brief discussion of the additional dynamical effects.

2. The first paragraph of section 5 (page 11) should belong to Section 2.1.

This paragraph will be moved as suggested.

3. Page 5, Line 15: Please mention the TC boundary layer height used in this study as well. Is the model performance sensitive to the TCBL height?

Page 7, Line 8 states that the model top is held fixed for all simulations at 2km. This was chosen to be above the typical height of super-gradient jets. We also state that '*While the boundary layer height likely varies substantially across global TCs, we choose to keep this fixed in the absence of readily available data*'. We also note that the TCBL height varies strongly with radial distance from the center of a given storm (Kepert et al. 2012). The height also depends on the specific definition of TCBL top. We have not explored sensitivity to our model top, but agree that it is a key parameter that will affect other aspects of model setup such as the factor used to inflate the input best track Vmax from the surface to model top. This unexplored model sensitivity will be acknowledged and discussed in the conclusions.

Kepert, J. D.: Choosing a boundary layer parameterization for tropical cyclone modelling. Monthly Weather Review 140, 1427–1445, https://doi.org/10.1175/MWR-D-11-00217.1, 2012.

4. Page 7, L11 'running for 24 hours for each forcing update is computationally impractical.' I am surprised to see that running 24 hours of KW01 is computationally impractical. What is the computational cost of KW01, and how is it compare to the computational cost of 2-km WRF.
I am asking this is because, for assessing wind risk, the most significant advantage of a simplified wind generator v.s. a full-physical model is its low computational cost. If running KW01 is computationally expensive, this system will not be able to use for real risk assessment, which (I thought) is one (and probably the most important one) of motivations of this work. (The other motivation is to understand wind risk over complex terrain using historical cases. For this purpose, we can always run WRF or other mesoscale models which may generate more realistic winds than KW01)

Thank you for raising this important point. The simulation wall-clock time strongly depends on the number of grid points. For the large domains needed to capture the long tracks of fast-moving storms (over Japan or the Northeast U.S., for example) the wall-clock time is substantially longer than using wind profile models alone. The most expensive domain, over Japan, is 900 X 1100 X 18 grid points using 2-km grid spacing and a 2 second timestep. A 24-hour simulation took 6 hours wall-clock time on 36 cores. Smaller domains run at 4km run much quicker.

Running a 24-hour period for a single event is therefore computationally quite practical. But running for 24 hours for each 10-minute forcing update to ensure full equilibrium is reached for each forcing update would rapidly increase computational demands to impractical levels.

Running the WRF model over the same domain would cost more due to the higher number of vertical levels, and a greater number of physical processes. We have not run WRF over these specific domains so we are not able to provide a computational cost. Even if it were feasible to run WRF for all 714 historical cases simulated here, future applications of our modeling approach to large numbers of synthetic TC tracks would presumably become impractical for WRF.

We will include a short discussion of the computational cost of our modeling approach at the end of the conclusions section.

5. Page 9, L24. Do you mean the maximum wind speed recorded at the station during the lifetime of the storm, which is different from the storm lifetime maximum wind speed (which is usually one value per storm)?

We don't see this specifically mentioned on page 9, L24. But your point is correct. We do indeed mean the max wind speed recorded at the station during the lifetime of the storm. This point will be corrected throughout the manuscript.

6. Page 11, L1: Where is this 20% bias correction factor comping from? Is it universally applied to all simulations?

On further consideration, and in response to similar concerns from other reviewers, we decided to remove this bias correction factor from this study. The original factor of 20% was determined by comparing our simulations with surface station data in urban areas for a subset of 8 landfalling U.S. hurricanes. This bias correction step was added to aid application of the dataset but clearly patches over an underlying problem.

Holmes (2007) found that the roughness length for urban areas can vary between 0.1 and 0.5m for suburban regions and rise to between 1 and 5m for densely packed high-rises in urban centers. Our model uses a single roughness length for all urban areas (suburban and city centers) of 0.8m and this was taken from the MODIS land use dataset – the same as used in the Weather Research and Forecasting (WRF) model. This value is too high for suburban areas, where a value closer to 0.2 is typical (Yang et al. 2014). Depending on the specific siting of the wind observing stations, it's probable that the introduction of multiple urban categories with different roughness lengths would improve our low wind speed bias. This detailed investigation is beyond the scope of this paper and we choose to leave this for future work.

Holmes, J. D., 2007. Wind loading of structures. 2nd ed. London and New York, Taylor & Francis

Yang, T., Cechet, R.P. and Nadimpalli, K., 2014. *Local wind assessment in Australia: Computation methodology for wind multipliers.* Geoscience Australia. 2014/33.

7. Page 11. L12-14 belongs to the figure caption of Fig. 6, not in the main text.

This text will be moved to the figure caption of Fig. 5 (the figure that shows all domains).

8. Figure 7 and the related discussion. Did you check the enhanced vertical diffusion and vertical advection in KW01? Can you show some analysis of these enhanced features? There is a lag between the terrain and the wind gradient. Why?

Figure 7 shows how the inland gradient of wind speed varies with distance inland from the coast, together with the terrain height. The gradient of wind speed and the terrain height is the along-track average over all simulated storms by region. The gradient of the wind speed is also the net effect of not just topography but also variations in surface roughness and the overall inland decay according to the input best track Vmax. This makes it challenging to isolate the processes driving the inland wind speed gradient. We will highlight this complexity in the revised manuscript, and tone down our asserted mechanism to a suggested mechanism.

Carefully constructed idealized experiments would be needed to isolate the processes (enhanced vertical diffusion and vertical advection in KW01) driving wind acceleration on the upwind slopes and crests of terrain features. We choose to leave this investigation for another study that would focus more on the process-level understanding rather than a global assessment as presented here. This point will be discussed in the conclusions.

The distance-rate-of-change in wind speed shows increases (or for some regions, a lessening of the inland decay) along the upwind slopes up to the crest (where, for example in Fig. 7b the wind gradient switches from positive to negative). The lee sides show some evidence of accelerated inland wind decay. This is similar to the topographic effect on the winds shown for the single storm Maria in Fig. 2. We don’t see a strong lead-lag effect.

9. Willoughby et al. 2006 is missing in the references.

Thank you for spotting this oversight. It will be added.