Thank you for these useful comments and list of references. 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.

Review of "Modelling Global Tropical Cyclone Wind Footprints" by James M. Done et al.

Summary

The MS describes a method for automated modelling of tropical cyclone winds, both instantaneous fields and the maximum wind swath over the life of the storm. An axisymmetric representation of the gradient-level wind is derived using inputs from, for example, a best track database. Then a nonlinear boundary-layer model is used to calculate the winds throughout the boundary layer, including at the surface (10 m), from this gradient-level wind, accounting for storm motion, heterogeneous surface roughness and topography.

There are three potentially serious flaws with this approach, relating (i) to the way the parametric profile of Willoughby et al (2006) has been used, (ii) to the likely inability of the nonlinear tropical cyclone boundary layer model to correctly model mountain waves, and (iii) to the authors' misapplication of the work of Harper et al (2010) in adjusting observed winds for different averaging periods. These are expanded upon below. In addition, some minor points where clarification is needed are noted.

Use of the Willoughby et al. (2006) parametric profile

The criticism here is not that the authors have chosen this profile – indeed, I consider it to be the most suitable tropical cyclone parametric profile presently available, because of its superior ability to fit observations. Rather, it is criticism of the way they have used it. The authors note that in section 2.2 that the "Holland et al. (2010) profile has the advantage of tying down the radial decay profile using an observation of an outer wind, say the radius of 34 knot winds" and go on to note that such observations are not always available. They then note that the Willoughby et al. (2006) profile has two exponential decay scales for the outer part of the vortex. In addition, the user must assign the relative weight of these two profiles, so there are three free parameters that determine the shape of the vortex outside of the radius of maximum winds (RMW), although in practice Willoughby et al. (2006) recommends that one length scale be held fixed at 25 km. Choosing values for the remaining two free parameters requires additional data; Kepert (2006a,b) and Schwendike et al. (2008)

describe the use of aircraft reconnaissance data for this purpose and discuss the associated difficulties. This choice can lead to substantial differences in the shape of the wind profile and hence the radius of gales, and Willoughby et al. (2006) show that a wide range of the second length scale occurs in nature, with their Fig 11 showing it can range from about 100 km to over 450 km. The authors of the MS under review not only omit to describe how they have chosen these crucial parameters; they also incorrectly assert that the Willoughby et al (2006) profile has "fewer required data inputs".

Thank you for this informed comment on our use of the Willoughby et al. (2006) parametric profile. We will remove the assertion that the Willoughby et al. (2006) profile requires fewer data inputs than the Holland et al. (2010) profile.

We agree that the outer radius of damaging winds can be highly sensitive to the choice of the free parameters. We did not provide sufficient detail on how the three free parameters are chosen. The revised manuscript will explain our choices.

Firstly, we will state that the length scale for the transition region across the eyewall is set to 25km when Rmax is greater than 20km and is set to 15km otherwise. For the shape of the vortex outside Rmax, we hold the faster decay length scale fixed at 25km, following the recommendation of Willoughby et al. (2006), and allow the second length scale (*X1*) and the contribution of the fast decay rate (*A*) to vary the shape of the profile. For the slower decay length scale, we will note that Willoughby et al. (2006) showed that a wide range of the second length scale occurs in nature, with their Fig. 11 showing it can range from about 100 km to over 450 km. We will also note the difficulties in using aircraft reconnaissance data for this purpose (as in Kepert 2006a,b and Schwendike et al. 2008). Given that aircraft data are not uniformly available globally, and our intention to only use readily available data, we decide not to include these additional data sources for subsets of global historical events. Willoughby et al. (2006) demonstrated dependence on Vmax, and latitude, with the more intense low latitude TCs being more sharply peaked. We also have Rmax readily available. We therefore choose to allow the remaining two free parameters (*A* and *X1*) to vary with readily available parameters Rmax, Vmax and latitude, following equation 11 in Willoughby et al. (2006). This globally consistent approach is needed to allow relative risk assessments across regions.

Use of the Kepert and Wang (2001) nonlinear tropical cyclone boundary layer model

This model, and others like it, are the most sophisticated diagnostic models of the tropical cyclone boundary-layer presently available. However, there are two important areas in which the authors have failed to establish that their use of the model is appropriate.

Firstly, the model as originally formulated was written in storm-following coordinates. This enabled the efficient simulation of moving storms, since a smaller domain could be used, and indeed Kepert and Wang (2001) remains one of the few theoretical papers on the tropical cyclone boundary layer to consider the effects of storm motion. The authors describe some modifications to the model, which they state are to allow for a time-varying gradient wind field, and for landfall. While their description is unclear, it appears that they may have changed to earth-relative coordinates, for they state that “a translation vector is added to the horizontal advection terms in KW01”. Unfortunately, the governing equations are omitted, so it is impossible to be sure. However, they do note that “the proportion of the translation vector added reduces close to the surface due to surface friction”. This is certainly incorrect; the whole of the coordinate system must move with the same velocity! Perhaps it is an attempt to allow for friction in the environmental flow, which the model (in its original form) assumes is equal to the translation vector. However, whether in earth-relative or storm-relative coordinates the model should be able to spin up the boundary layer of any environmental flow, and if in storm-relative coordinates does not require the addition of a translation vector. Perhaps the authors’ modification is correct, but until they give equations this cannot be established, and as outlined above, their description doesn’t sound correct.

Thank you for this comment. We agree that our original description was unclear. We also apologize for including some old and incorrect description of the model setup. The few lines of incorrect description was a legacy of some earlier model development and testing. The revised manuscript will clearly and accurately describe the final modeling approach that we settled on.

The original model coordinates of KW01 were storm-relative. The storm in this original coordinate system therefore did not move by definition. To account for the effect of storm motion, the original model includes the pressure gradient of the environmental flow in the forcing pressure profile at the model top.

We changed the model to run in Earth-Relative coordinates. Each simulation is conducted on one of the 17 geographically-fixed regional domains shown in original Fig. 5. Our early model testing retained the effect of forward speed in the forcing of KW01 but we found KW01 strongly damped the asymmetry and we did not have the resources to find the cause. We therefore chose to take out the effect of forward speed from the forcing for KW01 and treat it in pre- and post-processing steps as depicted in the workflow shown in original Fig. 1. We also do not add the translation vector to the horizontal advection terms. That was a mistake in our description so thank you for pointing that out.

Our approach therefore misses any interaction effects between terrain and the asymmetrical component of the storm wind field. The importance of these effects are unknown and we leave their inclusion for a future iteration of our model.

I note also that, since the Kepert and Wang (2001) model incorporates the effects of translation, the postprocessing step of adding on a motion asymmetry shown in Figure 1 is at best unnecessary, but most likely also incorrect.

As noted in our response above, we removed the effects of translation from KW01. Adding on a motion asymmetry was therefore necessary.

The second issue concerns the possibility of mountain wave activity. The model does allow for surface topography, although this facility has not before been the subject of published papers to my knowledge. However, it is unlikely it would accurately represent mountain wave activity, because of the shallow depth of the domain – modelling studies of mountain waves typically consider at least the full depth of the troposphere. Although some of the discussion in the introduction could be interpreted as evidence against mountain waves, it is far from rigorous – for instance, the authors note that the Froude number will be high without considering that this will also depend on the flow geometry. In addition, they note the “quasi-neutral stability”; while this is plausible near the eyewall provided one considers moist stability, it is incorrect at larger radius as shown by the observational composites of Zhang et al. (2011).

Thank you for this useful comment. We can see how our discussion of full NWP model simulations by Ramsay and Leslie (2008) may have been interpreted as an indication that mountain wave activity was represented in our modeling approach. Our model top at 2-km cuts off the free troposphere needed for large-scale mountain waves that can bring free-atmosphere winds down to the surface. It’s also unlikely that our model has the resolution to capture flow-separation turbulence downwind of crests and escarpments.

The revised manuscript will more clearly articulate the physical terrain effects permitted by the model. These include convergence, vertical diffusion and vertical advection on windward slopes and crests resulting in locally strong low-level shear and TKE production. In addition, the vertical boundary layer structure allows the potential for super-gradient jets (Franklin et al. 2003; Kepert and Wang 2001) to influence winds in high terrain. Finally, the time dimension allows for upwind effects due to upwind terrain variations and terrain to be incorporated.

In the revised manuscript, our discussion of the Froude number will acknowledge the contribution of mountain geometry. For high mountains, the wind has a greater potential to become blocked. On closer inspection we didn’t see any evidence of substantial blocking (upstream deceleration) in any of our runs. This will be discussed further for the case of Maria over Puerto Rico in an expanded Fig. 2. We will also acknowledge that our modeling approach doesn’t directly allow any blocking to affect the TC track, although the observed tracks that are used do include such blocking effects.

We agree that a neutral boundary layer is not guaranteed at large radii. Kepert (2012) indicates increasing static stability as subsidence increases at these larger radii. Even so, measurements of turbulent fluxes in high-wind environments between outer rainbands by Zhang et al. (2009) find shear production and dissipation to be the dominant source and sink terms of TKE.

Zhang, J. A., W. M. Drennan, P. G. Black, and J. R. French, 2009: Turbulence structure of the hurricane boundary layer between the outer rainbands. J. Atmos. Sci., 66, 2455–2467

Conversion of wind speed averaging periods

The authors have adjusted surface wind observations for averaging period, claiming as justification the work of Harper et al. (2010). This is incorrect. Harper et al. (2010) emphasise that their conversion factors are to be used for tropical cyclone intensity, and that they should **not** be used for wind observations. Please refer to the third paragraph of the executive summary, section 1.3 and appendix E of that report.

We understand that the shorter averaging period can only be considered a gust in the context of a longer averaging period over which the wind is considered steady (i.e., constant variance/turbulence). Estimating the 1-minute ‘gust’ from buoy-observed 10-minute mean wind should therefore be appropriate. But estimating the 1-minute ‘gust’ from the surface station-observed 2-minute wind is questionable since the 2-minute wind is not necessarily a measure of the mean wind. This 2-minute wind could be higher or lower than the true mean wind.

However, we still desire to use these 2-minute observations for model evaluation. We therefore choose to use the factor to estimate the 1-minute wind from the 2-minute wind, despite the inconsistency with statistical theory. In this, we assume that errors arising from this factor application are i) similar in magnitude to the uncertainty in the definition of the 1-minute wind from numerical model data, and ii) similar to errors due to gusts deviating from statistical theory due to winds not in equilibrium with the variable surface conditions.

Further comments

Page 3 line 2, the Willoughby et al (2006) profile is not intended for surface winds.

In the revised manuscript we will more accurately state that the Holland et al., (2010) profile, for example, models the surface winds directly whereas the Willoughby et al., (2006) profile models the gradient-level winds and an extra step is needed to determine the surface winds.

Page 5 line 8, and elsewhere, the authors claim to use an “average value of Rmax around the storm”. This seems strange, Rmax is usually not regarded as having significant asymmetries, unlike say R34.

This comment about taking an average value of Rmax around the storm will be removed.

Page 5 line 16, 500 m is too low here, as it is either at or below level of the supergradient jet.

We will change this to be consistent with the definition of boundary layer height based on the depth of the inflow. We now explain that ‘One definition for the boundary layer height is the depth of the inflow, defined as the height where the radial inflow falls to 10% of the peak inflow. Using radiosonde ascents in 13 hurricanes Zhang et al., (2011) find this height to be approximately 850m at the radius of maximum wind rising to approximately 1300m at larger radii.’

Page 6 line 8, the value of the eyewall surface wind factor of 1/1.32 = 0.75 is high compared to observations (Franklin et al. 2003, Powell et al. 2009) and theory (Kepert and Wang 2001).

Franklin et al. (2003) observed an inner core wind maximum at about 500m that increases to about 1km for the outer winds. They found a logarithmic profile below the wind maximum and a reduction of winds above due to the warm core. The result is a 700-hPa-to-surface wind factor of about 0.9 in the inner core. Kepert and Wang (2001) theory also has a factor of 0.9 in the inner core, with the factor decreasing to 0.75 in the outer winds. Knaff et al. (2011) took the Franklin et al. (2003) factor of 0.9 in the inner core and additionally reduced it by a factor of 0.8 to go from marine-exposure winds to terrestrial-exposure winds, giving a net factor of 0.9*0.8=0.72.

Initial testing using a variable factor for offshore and onshore track caused enhanced winds just offshore in response to the higher factor for the first inland track point. This is because the outer winds were still responding to the low surface roughness while being driven by stronger gradient winds. We therefore choose to hold the factor fixed and appropriate for inland winds.
This will be discussed in the revised manuscript.

Knaff, John A, Mark DeMaria, Debra A Molenar, Charles R Sampson, and Matthew G Seybold. 2011. “An Automated, Objective, Multiple-Satellite-Platform Tropical Cyclone Surface Wind Analysis.” Journal of Applied Meteorology and Climatology 50 (10): 2149–66.

Page 6 line 31, the Kepert and Wang (2001) model cannot resolve turbulence, since it uses a fixed pressure field. In this, it is unlike recent high-resolution simulations of tropical cyclones by Nolan et al. (2014) and Stern and Bryan (2018).

We agree that Kepert and Wang (2001) cannot resolve turbulence, but it does parameterize the effects of turbulence through its turbulence scheme. This will be more clearly stated.

Page 7 line 2, the model does represent buoyancy, although probably not particularly well since it ignores moist processes.

In the revised manuscript we will more correctly state that the model does not represent buoyancy *well*. But for most TCs, and for the inner core region of the strongest winds, we expect buoyancy effects to be small

Page 9 line 12, it is unclear how figure 3a shows this correction.

In response to the concerns of the other two reviewers we decided to remove this 20% 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.

Page 11 line 1 and in the conclusions, the claim that the model shows "no large bias" in urban areas seems optimistic. At a radius of 300 km, the bias is about -10 m/s. At this radius, this is likely well over half the observed wind speed, hardly negligible!

We agree that stating "no large bias" is not accurate. In response (and in response to another reviewer's comment) we will change this to state that the model compares well with a recently published global wind swath modeling approach (Tan and Fang, 2018). Their approach using similar quantities of input data and local wind multiplication factors to account for terrain features also showed typical errors of 8 to 10m/s. See their Fig. 6 that shows comparison with observations for 36 TCs during 1970–2014 for 25 stations in Hainan Island, China.

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.

The term "storm lifetime maximum wind" is generally used to refer to the wind swaths (e.g. page 13 line 8) but is ambiguous since it could also refer to the storm's peak intensity. In this part of the MS, these maximum winds will generally not occur at the storm centre, but they are analysed in terms of along-track distance. How is this calculated?

Thank you for raising this important distinction, that another reviewer also noted. We agree that although we used common definitions, they are rather imprecise. Our use of 'storm lifetime maximum wind' refers to the maximum wind speed recorded at a specific location (grid point or observing station location) throughout the lifetime of the storm. The revised manuscript will be specific whenever this term is used.

For Fig. 7 specifically, the wind data are extracted from the wind swath at the location of the TC track. This data is therefore the storm lifetime maximum wind speed at the specific locations along the TC track. Then we composited all TC tracks for a given region about their track points of landfall, and took an average over all tracks. This gives the region-average along-track wind swath vs. distance inland. This calculation will be made clearer in the revised manuscript.

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Nolan, D. S., J. A. Zhang and E. W. Uhlhorn, 2014: On the limits of estimating the maximum wind speeds in hurricanes. Mon. Wea. Rev., 142, 2814 – 2837. doi:10.1175/MWR-D-13-00337.1

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Zhang, J. A., R. F. Rogers, D. S. Nolan, J. Marks, and D. Frank, 2011: On the characteristic height scales of the hurricane boundary layer. Mon. Wea. Rev., 139, 2523–2535, doi:10.1175/MWR-D-10-05017.1.