

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”.

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.

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.

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).

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.

Further comments

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

Page 5 line 8, and elsewhere, the authors claim to use an “average value of R_{max} around the storm”. This seems strange, R_{max} is usually not regarded as having significant asymmetries, unlike say R_{34} .

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

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).

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).

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

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

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!

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?

References

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