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Discussions

Interactive comment on "Modelling Global Tropical Cyclone Wind Footprints" *by* James M. Done et al.

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Summary

The authors seek to "to advance our understanding of overland wind risk in regions of complex terrain and support wind risk assessments in regions of sparse historical data" through the application of a combination of analytical, numerical and empirical techniques. The authors make reference to many very successful more simplified approaches that have been developed over the past three decades that emanate from wind engineering, atmospheric science and insurance loss initiatives. They put the case that previous approaches, in their assessment, lack the essential capacity to incorporate complex terrain and "the essential dynamics and physics" of tropical cyclone (TC) behaviour where "the accuracy of wind speeds over urban (sic) is of critical importance". The use of a diagnostic 3D numerical boundary layer model is central to their

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Discussion paper

thesis.

While the desirability of such an approach can be supported, where practical, it is ironic that the demonstrated model skill is so poor at reproducing recent historical TC winds in areas where there is a significant amount of quality data available and in mostly flat landscapes. In no way does the model "compare favourably" with the displayed data. An 8 to 10 m/s error band in any hindcast event intended to assist, for example, offshore engineering design or sensitive onshore high-rise design, would be regarded as completely unacceptable. For insurance losses where damage is noted by the authors to be additionally highly nonlinear with wind speed, it would be massively unreliable. The stated need to apply an empirical "20% adjustment for urban areas" is not only completely inconsistent with the theoretical high ground being argued but is symptomatic of a modelling system that has some significant problems.

The Challenge of a Global Approach and the Expected Benefits

The problem with developing tools for global application unfortunately means that accuracy is inevitably impacted by the need to adopt spatially and/or temporally compromised globally available datasets. This situation limits, and actively dissuades, examination of the myriad of fine scale site-specific influences on the TC surface wind during a specific event that cannot be ignored. These have traditionally been transparently handled by reference to standard exposure and application of statistically based boundary layer turbulence approaches. The authors' more complex and computationally demanding approach needs to demonstrate at least a comparable utility.

In any case, it is not clear what practical application there is in producing such a global (deterministic) event set, given that the essential need is for risk management that implicitly requires a probabilistic approach. Cherry-picking of historical events does not yield firm statistical guidance and any such results will most likely be less reliable than any regional wind speed risk assessment that is based on (even sparse) long term data sets, given that aggregation of sites is often justified. In spite of the practical challenges in this space, the various engineering design standards around the world have over-time assembled realistic and likely suitably conservative wind risk frameworks, all of which embody the need to allow for height, terrain and topography effects.

The model's development is touted as valuable for insurance-related purposes. However, the principal cause of increasing world-wide losses for insurers is, together with uninformed risk-based planning, the failure to implement known good design, construction and inspection practices for residential development. The importance of globally modelling large scale terrain influences is also overstated given that, compared with typically nearly-flat or undulating conurbations, there is negligible insurance exposure to wind hazard in areas of very high or steep terrain.

Comments on Method

(2.) The step that "removes an estimate of the asymmetry due to storm motion" from the surface wind relies on the assumption that historical Vmax do reliably include such an influence. While Dvorak, for example, implies that is the case there is no specific allowance in the methodology. Hence the adopted empirical adjustment likely has little merit in terms of overall accuracy.

In quoting Harper, Kepert and Ginger (2010) (aka the WMO wind averaging guidelines - hereafter HKG), the assumption that numerically modelled winds calculated at a small timestep are representative of so-called 1-min sustained winds is incorrect. Section 1.6 of HKG specifically advises on that topic noting that numerical models without explicit eddy representation only estimate mean wind speeds, not gusts such as the so-called 1-min sustained wind. However, correcting for that (e.g. per HKG Table 1.2) will likely have no specific effect on the model performance.

(2.1) The authors state that the model is "agnostic to the source of the track data" as though that is some advantage, whereas the vast majority of historical datasets consist only of (lat, lon, Vmax) estimates with acknowledged high variability between agencies and also over-time. Rmax is also noted to be an essential parameter but is only recently available in some regions and not transparently derived. While these drawbacks are unavoidable, it further emphasises the challenge of using any (global) historical track data without critical assessment and expecting a high level of accuracy in the estimation of terrain-sensitive surface winds.

(2.2) While the Willoughby profile may be superior to some others for a hands-off global application it still requires an outer scale assumption and to note that TC scale, and its temporal evolution, is a critically important parameter in accurately modelling surface winds. The adopted land use surface roughness, with a scale of about 1 km, seems reasonable enough but should also be verified by example for the set of modelled storms. To note also that the references cited for drag coefficients are very dated and the authors could adopt more recent evidence to better suit their argued approach.

Comments on Results and Evaluation

(3.) The Puerto Rico example illustrates the intention and ability of the model to introduce terrain and topographic variability into its results but, without any verification, is otherwise meaningless and simply an "artist's impression".

(4.) To note that the use of 3-h sampled wind data is typically inappropriate for TC passages within, say, 100 km and may be a principal source of the poor comparisons. Application of HKG Table 1.1 is also dubious given the 3-h sampling but more so because of its limited and nominal exposure classes, which appear inconsistent with the aim of deriving fine scale surface winds. HKG Section 1.2 says "The aim has been to provide a broad-brush guidance that will be most useful to the forecast environment rather than a detailed analytical methodology" and "In particular, post analysis of TC events should seek to use the highest possible site-specific analytical accuracy for estimating local wind speeds. This would include consideration of local surface roughness, exposure and topographic effects when undertaking quantitative assessments of storm impacts." This implies an approach like Powell et al. (1996) is needed in such cases. Again, these oversights in applying HKG will likely have little effect on model performance, but do point to a lack of rigour in matters of wind magnitude adjustment.

As noted previously, the demonstrated performance of the model is very poor compared with the numerous less-complex examples that are cited. In spite of the authors' tendency to downgrade the utility of H*WIND I would instead encourage pursuing such comparisons in order to locate the model deficiencies and improve its performance for the demonstration storms.

Comments on Global Landfalling TC Footprints

(5) The detailed commentary and interpretation of aspects of modelled landfall and inland wind decay characteristics in various localities seems to overlook the fact that the model is only reacting to the imposed "best track" intensity variation and therefore can have no better skill than offered by a simpler parametric approach. Surely fully dynamic modelling (e.g. HWRF or similar) is needed to reliably explore such impacts.

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