

Interactive comment on “Estimates of tropical cyclone geometry parameters based on best track data” by Kees Nederhoff et al.

Anonymous Referee #1

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The authors use Best Track data to improve existing formulas for tropical cyclone characteristics, treating the variables as stochastic ones. The paper is well written and without doubt presents a useful step forward in the field. The approach followed is clear and the authors also share with the community the new parameterizations they produced. Overall I have no objections on the publication of the paper, just few comments/suggestions mostly for consideration.

- Given the background of the authors, the paper is currently oriented to ocean modelers, however the work could be useful also to anyone dealing with TC hazards. To that direction I would recommend expanding a bit the introduction also to wind hazards, adding more references (e.g. Peduzzi 2012 Nature Climate Change). On the ocean modeling side I would recommend citing some recent papers simulating tropi-

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cal cyclones (e.g. Bloemendaal 2019, Climate Dynamics; Vousdoukas 2018, Nature Communications)

- One major weakness of the study is that BTD are not accurate and as also shown in the validation. I think this is made clear in the discussion by the authors, but I was wondering whether the performance would be further improved if one could consider the BTD error in the fitting of the empirical equations (thinking of the possibility of introducing the error maybe by doing the least-square fitting in a Monte Carlo framework). I leave it to the authors if they would like to discuss about it in the paper or in the present open discussion. In any case, I would like to see the authors thoughts on how the parameterizations could be further improved; as it is clear that despite the obvious progress the data to provide a satisfactory solution to the problem are still not there.
- The correlation implied by figs 3 and 5 is ...daunting! I would suggest the authors to provide some additional information: for example RMSE and bias expressed as %, r² coefficients, but also some q-q plots (or scatter plots with colorscale expressing point density) which could show that despite the scatter the two variables are somehow related. For the time being especially fig 5 seems like noise.
- Some figure captions could benefit from better explanations of the contents of the figure (especially 7-8)

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