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Discussions

Interactive comment on "Maximization of the precipitation from tropical cyclones over a target area through physically based storm transposition" *by* Mathieu Mure-Ravaud et al.

Anonymous Referee #1

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This paper attempts to define a fully physically based method to estimate a maximum precipitation that would result from tropical cyclones over a given target area, which is more or less close to the effective landing area. This method is applied to four cyclone cases. As such, the subject of this paper is obviously of great interest for HESS.

The proposed "transposition method" relies on a series of steps that are rather precisely defined: (i) define the centre and radius of the cyclonic vertex, (ii) define the meteorological background field as being the field outside of the cyclonic vertex, as well as its linear interpolation inside of the vertex, (iii) define the perturbation field by subtracting the background field to the actual field, (iv) translate/shift the perturbation

field (v) linearly recombine it with the (fixed) background field, (v) run a regional atmospheric model (RAM) with the obtained initial conditions, (vi) estimate the resulting, accumulated precipitation over the target area and a given period of time (72 hours in the present study).

At first, references to the concept of Probable Maximum Precipitation (PMP) seem somewhat misleading: although the authors have been inspired by some techniques developed in PMP approaches, their goal is more precise as explicitly stated in the introduction and somewhat in the title of their paper. Furthermore, as discussed below, it seems that their study puts into question PMP rather than supports it.

Secondly, the claim that the present method is fully physically based is not obvious for at least two reasons: - whereas, the RAM can be considered as physically based on the subrange of the explicit scales, this is not the case for the parametrisation of the smallest scales that are essential for precipitation; - most other procedure steps are not physically based.

Furthermore, the linear nature of several steps (ii - iv, respectively subtracting, interpolating and adding the background field) are rather at odd with the nonlinearity of the system. It is also questionable to define the cyclonic vertex as a circle (step i), whereas the material contours of various fields are rather convoluted. A priori, these linear simplifications, as well as the parametrisation, may introduce non negligible model/method errors that should be acknowledged, despite they generate frustrations with respect to the applicative goal of the paper, namely the accuracy of the heavy precipitation estimates.

On the contrary, I believe that the authors should emphasise and promote a result of their study that is a consequence from the preserved nonlinearities of the systems. Indeed, they are right to observe and argue that these nonlinearities yield a complex sensitivity of the vertex track with respect to the initial translation of the vertex, in particular a small translation can be well sufficient to substantially modify the vertex track so that it will go over the target area. Similar observations are done and could be further developed on uncertainties resulting from the choice of the simulation starting date, therefore of its initial conditions. In particular there is a sensitivity to the relative intensity of the perturbation field, which might interfere with the aforementioned method errors (i.e., highest intensities will presumably amplify these errors). The authors are right to mention a similar sensitivity to the choice of the RAM parametrisation settings. By the way, according to the rightest hand side equation of Eq.1, it seems the authors selected the hydrostatic option of WRF, whereas it is basically a non-hydrostatic RAM.

A priori, the above results and considerations have important implications on the accuracy of the estimates of the heavy precipitations over the target area, i.e., they presumably displays a much higher variability than expected. Does it require an ensemble approach and a statistical analysis of the extremes? Does the latter put into question PMP approaches? I believe that these questions should addressed, at least tentatively, by the authors. Overall, I believe that the paper should devote more room to the methodological questions and display a terser presentation of the study cases.

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