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Hydrology and Earth System Sciences
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 #2

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General Comments:

The present study proposes a method to estimate the probable maximum precipitation over a specific river basin with the use of a regional meteorological model. The proposed method is physically based and transposes a TC location by separating the circulation associated with a TC and its background state. The way how the proposed method works is demonstrated for four hurricanes cases. In general, this type of approaches that relocate the initial position of a specific TC is useful for assessing the impacts of the TC hazards at basin scales, because, as the authors recognize, the precipitation pattern by a specific TC is critically dependent on the track of a TC as

well as the intensity of a TC. Thus, the present study potentially deals with a scientific important issue.

However, the scientific originality of the present study is doubtful. There are some studies that proposed a TC relocation approach by using a TC bogusing scheme. For example, Ishikawa et al. (2013) and Oku et al. (2014) proposed a TC bogusing scheme that uses potential vorticity to separate the TC field from the background flow. Their proposed approach has been applied for dynamical downscaling assessment of regional-scale precipitation induced by severe typhoons in the past and in the future climate simulations. The probable maximum precipitation has been estimated by searching a worst-case typhoon track. The recent reviews on this issue were provided in Mori and Takemi (2016) and Takemi et al. (2016). Considering these previous works, the originality of the present study is not well described. Please consult with these previous papers and the references therein. The sentence on page 3, line 29 "this is the first study investigating a fully physically based method . . ." should be revised by incorporating some of the previous studies. Please articulate and emphasize the scientific merit in this study.

Another issue is the proposed method itself. The explanation described in Section 2 seems not to be clear. This reviewer does not understand how you would define the TC circulation from the background field. If you assume that a TC has an axisymmetric structure and that the TC circulation can be approximated as some type of analytical vortices (such as Rankine vortex), you could separate the TC circulation from the background field. But how would you determine TC-related relative humidity and temperature from the background? I think that there are some other assumptions; for example, the thermal wind balance should be assumed in order to derive TC-related temperature field. If you include moisture in the temperature definition in the form of virtual temperature, you could also derive TC-related moisture field. However, the current manuscript is lack of sufficient explanations on how to isolate TC field from the background. Furthermore, if there is a background flow, you may need to subtract

background flow field in order to obtain axisymmetric flow structure of the TC. In addition, the way to determine the radius R of the TC is not explained. Is this radius the radius of the maximum wind? If so, TC-related circulation outside the radius R should somehow be eliminated from the background.

Overall, although this reviewer understands the scientific importance dealt in the present study, the scientific originality is vague and the proposed method is not convincing. Major revisions must be conducted before considering the publication of the present study.

Technical Corrections:

1. The figures are not numbered in the order of their appearance. Please take special care when you revise.

2. Page 6, lines 8-10, “It spawned . . . and into the New England area (Fig.8c).”: Fig. 8c does not include the New England area, which is misleading. Please reword.

3. Page 8, line 23, IVT: What does IVT mean? Please spell out.

4. Page. 10, line 32-34, “ However, in the case of Hurricane Isaac . . . the maximized precipitation field is overall slightly less intense than the observed precipitation field.”: Figs. 13 and 14 does not include a panel of the observed precipitation, and I cannot evaluate if this statement is correct or not. Please add the figure showing the observation field.

References:

Ishikawa, H., Y. Oku, S. Kim, T. Takemi, and J. Yoshino, 2013: Estimation of a possible maximum flood event in the Tone River basin, Japan caused by a tropical cyclone. Hydrological Processes, Vol. 27, pp. 3292-3300, doi: 10.1002/hyp.9830.

Oku, Y., J. Yoshino, T. Takemi, and H. Ishikawa, 2014: Assessment of heavy rainfall-induced disaster potential based on an ensemble simulation of Typhoon Talas (2011)

with controlled track and intensity. Natural Hazards and Earth System Sciences, Vol. 14, pp. 2699-2709, doi:10.5194/nhess-14-2699-2014.

Mori, N., and T. Takemi, 2016: Impact assessment of coastal hazards due to future changes of tropical cyclones in the North Pacific Ocean. Weather and Climate Extremes, Vol. 11, pp. 53-69, doi:10.1016/j.wace.2015.09.002.

Takemi, T., Y. Okada, R. Ito, H. Ishikawa, and E. Nakakita, 2016: Assessing the impacts of global warming on meteorological hazards and risks in Japan: Philosophy and achievements of the SOUSEI program. Hydrological Research Letters, Vol. 10, pp. 119-125, doi: 10.3178/hrl.10.119.

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