Anonymous Referee #2

Received and published: 14 March 2018

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 rainfallinduced 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.

Responses:

Comment:

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.

Response:

The authors were not aware of these studies and thank the reviewer for bringing them to their knowledge. These studies will be acknowledged in the revised manuscript. The sentence “this is the first study investigating a fully physically based method …” as well as mentions to “a new physically based storm transposition” will be revised.

Comment:

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.

Response:

The method used to separate the TC circulation from the background field in this study is very simple. The radius R of a TC is established by visual examination of the different fields and in particular of the wind field. In general, looking at the wind field (see the example given in Fig. 4), it is relatively easy to see where is the TC and what is its size. In this study, we did not use any automatic procedure to establish the TC's radius but did it manually for each TC. Once the radius R was established, the inside of the circle of radius R was cut off, and the fields inside the circle recovered by interpolation of the background fields. The perturbation fields obtained by subtracting the interpolated fields from the original fields were then shifted and added back to the background fields. The state variables that we modified are:

1) Surface variables: skin temperature, temperature at 2 meters, relative humidity at 2 meters, wind speed at 10 meters, surface pressure, pressure at mean sea level

2) Pressure level variables: temperature, wind speed, relative humidity, geopotential height.

The authors recognize that the vortex bogusing scheme used in the studies referred to by the reviewer is more involved than what was done in the current study. In particular, recovering the different fields by inversion of the potential vorticity insures to obtain a vortex in the initial conditions which is fully consistent from a physical and thermodynamical point of view. Vortex bogusing has been widely used for forecasting purposes for this reason: it is essential in the case of forecasting to have the best possible estimation of the initial vortex so as to not miss the storm's trajectory. Is the case of engineering design such as for the estimation of PMP, as important as in the case of forecasting? The authors do not think so. The linear assumption made by adding the shifted perturbation fields to the background fields while the governing equations are nonlinear is an approximation which affects only the initial conditions. The nonlinearity of the system is then fully accounted for by integration of the Regional Atmospheric Model (RAM).

Answering the reviewer's questions:

1) the TC-related relative humidity and temperature were not obtained from the other fields: the method described previously to obtain the perturbation fields was also applied to the relative humidity and to the temperature.

2) R is not the radius of maximum wind. As explained previously, R was established by visual examination of the different fields.

The reviewer is asking for major revisions, without clearly explaining what points need to be revised in a major way.

1) As far as the originality of the study is concerned, the revised version of the manuscript will clearly acknowledge the merits of the existing studies for the shifting of typhoons over Japan. Yet, to the authors' knowledge, this study remains the first to use a physically based TC transposition procedure for Tropical Cyclones in the Atlantic basin.

2) It took months to perform the simulations presented in this manuscript. Although the approximation made in how the perturbation field is estimated and then added to the

background field result in an initial vortex that is not fully consistent from a physical and thermodynamical point of view, these inconsistencies are subsequently smoothed out by the RAM since it numerically solves the nonlinear equations governing the conservation of the mass, momentum and energy. Fig. R1 below shows the evolution of the total precipitable water (PW) in Hurricane Ivan during the 6 first hours of the simulation with an hourly time increment. The PW is constructed from the WRF outputs, and more precisely using the pressure field, specific humidity field, potential temperature field, and geopotential field. Besides, since the PW indicates how much moisture is contained in an atmospheric column, it plays a central role in the study of intense precipitation. If there was any particular aberration in the aforementioned fields due to our vortex relocation procedure, this would have detrimental consequences on the PW and would be observable in Fig. R1. The middle column corresponds to no shift whereas the left column corresponds to an amount of shift of 0.8° W and 3.6° S and the right column to an amount of shift of 0.8° E and 3.6° N. As can be observed in Fig. R1 no aberration appears in the PW: the TC which corresponds to the circular region containing large values of PW off the coasts of French Guiana and Suriname is shifted as expected.

The objective of this work is to show that it is possible to use a regional atmospheric model to offer a physically based approach to the estimation of PMP in the eastern U.S. This paper presents a preliminary study showing the feasibility of such an approach. The authors understand the reviewer's concerns regarding the fact that the initial fields are not completely consistent physically and thermodynamically. There is no claim in this study that the vortex relocation procedure presented herein is the right one and the one that should be used in future physically based PMP investigations. This vortex relocation procedure is very simple. It gives an approximation of the initial fields after transposition of the TC which obviously is not as legitimate as in the case of the studies referred to by the reviewer, for which the potential vorticity inversion method guarantees to obtain more consistent fields in the initial condition. Still, as Fig. R1 shows, the vortex relocation procedure presented herein does the job: the TC is shifted as expected. The simulation start dates are taken early enough so that the model smoothes out the inconsistencies that may exist in the initial conditions. By the time of landfall, the different fields including the precipitation field are fully consistent physically and thermodynamically since the RAM numerically solves the nonlinear equations governing the conservation of mass, momentum and energy.

Comment:

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

Response:

It is not clear to the authors why the reviewer comments that the figures are not numbered in the order of their appearance. Indeed the manuscript was written and compiled with Latex so that the figures have to be numbered in the order of their appearance in the document, which is the case.

Now it is true that the figures do not always appear in the order of their appearance in the text. For example the first figure that is referred to as Fig. 4 is for the illustration of the transposition procedure. This is because this figure is discussed in more detail in section 3 on the transposition of Hurricane Ivan than it is in Section 2, and as such, according to the authors, it has more legitimacy to appear later in the document. However, in order to address the reviewer's comment, Fig. 4 will be placed at the beginning of the article and will become Fig. 1.

In the same way, Fig. 8c for the observed precipitation field in Hurricane Ivan is referred to at the beginning of Section 2 but appears later in the manuscript because it is further discussed when analyzing the maximized precipitation field in Hurricane Ivan. Since Fig. 8c belongs to a panel plot, it will not be possible to move this figure from Section 3 to Section 2.

Apart from these two cases, the authors do not see any aberrations in the way the figures are ordered.

Comment:

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.

Response:

Figure 8c does not include the New England area because Fig. 8c shows the observed precipitation field inside the simulation inner domain and the simulation inner domain does not contain the New England area since it has been chosen based on the location of the target watershed located in western North Carolina. The sentence referred to by the reviewer aims at giving some historical information and context to Hurricane Ivan. In order to address the reviewer’s comment, “(Fig. 8c)” will be moved from the end of the sentence to before “and into the New England area”, so that the sentence becomes: “It spawned heavy precipitation ranging from 3-7 in depth along a large swath from Alabama and the Florida panhandle northeastward across the eastern Tennessee Valley (Fig. 8c) and into the New England area”.

Comment:

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

Response:

The meaning of IVT is given on Page 8 line 18.

Comment:

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.

Response:

The observed precipitation field for Hurricane Isaac is given in Figure 16c.

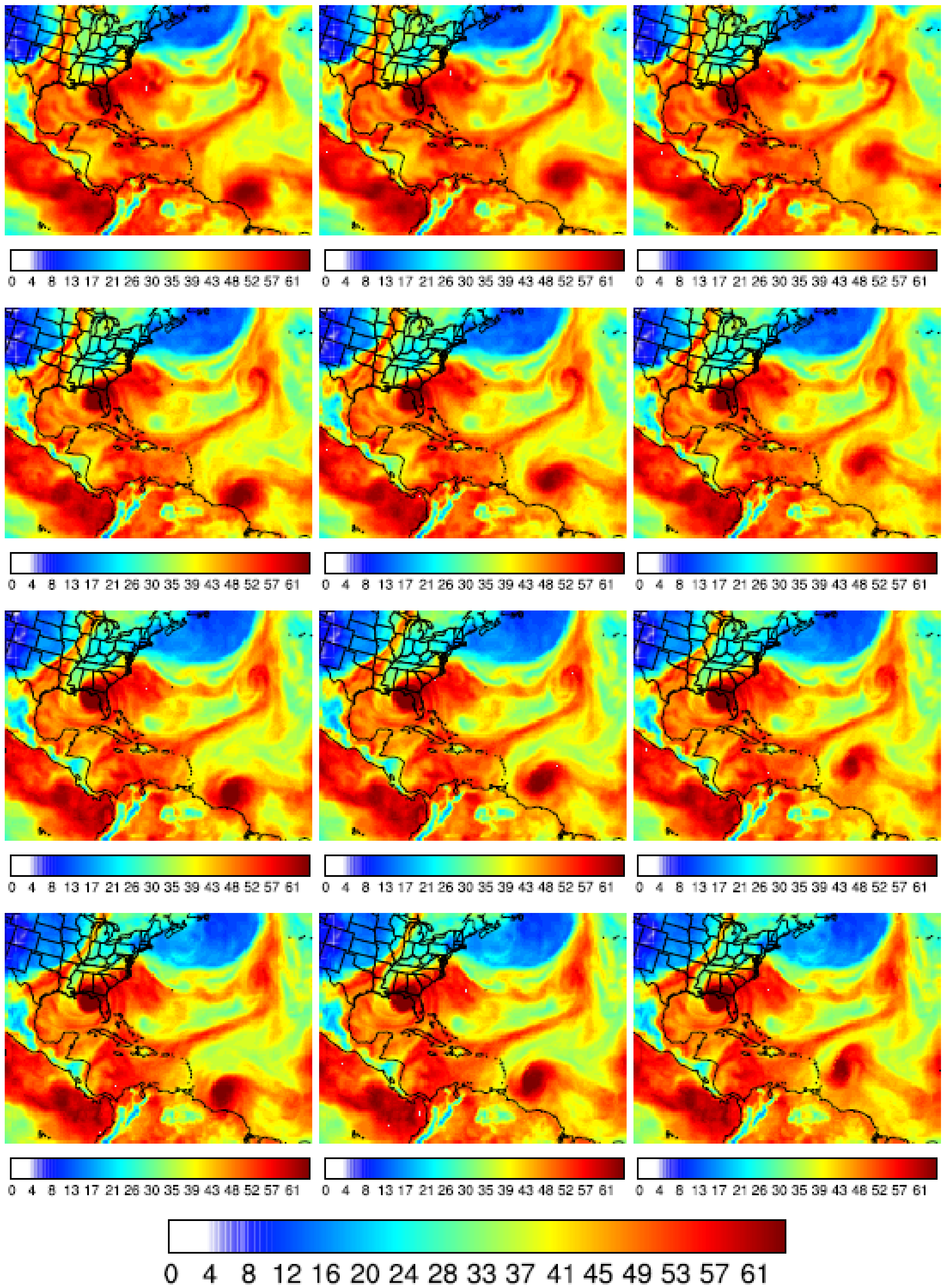

Figure R1: Evolution of the total precipitable water in Hurricane Ivan during the 36 first hours of the simulation with a 6-hourly time increment. The left column corresponds to an amount of shift of 0.8° W and 3.6° S, the middle column to no shift, and the right column to an amount of shift of 0.8° E and 3.6° N. Note that the TC over Florida is not Hurricane Ivan. Hurricane Ivan is located off the coasts of French Guiana and Suriname.