Response to Anonymous Referee #2

General Comments:

The authors present in their simulation-based paper an interesting diagnostic to quantify the characteristics (in terms of sensitivity to rainfall) of the flood hazard for the Chao Phraya River basin in Thailand.

The topic presented is generally of interest to the readership of the journal and follows a logical structure.

Together with the comments mentioned below, I recommend to thoroughly revise the manuscript, as there are several instances in the paper that require further clarification and discussion from the authors. Therefore, I suggest reconsidering the paper after major revisions.

We greatly appreciate your reviewing our manuscript and give us insightful and constructive comments. We would like to address the review comments as follows. In this document, the review comments are in black and our responses are in red.

Specific Comments:

Section Abstract:

1) P7028L13: The authors highlight that 'the presented approach is effective for large river basins', but it remains unclear what exactly is 'effective'. Is it an effective simulation or it the approach of evaluating the hydrological sensitivity effective? Please clarify.

We intended to say the simulation was effective for quantifying the sensitivity. Therefore we will modify the related part as:

"the presented basin-wide rainfall-runoff-inundation simulation was an effective approach to analyze the sensitivity of flood runoff and inundation."

Section Introduction:

2) P7028L23-24: Please specify how the interpretation of the additional 200mm rainfall can 'affect the understanding' of the flood characteristics.

Since the original sentence was unclear, we will replace the sentence as follows:
"Therefore estimating how the additional 200 mm of rainfall magnifies the runoff and flood inundation is essential to understand the flood characteristics in this basin."

3) P7029L22-27: Please rewrite the section, as it is not clear what the authors are aiming to convey.

We will rewrite the section as follows:
"Sankarasubramanian et al (2001) categorized hydrologic sensitivity studies into five types, which include a regression model based approach with historic records (Risbey and Entekhabi, 1996) and simulation model based approach with controlled input variables (e.g. Vano et al. (2012), Mizukami et al. (2014), Vano et al. (2014)).

4) P7029L22: The study of Sankarasubramanian et.al. 2001 is cited. However, the main conclusions of that study are not taken into account or even addressed in the discussion sections. Based on their results, Sankarasubramanian et.al. concluded that 'Both model choice and model calibration play an important role in determining the sensitivity of simulated streamflow. Therefore it is difficult, if not impossible, to estimate the sensitivity of streamflow to climate using a single watershed model'. Based on their conclusion, I recommend to add at least a section showing/discussion the influence of the manual model calibration (i.e. parameters choice)

We will describe the importance of employing different models and parameters for the sensitivity analysis as a limitation in the discussion section.

5) P7030 L9: 'They are generally difficult to define. 'Who is 'they'? Please specify.

We will revise the sentence as follows:
Most of exiting inundation models are applied only to floodplains and constrained by boundary conditions of upstream river discharges or inflow to the floodplains. Those boundary conditions are generally difficult to define if multiple locations are inundated in a large river basin.

Section Methods:

6a) Please add a section describing the general hydro-climatology of the Chao Phraya River basin (i.e. climate influenced by monsoon (how does that influence the climate), at what time in the year are the wet and dry seasons...) and of the 2011 event. I.e. how did that year differ from the average year (e.g. sequence of unusual events)? In the introduction, it is only mentioned that Oldenborgh et al. (2012) concluded that the year was 'not very unusual'...

6b) Additionally, to better inform the reader about the study area, add a information on the coverage of urban areas in the study region.

We will add a new section to describe the general characteristics of the basin and the flood disaster in 2011 as well as the information of urban areas.

7) P 7031L6: Fig 1 should be Fig 2 and P 7032L9: Fig 2 should be Fig 1

The order of the figures were wrong. We will correct it.

8) P7033L6&7: Specify meaning of the variables of equation (2) and (3).

We will add the definitions of the variables.

9) P7033L13: '...empirical equations cannot represent well. ..' Represent what? Please specify.

It was "represent" cross section with and depth. However after the revision corresponding to the Referee #1, the sentence of "represent well" will not appear.

10) P7033L17: Are these 400 stations roughly equally spread within the basin or sparsely for some region. Add one sentence on station coverage and the possible effect on the simulation.

We will add the following sentence.

Daily rainfall records were observed at about 400 stations almost equally distributed in the whole basin

11) P7034L17: 'G-A model' does this refer to the Green-Ampt equation mentioned before? If so, please add abbreviation in parentheses (P7033L28)

We add the abbreviation of "(G-A)" in section 2.2.

12) P7034L19: Here the threshold of 0.5 m water depth is described for the first time to define the area as being inundated by the flood. Please provide a thorough discussion why that threshold has been chosen and what the authors take is on how a different threshold would influence the ultimate outcome of this study (i.e. flood inundation elasticity).

We choose 0.5 m as the threshold to follow the previous studies (Sayama et al. 2012) since deeper than the level cause more severe damages typically with above-floor flooding (Okada et al., 2011). We will state it in the revised manuscript.

Regarding the impact of the threshold on the elasticity estimations, we recalculated it with 0.3 m, 0.5 m (original) and 0.7 m thresholds. In case with dams, the estimated elasticity values were 4.1 %, 4.2% and 4.4 %, respectively. Generally the higher thresholds result in higher elasticitiy because the inundation volume itself (i.e. the denominator of the second term in the equation (2)) becomes smaller. However we found the differences are less than 0.2 points in the above test cases.

13) Table 1 is not mentioned in the text, please add. Additionally, specify if the land cover classes 'forested area and cultivated area' correspond to the regions 'Mountains and Plains' mentioned in Table 1. If so, please homogenise naming convention. Also explain what the parameters mentioned in the table correspond to (in words).

We will add the following sentence in 3.1:

"Table 1 shows the calibrated model parameters for mountain and plain areas."

Also we will use "mountains" and "plains" consistently in the revised manuscript. As for each parameter, we will modify the explanation of the Table 1.

Section Model simulation results:

14) P7036L1: Figure 3 does not only show the discharge at C2 but also two other locations, change text accordingly.

We will modify the sentence by removing "at Nakhon Sawan (C2)".

15) P7036L6: the calibration focuses on 'naturalised C2 monthly discharges'. Please specify what model parameters were adjusted in the manual process (i.e. only the parameters for the two land classes (mentioned in Table 1) or other parameters as well?).

All the model parameters are listed in the Table 1 and they were manually calibrated for mountain and plain areas.

16a) P7036L11: two metrics are mentioned in the text however, the appendix only shows the NSE (the Figures show R2 but without any mention in the text). (Also, add a reference to the NSE) Here I would ask the authors not to only rely on a 'relative error measures' such as the NSE only, but also to include an volumetric error measure (e.g. 'mean error'), as the water volumes are also important for quantifying the flood inundation extend simulations. Please add information on this as well.

We will add the definition of R^2 and a newly added metric, Relative Volume Error (VE), in addition to NSE (with reference) to evaluate the simulated river discharges.

16b) Additionally, clarify if calibration was performed by focusing on the model performance measure only.

We describe this part as "Model parameters were then manually calibrated by focusing on the naturalized C2 monthly discharge". The following revisions corresponding to (17) and (18) also will clarify this point.

17) From Section 3.1., it is not clear if the model was ONLY calibrated to the discharge at C2 and the parameter settings were then used for the entire basin or if the subbasins at the gauging stations and dams were calibrated later. Please explain in the text and not only in the caption of Table 2.

We did not conduct additional parameter calibrations for each gauging stations or dams.

Related also to the comment 18), we will avoid the term of "calibration" for other locations except for C2. Instead, we show only evaluation indices for the period (i.e. 1980-2011) in Table 2.

18) P7036L16: If calibration is only done for C2 (if I understood the section 3.1 correctly) I would not use the heading 'Calibration' for the other stations in Table 2 as there are practically 'Validated' for the entire period and do not require a split into calibration and validation period. If all sub-basins were 'actually calibrated' 'I would suggest to apply a similar approach as presented in Table 3 and add the averages at the bottom of Table 2 as well.

Please refer to the response for 17).

19a) P7053: Figure 3: Please specify if observed flow for C2 is the real observed discharge or if this is the naturalised one. It is also suggested to change the line type of the 'simulated' discharge to 'dashed' or 'dotted', so that the reader can better evaluate the underlying observed discharge. Maybe also consider to add the 'underperforming' Y17, so the reader can better understand the degree of the poor performance mentioned in the text. Additionally, I suggest adding the following points to the caption of Figure 3: - which stations are above and below the dams - does the performance measures refer to calibration or validation periods. - Mention again the periods used for calibration/validation.

We will specify C2 discharge as the naturalized one. Also we will change the line type to improve the visibility. In addition, we will add the result at Y17 for better understanding of the model performance. Please note that among the cited locations, only C2 is located the downstream of the two dams. We will clarify this point in the figure.

19b) P7036L11-20 Briefly discuss the differences in the way how monthly peak discharges are being captured for the different locations shown in Figure 3.

We will modify this part as:
"Figure 3 shows the observed and simulated hydrographs at selected locations including N1 and P1. Except for Y17, the other locations also show reasonable agreement as summarized in Table 2 with the range of NSE between 0.67 and 0.89 (average NSE = 0.81)."

20) P7036L24-P7037L3: Add discussion on the reliability/quality of the remote sensed data in evaluation flood extend. Also, add a sentence if the observed flood extents between the year 2011 and the other years are expected to differ due to the different data sources mentioned in the text.

We will add a discussion on the uncertainty of the remote sensing inundation extents. In general the errors are associated to the choice of water detection index, the selection of threshold and other intrinsic errors in remote sensing (cloud cover, spatial-temporal resolutions etc.).

Regarding the difference between UNOSAT and GISTDA, only peak inundation extents in 2011 have the estimations from the two data sources. The CSI index between the two data was 0.54, suggesting how difficult and uncertain in the estimations of large scale flooding like the one in 2011 at the river basin scale.

In this study, we decided to use UNOSAT for 2011 since our field survey suggested its reasonable identifications at least within the lower delta where we could visit after the flood (also used in Sayama, et al. 2015). In general, we cannot expect to calibrate the model in detail by comparing with the simulated inundation extents. Instead, our objective here was not to understand how the model in general agrees with the remote sensing for different scales of flooding in different years. We will clarify this point in the revised paper.

21) P7054: Figure 4, Please increase size of the figure as currently a spatial comparison is hardly possible. Additionally, add location of C2 (for reference) and legend with colour scale explaining the colour codes used in the Figure. Add to Figure caption a note on the different data sources of the remotely sensed data.

We will add the C2 location for the reference and color legend. Also we will revise the caption to explain the source of the remotely sensed data. As for the size of the figure, please allow us to consider it based on the final format.

22) P7037L7 & Appendix: Innundation is evaluated using ANE and FIT. a) I'm not convinced if FIT is the best performance measure to use here. As it only evaluates the 'matching' pixels between observed and simulated inundated areas with respect to the 'lumped' areas that are flooded independently of referring to simulated or observed areas. This does not allow evaluating in detail, the goodness of the model performance in terms

of pixel overlap. Instead, I advise using two measures that, while similar to the FIT measure already used, enable a clear evaluation of: I) how much of the observed extent is captured by the simulation ((IAobs aLl' IAsim) / IAobs). II) how much of the simulation extent actually captures the observation ((IAobs aLl' IAsim) / IAsim).

We will add the two suggested indices to evaluate simulated flood inundation extents with remote sensing.

22b) Additionally, instead of using the 'absolute normalised error' I would use the 'normalised error' as this allows directly inferring from the values in the table if there is an under or overestimation (although from Figure 4 it can be seen that it is an undersestimation).

We will use “normalized” error instead of “absolute normalized” error. Thanks for the suggestions.

23a) P7037L15-24: Using the new model evaluation measures outlined above, a better evaluation of the simulation performance will be possible, as especially with regard to weather the simulated cells actually match the observations. Then a more detailed discussion on the differences between large and average floods can be made. Additionally avoid terms like 'some underestimation' and rather quantify amount.

We will use the newly introduced evaluation indices for further discussions and avoid the ambiguous terms.

24) Explain in text if only the depicted simulations 2005-2011 were used to evaluate the performance or if the entire series was used. If so, maybe a time series showing the obtained model performance indices or some other sort of summary of the simulation performance might be appropriate.

For flood inundation extents, we used only 2005-2011 due to the availability of the remote sensing information. We will add the comment in the revised manuscript.

25) P7037L25-29: Please but this section in context with the results obtained above. So what do the mentioned points mean for the simulations? Please clarify and expand.

We will explain more about the inundation depth survey for 2011 and the comparison with the RRI simulation.

Section Sensitivity of flood runoff and inundation

26a) P7038L5: Figure 5 does not show days of the year but rather the daily values? Additionally, it appears that not only rainfall is a cumulative water balance component but also ET and runoff. Please clarify in the text. Why is there only little difference between wet & dry season in the ET?

We will simplify the related sentence as

Figure 5 shows the daily values of cumulative rainfall, cumulative actual evapotranspiration, catchment storage, cumulative runoff and flood inundation, respectively.

Regarding the seasonal variations of ET in this region, Tanaka et al. (2008) reported that tropical evergreen forest in the basin has deep soil layer (~5.3 m), which allows fairly steady evaporations throughout a year. Essentially the model behaves also similar way; i.e. stored water in soil layer is evaporated in dry seasons. For the PET, as described in the main text, we used Penman Montieth equation with spatially and temporally variable LAI. As a result, our estimation showed rather stable ET, whose pattern is similar to the previous report (Figure 8 in Tanaka et al. 2003).

26b) The grey lines in Figure 5 are barely visible, please increase line width. I might be interesting to add the two other major floods with different colours as well, so that comparisons between the events become possible (This also applies to Figure 7 and 8 in which labels with years could be used for the extreme points). Specify in the figure caption the period used to calculate the average and if the simulated water balance includes the dams.

We will increase widths of the gray lines in Figure 5. Then we will add years (1960-2011) for the average in the caption. As for the different colors in different years, we would prefer to keep the original idea to focus on the general characteristics and 2011 as a highlighted case. We agree that different factors including temporal and spatial patterns of rainfall influence on the water balance components. However, to discuss the characteristics in quantitative way is rather difficult within the scope of this paper.

27) P7038L11-16: provide a more in depth discussion of all the water balance components shown in Figure 5 with particular reference to the 2011 event (e.g. exceptionally high rainfall in mid-March, or flood inundation started a month earlier compared to an average year...) Additionally, substitute the word 'trend' (P7038L12) with the appropriate description.

We will revise the section thoroughly. The suggested point and small ET variations are discussed more in detail in the revised manuscript.

28) P7038L17-20: Remind reader in one sentence as previously explained why this analysis is done (put into context). Please also clarify if the months of cumulative rainfall are counted from the start of the year or counted from the peak inundation backwards. Is the full period (1960-2011) used or only the 'post dam building' period. Clarify in text and caption

We will add the following sentence:
To analyze the relationship between rainfall amounts in different durations and peak inundation volumes, Fig. 6 plots cumulative rainfall counted backwards from the peak inundation at each year (x-axis) and peak inundation volumes (y-axis).

29) P7056: Figure 6, can you use colors to indicate the points (years) that belong to the three largest inundation volumes. Additionally add explanation that this is cumulative rainfall.

For the same reasons as the response to 26b), we would like to keep the original figure for the sake of simplicity.

30a) P7056 and P7057: According to the description, the relationship shown for Figure 6 (6months) and Figure 7 for the variable of inundation volume should be the same. However, when examined closely, they are depicted differently. In Figure 6 (6 months) the line intersects zero inundation at a rainfall of 800mm, whereas in Figure 7, the red line intersects approximately at 900mm! Please explain why the two figures are different!

Fig. 7 draws the regression line with top 35-year records in rainfall (P > 950 mm), while the Fig. 6 used all the points. Although the original manuscript stated the method for the regression, it was still unclear. The revised paper will further clarify this point.

30b) P7057: Figure 7: Please add the R2 to the different established relationships (This also apply to Figure 8)

We will add a table to present the regression analysis results including R^2, slope, intercept and their p-values.

31) P7038L21: are the relationships established at the time of peak inundation? Please specify.

Yes. We will explain it by adding "at the time of peak inundation" in the sentence.

32a) P7038L24: The modelled ET and the established linear relationship with P seem odd. Please further discuss, why the model is producing such an outcome

According to the liner regression between P and ET, they are positively correlated (with the slope of 0.061 but statistically insignificant) (the values will be added as Table 4 in the revised manuscript). In the Chao Phraya river basin, previous study reported steady evaporations throughout a year regardless the amount of rainfall in different seasons and years in evergreen forest with deep soil layer. Essentially the model behaves in a similar way; i.e. stored water in soil layers during wet seasons is used for evaporations in dry seasons. Therefore, the variations in ET are comparatively smaller than other water balance components.

32b) P7038L25-26: remove the vague expression 'some correlation' from your discussion and rather quantify the type relationship observed. Additionally, all the relationships established in Figure 7 (also Figure 6 and 8) are linear, however here briefly a 'plateauing ' i.e. a levelling of the relationship is mentioned, however without further discussion. I strongly suggest adding a discussion for which precipitation ranges the authors consider the established linear relationships as being valid, particularly with the focus on extreme rainfalls (as they are of specific importance for elasticity analyses).

We will thoroughly check the manuscript to remove those vague expressions and describe the results quantitatively with a table showing the statistics. The expression of “plateauing” was originally used to describe the possible upper limit of storage change in Figure 7. However only a point with 2011 simulation cannot be a strong evidence. Therefore we will remove the statement of the plateauing, instead discuss more the characteristics of ET as pointed out above.

33) P7039L1: Change order of 'flood runoff and inundation volumes' to 'inundation volumes and flood runoff' to correspond to the order of the components as presented in the text.

We will revise the manuscript based on the comment.

34) P7039L2: 'that' is unclear. Rewrite

We will rewrite it.

35) P7039L2: how is the 6 month rainfall of 'normal years' being determined? Average of 52 values?

Yes. To make it clear, we will rewrite the section by stating “the average six-month rainfall is about 1000 mm”.

36) P7039L9: Table 4 is missing

Table 4 will be added to show the quantitative results of the regression analysis in Figure 7 and 8.

37) P7039L17: Why 2 additional months for discharge? Add section explaining this choice.

We will add the following explanation:
we decided to extend the water balance calculation period for two months after its inundation peak, so that the inundated water is receded to turn into runoff and other water balance components.

38) P7039L18-19: I don't see why for dF the analysis is also shown in the Figure. This does not yield any additional valuable information. If the authors decide to keep the Figure as is, swap the description of figure 8b to 8c).

We follow the suggestion and will show only the inundation volumes in the panels a) and b) with six month rainfall, and the runoff volumes in the panels c) and d) with eight month rainfall.

39a) P7058: Figure 8: I would suggest reducing Figure 8 to showing only the inundation in panel a) without dam and b) with dam for the 6 month precipitation and c) and d) for the discharge only for the 8 months.

Same response as 38)

39b) Please indicate if the same years (number of years) have been used for both panels (without and with dam). If different time number of years have been used (in Figure 8 it looks as if the 'with dam' panel has less data points). I recommend using the use the same years, to ensure that the differences in results (different relationships) obtained are actually due to the effects of the dams and not due to the presence/absence of years.

In the original manuscript, we used two different periods: 1960-2011 for "without dam" and 1980-2011 for "with dam". However for the better comparison, as suggested by the reviewer, we will plot only 1980-2011 in Figure 8 to analyze the effects of the dams.

40) P7039L27-P7040L9: Please move this section to earlier in the paper when the elasticity index is introduced and discussed. Here only a shorter section on the index characteristics is required.

We will move part of this section to Section 4.2, which introduced the concept of elasticity.

41) P7040L9: 'the results suggest...' Please indicate to which panel this statement is referring.

We will replace the expression "the results suggest" with "Figure 8 (a)" to identify the panel in Figure 8.

Section summary and Limitations:

42a) P7041L2-14: In this section please always specify the base period used (i.e. 6 vs. 8

months) for rainfall and inundation or runoff and if dams were considered or not. Also add a note if there is a big difference between 6 and 8 month rainfall. If there is a big difference then Figure 9 has to be adjusted to accommodate the differences in rainfall for 8 and 8 months.

In this section, we discuss the results of 6 month inundation and 8 month runoff with dams. We will make it clear in the revised manuscript. Regarding the difference between 6 and 8 month rainfall, since the flood inundation starts decreasing after a rainy season and we take the date of peak inundation for the end of the six month period (and extend two more month for the total runoff estimations), we confirmed that the difference between 6 month and 8 month rainfall was negligibly small; for example, in case of 2011, the difference was only 1.3% of 6 month rainfall. For the sake of simplicity, we will keep the notation of rainfall amount in Figure 9 as it is.

42b) P7059 Figure 9: please specify in the figure caption if dams were considered.

We will add the "with dams" in the figure caption.

43) P7041L18-28: Reorder the order of the terms used, either in equation 5 or change the order of the terms described in this section to have the same order as the original equation.

We will reorder the description in this section to make it consistent with the equation (5).

44a) P7042L2: Please clarify what a 'historic regression based approach' means.

We will add the following line to explain our approach:
".. approach, which generates synthetic records of flood runoff and inundation from a model simulation and fit regression lines to estimate the relationship between rainfall and the variables."

44b) P7042L10 and L13: 'six months' were only used for inundation; please add the 8 months for runoff.

We will add the following notation in the line:

"(and two more months were extended for total runoff analysis)"

45) P7042L13&14: Please specify how the other factors mentioned in the text might influence the flood simulations and particularly the estimation of the elasticity.

The other factors including spatial and temporal rainfall variations and antecedent conditions influence on the deviations from the simple relationship between rainfall amount and the other water balance components. Our standpoint in this study is those hydrologic effects should be reflected in the elasticity estimations. In fact, by using the RRI model, we represent these effects in the simulation, consequently also in the estimation of the elasticity. We will restate this part to clarify our standpoint.

46) P7042L16-18: Please expand on how the 'flood hazard' of the study region can be 'quantitatively understood' by this study, as this is not clearly articulated.

We will revise this sentence by adding the following more specific explanation:
"which helps to quantitatively understand how much monsoon rainfall turns into flood runoff and inundation volumes in this region."

47) In the 'limitation section' an in depth discussion (advantages/caveats) is needed on the use of linear relationships between rainfall and the other components of the water balance equation. Particularly with a focus on extreme rainfalls (low and high) and the possibilities of non linearities with regard to the estimation of the elasticity indices and the quantification of the flood hazard.

We will add the following sentence to address the "non-linearity" issue.
"Another reason for the deviation is due to the linear regression between rainfall and other variables. Although this study employs the linear regression because of its simplicity and robustness within the range of historic rainfall, the linear regression may be inadequate for unprecedented extreme events in the future."

Section Conclusions:

48) P7043L7-10: Here an interesting point is raised that had not been mentioned before. I would suggest to introduce the issue of the dam management being made responsible for the flood damage in the abstract or at least mention it in the introduction. So far, in

the introduction (P7029L3-4) only the possible effects of the conversion of the agricultural land into other uses has been elaborated.

We will mention the dam management being made responsible for the flood damage in the introduction.

49) P 7043L17-25: I caution to bring the current study in relation with climate change impact analyses and simply extrapolate the linear relationships established. Particularly, with the expected non-linear response of the hydrological systems including the monsoon.

We agree that the estimated elasticity indices cannot be simply used for the climate change impact assessment. Here we intend to say for giving caution that seemingly insignificant rainfall change has significant impact on runoff and inundation in this basin. To make our point clear, we will revise the tone of the paragraph and address again the issue of non-linearity for unprecedented extreme cases as well as other climate change factors including temperature.

50) In the introduction the possible effect of conversion of agricultural land are being presented as cause of the extreme flooding in 2011. However, in the study it appears as if the model parametrisation had been kept constant. Could the authors please comment on how this assumption influences the estimation of the elasticity indices. This is of particular importance as the authors highlight that their elasticity indices can be used for quantifying the flood hazard.

We will revise the last paragraph in the conclusion to state the presented approach does not consider the effect of landuse change; and therefore, the use of the estimated elasticity values need careful attentions for future projections. Nevertheless, we would still like to emphasize the importance of analyzing the simulated long-term hydrologic variables including flood inundation volumes to understand the characteristics of flooding in a basin.

Typographical corrections needed: P7029 L16: correct 'rainall' to 'rainfall' P7030 L2: correct 'elasiticiy' to 'elasticity' P7030 L3: 'knwoledge' to 'knowledge' P7030 L6: 'luck' to 'lack' P7030 L14: 'interact each other' to ' interact with each other" P7036 L3: 'parmeters' to 'parameters' P7036 L11: 'metrices' to 'metrics' P7036 L13: 'faily' to 'fairly' P7036 L14: 'valiation' to 'validation' P7037 L27: 'floodplauns' to 'floodplains' P7040 L14: 'consiering' to 'considering' P7040 L15: 'resoivoirs' to 'reservoirs' P7042 L23: 'sensitivites' to 'sensitivities'

We apologize for many typos and appreciate it so much for your detail review on our manuscript.

References

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2) Tanaka N., Kume, T., Yoshifuji, N., Tanaka, K., Takizawa, H., Shiraki, K., Tantasirin, C., Tangtham, N., Suzuki, M.: A review of evapotranspiration estimates from tropical forests in Thailand and adjacent regions, Agricultural and Forest Meteorology, 148, 807-819, 2008.
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