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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Future changes in flash flood frequency and intensity of the Tha Di River (Thailand) based on rainfall–runoff modeling and advanced delta change scaling" *by* S. Hilgert et al.

Anonymous Referee #2

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Review of "Future changes in flash flood frequency and intensity of the Tha Di River (Thailand) based on rainfall-runoff modelling and advanced delta change scaling" by Hilgert et al. The study assess future climate changes impacts on floods and flash floods of a river in a mountainous catchment in Thailand through the process of RCM output, a bias correcting method (ADC) and the HBV model. Climate impact studies are in needed to assess future challenges in sensitive flood-prone areas. However, future climate change studies based on GCM scenarios are inherently uncertain and it is very important to assess the uncertainties in such a study as well as the assumptions of the methods used. Further, the raw RCM output is highly questionable (approx. over-

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estimation by 225% of precipitation!), which would mean that the modelled water cycle in the model is not physically realistic. These major caveats undermines the validity of the model exercise and I therefore do not recommend that the paper is accepted for publication.

Major comments

1. Uncertainty. As already stated the uncertainty in any modelling should be an intrinsic part of any modelling study. The HBV model is calibrated and validated, which in table 2 indicates a suboptimal parameter estimation since the validation is considerably worse than the calibration period. This needs further exploration in terms of the modelling setup and calibration, which a more thorough sensitivity analysis could shed light on. The scientific literature is full of good examples of how to deal with this issue

Most uncertainty in climate change impacts stems from the climate model output, and it is clear that the RCM is underperforming in this study. An overestimation of precipitation with 225% of annual values is not reasonable and the model should be rejected. A further investigation of the RCM output, for example pooling climate model output from neighbouring grid cells could give a hint as to if the model performs equally bad over a larger area and also give a better estimate of the uncertainty.

2. Statistically significant changes. Related to the above topic, the values of precipitaiton and discharge change are given as absolute numbers with very high precision, which is absurd given the assumtions behind the scenarios and the uncertainty in the implementation. Please provide propoer estamiates of the range for the changes and whether they can be said to be statistically significatn or not.

3. Quality of RCM output and bias correction. There has in recent years been an increase in the area of post-processing climate model output, so called bias correction or model output statistics. In this paper, a method proposed by van Pelt et al is used. However, there are very few references to other studies and reviews in this field, and the authors should provide a better background as to why this method was the preferred

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one and its weaknesses and benefits. The paper would need more references to the vast amount of earlier work in section 3.2

4. In the methodology of van Pelt, how do you deal with correlations between parameters? How does a possible temperature bias affect the results? You state in section 4.2 that the biases are corrected for, and you then go on to claim how much each scenario will affect precipitation (including very precise changes in percentage, see above comment on uncertainty). What is the signal to noise ratio of these changes? How big were the projected changes before the ADC? Has the ADC altered this signal, and if so, is that physically realistic? In Section 5.3, you discuss the non-linear effects of the ADC, but how much of the change in signal is derived from the ADC?

5. Stationarity. In L10-11, P7332 you state that a suffuciently calibrated and validated model can be legitimately used in claimte change studies. This is not true. The model can be used in the climate in which it was calibrated, but that does not necessarily state anything on how it would behave outside its calibrated range.

6. Section 4.5 is highly speculative, and the arguments put forward are based on very uncertain calculations, for example that there is a 133% increase in 125 m3/s threshold exceedance for a scenario (see also above comment on uncertainty). I do not agree that the statements here are supported by the results. The word “significant” is used here, but in what sense do you mean significant?

Minor comments.

1. L1-5 P7328. You state in the abstract that extreme flood-causing precipitation events are expected as a consequence of climate change, however, you then state that Thailand suffers flash –flooding every 2.2 years. This is not an extreme event, but rather the normal situation.

2. L6. P7328. I react to the phrase “harmful discharges”, I would suggest “hazardous” instead.

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3. L24 P7329 – L8 P7330. This part should belong to methods rather than introduction.

4. L9 P7330 -25 P7330. This whole section is too detailed and can be deleted or replaced with just a few lines describing the broad structure of the paper.

5. P7331, Sectoin 2.1. You talk about flash-flood in this part, but it is not clear to me why these are not described as floods. To me, a flash flood would have a short life span and be mainly rain-induced. Hower the riveer banks overtopping sounds more like a fluvial flood. Please state more clearly how you define a flahs-flood.

6. L3-5 P7333. Do not understand this reasoning.

7. L6 P7333. First sentence does not make sense

8. L17-25, P7338. So, here you state that the observed precipitation values are of very poor quality? Why is this uncertainty not taken into account in the modelling exercise? I also do not understand the argument why using model climatology is better because the observations are of poor quality. Please explain.

9. L13-15, P7339. Here you state that the validity of ADC depends on the validity of the GCM and RCM. If that is the case, then would not the poor performance of the RCM in this area deter you from applying ADC?

10. L20-22. You state there that ADC has a "limited ability to account for long-term-shifts in seasonality". I would argue that it has none, since it is not a physical model, but a statistical post-processing. Unless you force it to include shifts in seasonality it does not have that ability.

11. In the conclusions you state that even todays moderate flooding is a problem, and this might have a dramatic increase (30%) under a future climate scenario projection. Given the vast uncertainty of the climate impact study, I would argue that measures needs to be taken to cope with the situation today. A future climate situation might worsen the situation, but that is very uncertain. The cost of doing measures will have to be weighted against the potential loss, and a cost-loss analysis would be very helpful

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here to understand the correct measures.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 7327, 2015.