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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Risks of seasonal extreme rainfall events in Bangladesh under 1.5 and 2.0 degrees' warmer worlds – How anthropogenic aerosols change the story" *by* Ruksana H. Rimi et al.

Anonymous Referee #3

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This study focuses on the investigation of changes in total and extreme precipitation in Bangladesh due to changes in greenhouse gas and aerosol concentrations. Large ensembles of regional climate simulations are used to represent regional dynamics and aerosol effects with sufficient detail and at the same time obtain statistical robust results also for extreme events with long return periods. In my opinion, the research presented in this manuscript is generally sound and provides novel insights (although not outstandingly new/innovative) into future rainfall changes in a highly impact-relevant region. However, I think the presentation and discussion of the research needs a

substantial revision before the manuscript could be published. In my view, the interpretation of the results is too superficial at several places throughout the manuscript. Furthermore, the language and wording are not always adequate. With respect to the second point, I list a few issues below, but this list is not complete, and actually the native speakers among the co-authors should be able to fix this in a better way than I am.

Specific comments:

Page 1 Line 22: As this is a model study, I'd avoid the term "impacts were observed"

P 1 L 28: "specifically with respect to...": I was confused when reading this as I though the whole study would focus on extreme events. It is not clear from the abstract that also seasonal mean rainfall is analyzed.

P 2 L 16: Nirapad (2017) is not in the list of references

P 2 L 23: "help to provide...": wording issue

P 3 L 12-14: I think these detailed information regarding the sub-regions would fit better in the methods section.

P 3 L27: "observational" appears too often in this sentence

P 3 L 35: This is my only purely methodological comment: I am a bit skeptical with respect to the usage of bi-linear interpolation, as this does not conserve the area-average rainfall amount and also biases the extremes compared to the original grid point values. I would ask the authors to at least test the sensitivity of their approach using a more appropriate conservative interpolation method (see, e.g., Chen and Knutson, 2008, doi:10.1175/2007JCLI1494.1).

P 4: The fact that some experiments are mentioned twice (in the first paragraph and further below) leads to some repetitions.

P 4 L 16: I'd mention already here in which way the ensemble members differ from each other.

P 4 L 33-34: This notation is awkward. I'd either write this in text form or as a "real" equation, but not mix these things up.

P L 12-15: This description of the aerosol affect is too short and not very clear. The term "omitted aerosol induced rainfall" should be explained. I am also confused by the sign of the signal and the figure caption: The caption of Fig. 2 says that the figure shows present-day relative to GHG only; positive values would thus mean that the present-day rainfall including the aerosol effect is larger than the rainfall due to GHG only, which is not consistent. Finally, before directly linking this result to potential future decreases in the aerosol effect already in the second sentence, the actual content of the figures should be described and explained.

P 6 L 19-28: I think this whole discussion is too superficial. There are many speculations on how thermodynamic and dynamic effects could influence the precipitation changes which, in my view, are speculative and should be based on a more quantitative and solid analysis. For instance, I am not sure how an "approximately linear" scaling is deduced from the data presented in this study. If this just refers to the differences between the 1.5 and 2.0 simulations, I could well imagine a case in which precipitation increases due to both increase in the atmospheric moisture content and in the monsoon circulation, and this increase is amplified in the 2.0 case, which may also produce a linear change over these simulations. Moreover, is a linear scaling really what we expect thermodynamically? The Clausius-Clapeyron relation is non-linear. The conclusion that dynamic changes play a secondary role should be manifested in a quantitative way. Also the statement that the thermodynamic response "usually scales with 20-40% of Clausius Clapeyron" is vague and, as such, not comprehensible.

P 7 L 1-2: I cannot follow here: In the region with the strongest decrease in Fig. 3a, the aerosol effect is small.

P 7 L6: The abbreviation "SPI" has not been introduced.

P 7 L 11-3: Again I cannot follow. For instance, the changes in 5b,d are similar to those in 4b,d

P 7 L 14: It is difficult to assess the relative change at this point, as the figures show absolute values.

P 7 L 16-17: This doesn't fit with the interpretation in the figure caption. In general, I find it difficult to shift parts of the discussion to the figure captions.

P 7 L 18: lapse rate and stability changes are not different feedbacks

P 7 L 21: Again I don't understand what "linear" increase means here.

Section 3.3, first paragraph: There is an imbalance between the amount of text/discussion and the number of figures. The reader is left alone with much of the material shown in Figs. 6-9. Either expand this discussion, or, if you think that the results are not that important, move parts of the figures to the supplement.

P 8 L 4: "appear to counter": I cannot see how you come to this conclusion.

P 8 L 24: "to a lesser extent": Really? Aren't the relative changes larger for the extremes?

P 8 L 31: "we conclude that the drier subregions...": I don't think this has been demonstrated. To show this, the masking effect has to be quantified. Furthermore, it is not clear to me which region and season you're referring to.

Fig. 1: I think this figure is too busy. I cannot distinguish the different shadings and also the lines of the observations are hard to see.

Fig. 3: shorten caption (as Fig. 2, but for the monsoon season)

Fig. 4: I cannot follow the interpretation in the caption. For instance, I don't see such large differences in the masking effect between the regions. More in general, it is hard for me to understand how the masking effect is quantified here.

Fig. 10: Again, the interpretation in the caption is unclear (e.g., which region and season are you referring to?)

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