Reply to comments to referee 1

I thank the authors for the revised version of the manuscript. The "Results and Discussion" section and the figure quality has substantially improved. However, the Introduction and "Data and Method" parts are not yet appropriate. Currently, I can not recommend publication.

We wholeheartedly thank the reviewer for their measured and constructive criticism, which we are addressing in detail below.

But first, we owe the editorial team (who have gratefully reopened the review process after we have failed to stick to several deadlines) and the referee(s) an apology for the extremely long delay of the revision. The timing of the reception of the current round of reviewer comments (November 2019) coincided with the end of the PhD thesis of the main author. Ruksana not only had to defend her thesis, but leave the country, suddenly raise two kids on her own given the partner had to stay in the UK, and to get to grips with her new role as assistant professor in Bangladesh. And then, as we all know, came Covid, which complicated things even further. It has only been recently, that things have calmed down a bit, re-enabling Ruksana to start thinking about the paper revision.

As second author, I had to move institutions during Covid as well, along with a major shift in priorities. Therefore, on behalf of the whole author team, I hope you can accept our apology and would be willing to review this set of revisions. We thank everyone involved in advance!

Main Comment
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* The text (mainly the Introduction and Data and Methods) really need to improve. There is a large number of mistakes and inconsistencies that can be corrected and improvements that can be made. Most of them are small but they add up. I ask all co-authors to work on the text such that it can reach publication-quality.

We have completely revised the manuscript, re-arranged a few paragraphs, deleted confusing formulations and added additional context where necessary. Especially the Data and Methods section has been overhauled extensively. We do admit that the previous version wasn't appropriately revised before submission, for which we apologise as well.

In the following, we address each of the major and minor comments individually. We have revised the text suggestions accordingly, unless the text has been deleted or reformulated altogether.

Major Comments
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* It would be very helpful to include a figure (or a table) summarising the global mean temperature and aerosol levels over South Asia for each experiment.

This is an interesting suggestion, but given we are using an RCM (HadRM3P), the global mean would be based on the driving AGCM (HadAM3P). This in itself would still be a useful information, except that ACT (historical) and NAT (counterfactual) RCM scenarios are both based on ACT HadAM3P simulations. The only difference is in the radiative forcing and the delta SSTs (based on CMIP5 (historical minus historicalNat) are different, plus sufficient spin-up so that the atmosphere can respond to the respective scenario. Those delta SST numbers are provided in the HAPPI paper for various AGCMs, but it is not the global mean, which essentially cannot be estimated w/o a dedicated HadAM3P NAT simulation. It is even more complicated with the aerosol levels, as we'd need to diagnose the regional forcing, which would require an entirely new analysis altogether. We could plot the aerosol concentrations, but that would be somewhat misleading as the resulting aerosol forcing patterns are usually very different. Hence we are afraid that this suggestion cannot be sensibly implemented. We hope this detailed explanation is good enough a justification.

* The aerosol levels are "0" (NAT, GHG-ONLY), "1/3" (H1.5, H2.0), and "1" (ACT). Thus, in all comparisons between experiments (except ACT - CO2-ONLY) there is a change in global mean temperature and aerosols. This makes it very hard to disentangle (in my head) the contributions of global mean temperature and aerosols. One way around this (assuming additivity of the responses) would be to conduct a linear regression: (mean) rainfall as a function of global mean temperature and aerosol levels for each season and region. (I think this could be very insightful but I also understand if you deem it beyond the scope of the paper).

As you rightly point out, the only change in ACT vs GHG only is the aerosol level. Therefore, to detect the changes associated with anthropogenic aerosols, it is reasonable to assume that the difference between those two scenarios can be interpreted as the (inverse) net aerosol effect. Both, the associated (regional) temperature and rainfall change would be attributable to aerosols and can be used to qualitatively disentangle the forcing contributions, as we have tried to demonstrate. Any regression analysis would have to take all contribution factors into account, i.e. it would have to be a multiple regression analysis, which is hampered by the same problems that we outlined in the previous reply. Again, it is an interesting suggestion, but at this point it would be beyond what we are able to include in this paper.

* Is my summary of your results correct?: "In general, higher global mean temperatures lead to higher rainfall and higher aerosols to lower rainfall, however, the relative importance of the two varies between the regions"? If so, it would be good to add such a sentence at the beginning of the results as it would help to understand the rest of the paper.

Yes, that is a fair assessment of the results. We have included this sentence in the introduction to put it as prominently as possible.

* I still don't find the notion of a "linear response" fitting. Linear can be defined as "arranged in or extending along a straight or nearly straight line". Given the change in aerosols you would not expect a linear response between the experiments (unless the aerosols do not play a role). I would say "monotonic" or maybe "gradual" would be more appropriate words here.

We agree. The same issue has been raised by the other referee. We have therefore changed the wording to 'monotonic' where ever possible in order to avoid contentious interpretations.

* You still mention results in the figure captions. Remove them.

Amended.

Minor comments
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* Is there a reason the regions are called "sub-regions"? I'd recommend just calling them "regions".

We never really thought about it to be honest. In some sense, we consider Bangladesh as our main region (within the larger South Asian regional model domain), with the four smaller regions within Bangladesh as sub-regions. So we decided to leave it for now and hope our argument is convincing.

* P4 L37-L42: "representing the current decade" has a strange ring to it, as it basically is a pre-industrial simulation. Can you reformulate.

We agree. It might have been close to a current decade when we set out to do the analysis, but given how much time has passed since, we have reformulated the text and use 'recent decade' instead.

* In Figure 2 and Figure 3 (also Section 2.3) you compare e.g. "ACT to NAT" - I had more trouble than necessary figuring out which way round this actually is - it would be so much easier if you wrote this as "ACT - NAT". (NOTE: in the supplementary you write this as "ACT relative to NAT", i.e. the other way round).

Valid point. We hope that the revised version of the manuscript is more consistent now. We always show the difference between ACT and NAT, i.e., how has rainfall changed under ACT relative to NAT conditions (with NAT being the baseline).

* You can delete Table S1, all information is contained in the text (once you mention the one missing resolution).

Amended.

* P11 L9-L22: This section belongs to the results.

Amended.

* The boxes indicating the regions in Figure 2 e got lost.

They are corrected now.

* The abbreviation SA (in the figure captions) is not defined.

Amended.

* P7 L29: I don't think this is a confidence interval. I would call that "range".
P7 L37: I am still not happy with this formulation: you do not know whether the bias is present in all scenarios - you assume so (and it is fine to assume this), but you do not know.

We have changed it to uncertainty range and added an additional paragraph to clarify the bias problem for different scenarios.

* Not all colors in the figure captions are correct.

Amended.

* You mix red and green in the figures, which is not colorblind-friendly. Can you make one of them magenta?

We tend to agree that the colour choice of the figures is not always perfect. While not purely rainbow, we would make different choices if we were to restart the whole analysis. Given that we would have to redo all the figures in order to make these small changes, we would hope that we get away with it this time around. A larger version of the figures will be available in the final version of the paper, which should help to increase readability. Also, we will make sure that colour-blind friendly plots are used exclusively in any future paper, presentation or other publication for that matter.

Reply to comments to referee 2

In this revision, Rimi and co-authors have improved their manuscript and properly accounted for most of my previous comments. I think the presentation of the results is now clearer, and the paper is almost ready for publication. I only have a few minor remarks for the authors to consider when preparing the final version of the manuscript.

We also thank the reviewer very much for their measured and constructive criticism, which we are addressing below line-by-line.

But as before, we first want to send our sincere apology to the referee(s) for the extremely long delay of the revision. The timing of the reception of the current round of reviewer comments (November 2019) coincided with the end of the PhD thesis of the main author. Ruksana not only had to defend her thesis, but leave the country, suddenly raise two kids on her own given the partner had to stay in the UK, and to get to grips with her new role as assistant professor in Bangladesh. And then, as we all know, came Covid, which complicated things even further. It has only been recently, that things have calmed down a bit, re-enabling Ruksana to start thinking about the paper revision.

As second author, I had to move institutions during Covid as well, along with a major shift in priorities. Therefore, on behalf of the whole author team, I hope you can accept our apology and would be willing to review this set of revisions. We thank everyone involved in advance!

Just as a note: the page/line numbering in the response document is not consistent with the revised paper (at least in my pdf). For instance, the term "might partially" (page 37 in the response document) appears on page 10, line 9 in my pdf, instead of page 9 line 40 as indicated by the authors.

We are not sure why the line numbering was not consistent. Again, our apologies for the inconvenience. Presumably, it is down to Word files acting differently at different Operating Systems upon opening them. I suppose, this could be avoided when switching to pdf format.

I appreciate the effort made by the authors to update the version of the APHRODITE dataset, which has a positive effect on the results, and to include an additional model intercomparison analysis. I just think that describing the results of the latter in the conclusions section is somewhat uncommon. You may consider moving this paragraph to the results section.

We agree. This paragraph should have been in the results section all along. It's incorporated there now.

Two reviewers commented on the use of the term "linear". The authors note that "By linear response, we meant steady and gradual increase…". However, this is not the original meaning of this term, which, in my opinion, leads to confusion (a linear response is more than just steady and gradual). I'd suggest to replace "linear" with "monotonic".

We have now used 'monotonic' where ever possible. Hope this makes our intentions clearer.

My comment on page 7, line 14 in the original manuscript referred to the text on Figs. 4/5 (not 2/3; "yet the relative change is smaller", page 9, line 11 in the revised version). Maybe it would improve the readability if the relative changes were specified more explicitly (add a few numbers in the main text).

We did add a few quantitative estimates which hopefully help to put the results into better perspective.

I'm still slightly confused by Fig. 2. Does panel a show NAT relative to ACT (as stated in the heading) or ACT relative to NAT (as stated in the caption)? In addition: Why do some of the patterns seem to differ between upper and lower row (e.g., panel a vs. e in the southern part)?

Valid point (see above). We hope that the revised version of the manuscript is more consistent now. We always show the difference between ACT and NAT, i.e. how has rainfall changed under ACT

relative to NAT conditions (with NAT being the baseline). But more crucially, you have indeed spotted a problem in Fig 2g. It was inconsistent with Fig 2c, which has of course been amended. All other figures may appear differently, but it is in fact due to the differing range of values. Perhaps not the ideal choice, but the idea was to highlight the details in the smaller region of Bangladesh, which would otherwise overload the figures for the whole South Asia domain in the upper row of Figs 2 and 3.