Responses to reviewer comments

Anonymous Referee #1

This is a high quality paper. The study is clean, thoroughly done, and I learned something. Bravo. I have a few minor comments.

Your final section is much heavier on the summary and pretty light on the conclusions. What else can you say about how this study fits more broadly into the scientific literature?

We thank the reviewer for the kind words.

We have added some additional text placing our study into a broader context. For example, we mention a possible implication of our findings on aerosols and ENSO:

"More broadly, our findings suggest a possible anthropogenic influence on this mode of climate variability, which may complicate efforts to separate variability arising from naturally from those forced by anthropogenic drivers."

We also conclude the section by adding some additional text to the final paragraph:

"Aerosol-precipitation interactions remain one of the most uncertain aspects of future climate change, especially on the regional scale (Rosenfeld et al., 2008; Michibata et al., 2016). To reduce the uncertainty of how future regional aerosol decreases will impact regional precipitation, a thorough analysis with multiple models, including several regions and aerosol types, is needed. Our results show robust precipitation responses to regional aerosol emissions changes do occur, indicating promise for future work. One caveat of our study is that in each of the models, aerosols do not exert a microphysical effect on deep convective clouds; however they can alter precipitation associated with deep convection through the aerosol direct effect."

We have also edited text in other places throughout the conclusions section, with the aim to place our results in broader context. We refer the reviewer to the revised manuscript (with changes highlighted) for the full suite of revisions to this section.

One important point about your analysis is that you did the step changes one at a time, which doesn't tell you about nonlinearity. I don't think you need to do any additional simulations, but it would be useful for you to comment (insofar as you're able to do so) about additivity of the perturbations, or lack thereof.

A subset of our simulations could tell us something about additivity across different aerosol types within a given model. For example, the US_ALL simulation could be compared to a sum of US_SO2, US_BC, and US_OC. We have done that for US aerosols in GFDL-CM3 in Figure 1 below and find that summing of the individual forcers results in a much larger precipitation response than the combined perturbation simulation.

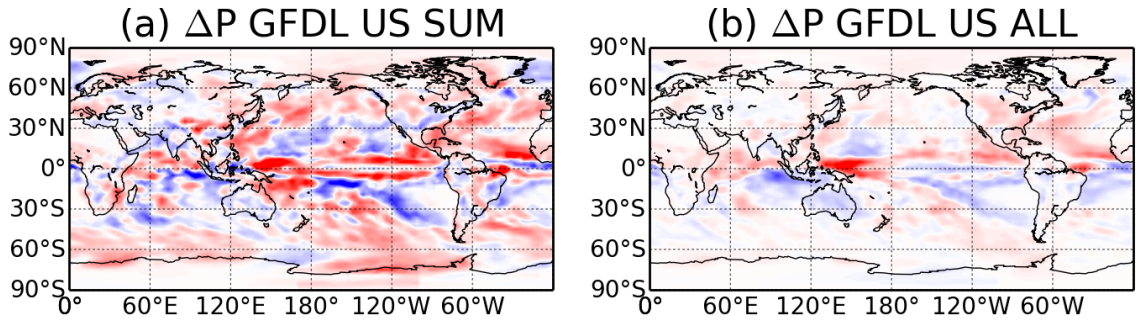

Figure 1: Sum of individual US aerosol forcing experiments compared to combined perturbation

In light of these results, we have added to following sentence to section 4.3 of the manuscript:
"In cases where regional aerosols were perturbed both individually and altogether (for example, US_ALL, US_SO2, US_OC, and US_BC), we find that the summation of the individual perturbations usually results in a larger precipitation response, both regionally and globally, compared to the combined perturbation (e.g. US_ALL), indicating nonlinearity among the individual responses (see Fig. 1 and Fig. S2)."

We have also added the following line of text to the conclusions:
"A possible avenue of further study may be combining different regions into a single perturbation simulation, resulting in a larger climate response and the ability to test for additivity or linearity among the simulations"

Our current suite of simulations does not provide information about additivity of the precipitation response across different regions. We leave this for future work.

The fact that GISS-E2-R didn't include aerosol indirect effects is interesting in the light of Malavelle et al. (2018) [https://www.nature.com/articles/nature22974]. The conclusion from that paper is that the first aerosol indirect effect is far more important than the second one. This of course doesn't mean that GISS is "right", and the other two models are "wrong", but a comment may be in order.

Correct, GISS-E2-R did not include the cloud lifetime effects or 'second' indirect effect. This is a good paper for us to cite. We have added the following sentence to the concluding discussion about the GISS results, which also partially address the reviewer's first comment about placing conclusions into a broader context:

"Using both climate models simulations and satellite observations of a major volcanic eruption, Malavelle et al. (2017) found that aerosol-induced changes in cloud liquid water path (the cloud lifetime effect) were undetectable, suggesting that the cloud lifetime effect may be less important than the cloud albedo effect for climate models."

The mechanism you invoke reminds me of a few papers:
https://www.nature.com/articles/nclimate1857
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2014RG000449
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015GL066903 All three of these support your mechanism (especially the first one) and might be useful to reference if appropriate.

Each of these has been cited in the revised manuscript.

One thing you don't mention about Sahelian rainfall is the character of the rain. Mean changes could indicate more extreme events. I don't know if this is relevant or if you can comment on it, given the scope of the study, but I thought I'd mention it.

This is something we are currently working on for a future publication, so we prefer to save comments about precipitation extremes for that manuscript.

Your discussion of Mediterranean precipitation changes is something of a counterpart to this paper: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2017GL076669 I don't think you need to do anything to address this comment – just something interesting that occurred to me.

We thank the reviewer for bringing this work to our attention.