Review Gao et al

The manuscript now includes more relevant figures in the main and supplementary material. However, the manuscript still has some major caveats related to the experimental setup and observational comparison after the comments from the reviewers and editor. I recommend another round of major revision based on the comments below. If the authors would not be able to give a scientific reasoning why a comparison of the equilibrium simulations with the transient (fast) observed precipitation response over Australia (see major comment 1) is valid, I would recommend that the article is rejected but could be considered for resubmission if the author's performed simulations to assess the fast climate response and find that these simulations justify their hypothesis.

Major comments

1. My main concern is still regarding the experimental setup since the changes over Australia in an equilibrium climate simulation (around 100 years) are compared to the fast climate response in observations (less than 10 years). A paper by Liu et al 2018 examined the fast and slow precipitation response over Australia shows that while the fast response shows a drying to Asian sulfate aerosols, the slow response shows a wettening (see Figure 1). Additionally, a recent paper by Hwang et al 2024 shows very different east-west Pacific and Indian Ocean SST patterns which lead to differences in the flow towards Australia (see Figure 3). Based on this previous literature, it does not seem justified to attribute the recent short-term drying (fast response) in Australia using equilibrium simulations which only show the slow responses.

   In response to the reviewer and editor the authors write the following:
   - „While transient simulations offer a more dynamic representation of temporal changes, the relatively short period from 2013 to the present may not provide enough time for the climate system to fully respond to aerosol changes."
   - "As the climate system responds more robustly over time, transient simulations will likely become a more appropriate tool."

   I would argue that both these statements are not correct: In the former, if the authors argue that the "real world climate" did not have enough time to fully respond to the aerosol changes then the same reasoning should be applied to the modelled climate. Thus, it seems incorrect to compare equilibrium climate simulations where the climate system had a lot of time to fully respond to the aerosol changes with the transient "real world climate". Similarly, the second comment is incorrect as over time the equilibrium simulations will become more appropriate (e.g. if it is a long-term Australian drying trend that should be attributed) while the short-term trend that the authors try to assess would be more accurately captured by a fast response.

   The authors now added a short paragraph in the discussion (L432-439). However, the authors will have to add a detailed discussion of their results (and choice to use an equilibrium simulations) in the light of the papers by Hwang et al 2024 and Liu et al 2018 as well as any other relevant papers. How can this attribution of the fast Australian drying based on equilibrium simulations be trusted if previous literature

shows large differences in the precipitation patterns over Australia and SST around Australia (and related mechanisms) in the fast and slow response? If the authors would be unable to give a scientific reasoning, I would recommend that the article is rejected but could be considered for resubmission if the author's performed simulations to assess the fast climate response and find that these simulations justify their hypothesis.

2. Thanks for providing the additional Figures S12 and S14 which help to examine the effect of different datasets and time periods. However, the authors still focus on the observational 2010-2019 and do not address how the anomalously wet year in 2010/11 might bias their assessment. This is particularly relevant since the authors theoretically want to compare the influence of Chinese aerosols on precipitation trends over Australia from 2013-2019.

   In order to assess the impact of including these three additional years, I recommend to create a spatial precipitation trend figure based on observational data showing the 2013-2019 trend in comparison to the 2010-2019 trend that the authors already show. If the 2013-2019 trend plot shows similar changes as the 2010-2019 plot, then this could help to make their statement of including the additional 3 years to reduce the influence of internal variability more robust.
   Additionally, the large impact of internal variability in the observational data should be discussed in the discussion further.

Liu, L., et al. (2018): "A PDRMIP multimodel study on the impacts of regional aerosol forcings on global and regional precipitation." *Journal of climate* 31.11, 4429-4447.

Hwang, Yen-Ting, et al. (2024): "Contribution of anthropogenic aerosols to persistent La Niña-like conditions in the early 21st century." *Proceedings of the National Academy of Sciences* 121.5