Review of Gao et al - Dry and warm conditions in Australia exacerbated by aerosol reduction in China

This research article investigates the impact of anthropogenic aerosol reductions in China on Australia's climate. The study found that the decline in Chine's aerosols since 2013 contributed to drier and warmer conditions in Australia by altering temperature and pressure gradients, which intensified the Southern Trade Winds and caused moisture divergence over Australia. The study also links these climate changes to an increase in wildfire risks in Australia. This research highlights the significant influence of distant aerosols on regional climate and offers insights for drought and wildfire risk mitigation.

The manuscript is interesting, well written and tackles an important topic of research (i.e., impact of Chinese aerosols on Australian climate). However, some technical details between the comparison of modelling results and observations need to be corrected and the selection of figures should be adjusted. I recommend acceptance of the manuscript if the major comment below can be addressed.

Major comments

1. One of my main comments is related to the comparison of observation/reanalysis data and simulated results: There seems to be some inconsistency between the timeperiods used. In the method section it is mentioned that the period 2013-2019 is used for the observation/reanalysis data as well as the simulated data. However, in the captions of the supplementary figures as well as in the description of these figures in the text (e.g. L274, 278) it is mentioned that the observations are for 2010-2019. Please clarify if the same timeperiod is used for observation/reanalysis data and modelling data and if that is not the case, the plots have to be redone for the correct timeperiod to ensure an accurate comparison. Besides, is this warming and drying trend over Australia still continuing or why did the authors look at the time period 20213-2019?
2. While it is great that the author's tried to reduce the figures in the main text to only 4 to explain the whole story, in particular the mechanistic analysis (Section 3.2) is difficult to follow for the reader with the limited number of figures. For instance, a combination of Figure S16 and S17 (i.e. the filled contours showing the SST pattern overlaid by the climatological wind field) would be a relevant figure to show. Additionally, Figure S21 is heavily referenced in the manuscript but the figure is only shown in the supplementary. Besides, maybe a small schematic of the described mechanism similarly as in Fahrenbach et al. 2024 would be helpful to guide the reader through the description.
3. On the topic of figures, it would be important to show a comparison of the simulated changes with the observed precipitation pattern (Figure S8) as well as the observed wind changes (Figure S18). This is particularly relevant since the authors are trying to do an "attribution" study and it has to be quantified that the observed and modelled changes agree. Additionally, the authors claim that the modelled and observed wind changes are similar (L319-321). While I do acknowledge that 3D wind changes are not the most reliable fields in reanalysis data, this is a bit of an overstatement. Figure S18b and c show very few significant changes making it difficult to understand the simulated flow and Figure S18d shows the largest significant trends in the winds east of Borneo and around

southern Australia, while the authors describe the weaker northern Trade winds and stronger southern Trade winds based on the simulated data. Maybe the authors could think about showing all wind vectors and colouring the significant ones in, so that the reader can at least see if the observations show the same trend even if they might not be significant based on this test?

4. My last comment regarding the figures is that the figure S15 should also be included in the main text. It seems biased to try to find a link / attribution but only show the plots for China which the authors have identified as the relevant one. Maybe a figure showing the annual precipitation trends for CHN, OTH, NA+EU and then a seasonal plot for the CHN plots would be best?
5. The authors discuss the influence of the (very strong) low bias in PM2.5 in CESM1 compared to the observations in L385-388, which is good and relevant. However, this should also be mentioned throughout the manuscript, for instance when the authors try to estimate very precise values for the influence of the Chinese aerosol reductions on precipitation and temperature (L270-271).

Minor comments

1. L29-31: The times mentioned in this sentence seem confusing since when first reading it seems that a trend from 2013 is caused by something happening around the 2010s. Maybe using "conditions since the 2010s" would help to settle this confusing sentence.
2. L68-70: Please change "increasing GHGs" to increasing GHG emissions.
3. L72-75: This sentence is very long and confusing, please split it up into two or shorten it
4. L73: "Earth's" instead of "earth's"
5. L104: "especially in northern Australia/especially in the North of Australia" instead of "especially the northern Australia"
6. L104: "affected by the Australian monsoon" instead of "affected by Australian monsoon"
6. L153-159: Is there a reason for the choice of the GPM dataset rather than for instance GPCP data?
7. L224: "Earth's surface" instead of "earth's surface"
8. L244: The setting of DF to 10 according to Sharples et al 2009 needs some more explanation. At least one sentence why Sharples et al choose this value and why it is also applicable here.
9. L277: Please change "evidence" to "indication".
10. L389: Please use "Earth System Model" or "fully-coupled climate model" instead of "aerosol-climate model" which would imply to me that this model is not fully coupled (which is the case according to the method section)
11. Figure S3: The colourbar of these two plots should be the same as the reader might be tricked into thinking that the magnitude changes between the observed and modelled data are similar.