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Interactive comment on “Aerosol-induced changes in summer rainfall and circulation in the Australasian region: a study using single-forcing climate simulations” by L. D. Rotstayn et al.

Anonymous Referee #1

Received and published: 12 April 2012

In this manuscript, the authors analyse the contributions of individual forcing agents to recent trends in Northwest Australian rainfall using the CSIRO Mk3.6 model. The key finding is that, in this model, the greenhouse-gas and aerosol effects on rainfall oppose each other: increasing greenhouse-gas concentrations decreases Northwest Australian rainfall, while increasing aerosol emissions increases rainfall. These findings are important, given that many climate-change scenarios include reductions in aerosols and rapid increases in greenhouse gases. Consequently, the authors find that CSIRO Mk3.6 model displays a considerable reduction in Northwest Australian rainfall under the moderate RCP4.5 scenario.

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The authors present their results in a clear, if long-winded manner. The methods and results are generally well-explained, with considerable attention paid to describing the Mk3.6 aerosol scheme and the prescribed aerosol emissions. My main concern with the manuscript is that the authors attempt to attribute the observed increasing trend in Northwest Australia rainfall to aerosols. While Mk3.6 increases rainfall in this region in response to aerosol forcing, the counter-balancing decrease in rainfall in Mk3.6 from greenhouse-gas forcing means that the all-forcing "HIST" ensemble does not reproduce the observed rainfall trend. The authors correctly include this caveat in their discussions, but they still conclude that aerosols are a potential explanation for the increasing rainfall trend in Northwest Australia. This conclusion is not justified, since the observed increasing rainfall trend is not reproduced in the all-forcings runs that provide the closest approximation to the real climate system. The key finding in the manuscript is that the authors' use of single-forcing simulations has allowed them to identify the opposing rainfall trends in Mk3.6 from aerosols and greenhouse gases, and so enabled an improved understanding of the Mk3.6 rainfall response to the RCP4.5 scenario. The authors need to revise their manuscript to remove or downplay the attribution issue, focusing instead on the key finding above.

The manuscript also requires some clarifications and corrections, particularly in terms of aerosol-induced changes in the Hadley circulation and the authors' proposed air-sea interaction mechanism for maintaining the anomalous circulation off the northwestern Australia. and the comparison of the modelled "aerosol-only" changes to reanalysis data. These are detailed below, along with several other minor revisions that I recommend prior to the article being accepted for publication.

Further, I recommend that the authors consider paring down their text. The authors do not begin to discuss their results until nearly halfway through the manuscript, after devoting considerable space to a description of the model, particularly the aerosol scheme and prescribed emissions. Given that the authors reference other papers (by the same lead author) that describe the aerosol scheme, I believe that they should

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remove some of the more technical details from the present manuscript. At its current length and technical depth, the manuscript appears rather daunting.

* Major revisions recommended

1. Page 5133, lines 10-13: The authors conclude that since two sets of paired differences between the HIST and NO_AA ensembles can capture the observed trend in Northwestern Australian rainfall, some combination of aerosol forcing and natural variability may be responsible for this trend. I struggle to reconcile this statement with the caveats that the authors quite rightly note later in the same paragraph, particularly the inability of the HIST ensemble to reproduce the observed increase in rainfall. If Mk3.6 cannot capture the observed trend in the all-forcings historical integrations – which is the closest approximation of the real climate system – then why should the rainfall trends from the individual-forcing simulations be believed? The real climate system includes both aerosol and greenhouse-gas forcings, of course, and yet contains a large increasing trend in Northwest Australian rainfall. These forcings counter each other in Mk3.6. If the Mk3.6 response to aerosols is correct – as implied by the authors' attribution of the observed trend to aerosols and natural variability – then the greenhouse-gas response must be incorrect (either in sign, magnitude, or both), which throws the RCP4.5 results into question.

As the authors note, it may be that Mk3.6 underestimates the decadal variability of Northwest Australian rainfall (page 5147, lines 17-19). If this were the case, it would open the possibility that the observed trend was purely natural variability, rather than including a forced component from aerosols. The authors claim that the inter-annual standard deviation of the Mk3.6 rainfall is reasonable, even high (pages 5132-5133), but inter-annual variability is not the same thing as decadal variability; models can correctly reproduce one but not the other. Have the authors examined the standard deviation of decadal-length (e.g., 11 year) running means from the pre-industrial control simulation, or made any other estimate of decadal variance? Without any evidence for the level of decadal variability in Mk3.6, it is not possible to evaluate which of the

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authors' hypotheses concerning the observed increase in rainfall – an aerosol-induced trend, natural variability or some combination of the two – is correct.

Essentially, I do not see how the authors can infer from their simulations that the observed trend in Northwestern Australian rainfall is aerosol-induced when their all-forcings run cannot reproduce the trend due to the counteracting effect of greenhouse gases. The authors should downplay this conclusion in their revised manuscript, making sure to add caveats where necessary in their discussion.

2. Pages 5142-5143: The authors' proposed positive air-sea feedback requires considerable clarification. Firstly, the authors argue that cyclonic wind anomalies off the coast of Northwest Australia increase the climatological wind speeds. They then link the increased wind speed to cool SST anomalies, which they say will reduce "the tendency of the convection and the associated cyclonic anomaly to move in any direction". I do not understand this "anchoring" effect. Tropical convection favours warm SSTs, not cool ones. If anything, the enhanced winds and cooler SSTs would tend to suppress convection, not enhance and "anchor" it. Similarly, I cannot see how the "converse of the above argument" would apply to an anticyclonic anomaly (lines 27-28). Also, the authors state that this anti-cyclonic anomaly would also "increase wind speed to the north, west and south", which makes no sense given their previous statement that cyclonic anomalies enhance the climatological wind speed. This mechanism needs to be clarified or removed from the manuscript.

Secondly, the authors then state that the maximum in Trel off the Northwest Australia coast in the HIST minus NO_AA ensemble is "at least in part due to the effect of the atmosphere on the ocean". This also does not make sense, since the HIST minus NO_AA ensemble shows increased near-surface wind speeds in the region of the local maximum in Trel. Increased near-surface wind speeds should reduce the local SST, not increase it. The authors' alternative hypothesis – that the warmer SSTs induce convection and cyclonic circulation – is much more likely and physically consistent.

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I recommend that the authors carefully revise the entire section on their proposed air-sea mechanism, making sure to correctly link anomalies in wind speed, SST and convection.

3. In their comparison of the trends in the HIST minus NO_AA ensemble to reanalysis data, the authors must include the caveat that this is not a "clean" comparison: the reanalysis data includes changes in both aerosols and greenhouse gases, while the HIST minus NO_AA ensemble represents the modelled response to only aerosol forcing. As in the previous comment above, the authors must be careful not to claim that their HIST minus NO_AA ensemble in any way represents the real climate system. Ideally, the authors would compare the reanalysis data to the all-forcings HIST ensemble, but the HIST ensemble does not represent the observed rainfall increase. The authors must be clear about the comparisons that they are making and the caveats that they entail.

* Minor changes recommended

1. The authors repeatedly state that an impact of anthropogenic aerosols in JJAS is to weaken the Hadley Cell by reducing ascent over the Asian landmass and reduce descent slightly south of the equator. The Mk3.6 model reproduces this effect. The authors then note that they find no such southward shift in DJFM, but instead an east-west shift (e.g., Abstract, lines 15-22). I am unsure why the authors would expect to find a southward shift in DJFM, though, when the ascending branch of the Hadley Cell lies south of the equator and the descending branch to the north. A southward shift in austral summer would imply a strengthening and expansion of the Hadley Cell, not a weakening. If the effect of anthropogenic aerosols is to weaken the Hadley Cell, then shouldn't the authors expect to find a northward shift of the Hadley Cell? There is some evidence of this in January (Fig. 19d), when aerosols induce anomalous ascent to the north of the equator and there is very slight anomalous descent in the core of the ascending branch. The rationale for aerosols causing a southward shift of the Hadley Cell in DJFM needs to be clarified in the text.

2. The authors provide two, potentially conflicting descriptions of the volcanic forcing in their simulations. On page 5117 (lines 12-13), they state that they prescribe "SO₂ from continuously erupting volcanoes (8.0 Tg S/yr)". Yet on page 5221, lines 22-25, they write that they prescribed "zonally averaged distributions of stratospheric sulfate based on Sato et al. (1993)", which is a time-varying dataset of observed volcanic eruptions. Which dataset the authors used, and for what quantities, needs to be clarified.

3. In section 3.2, the authors calculate the RFP for anthropogenic aerosols from "two 30-yr atmospheric runs with prescribed SSTs and sea ice". Later in the section, they compute the direct and first indirect aerosol forcings from "five-year runs with prescribed SSTs and sea ice". I can find no mention of the experiment design for these simulations, or indeed any other reference to them in the text. The authors need to include at least a brief description of these experiments. What SST and sea-ice forcing was used? Over what period were the integrations performed?

4. In section 3.3, the authors should standardise their descriptions of their ensembles by stating the differences with respect to the HIST ensemble (i.e., "as in HIST, but ..."). For example, the GHGAS ensemble could be described as "as in HIST, but with only changes in long-lived GHGs"; the NAT ensemble would be "as in HIST, but with only changes in volcanic and solar forcing". Alternatively, would it be possible to summarise the forcings applied to each of these ensembles in a table? This would allow the authors to reduce the text and provide a visual reference for the reader throughout the manuscript.

5. In Figure 3, the shading for the range of the NO_AA ensemble completely obscures the shading for the range of the GHGAS ensemble, as well as the HIST ensemble until 1960. It is therefore not possible to determine where the observations lie in relation to these ensembles; this is important, given that the authors claim that "observed global-mean T_s is mostly within the range of runs in the HIST ensemble" (pg. 5126, lines 15-16). The authors could represent the ensemble spreads as dashed lines, rather than shading, or present each ensemble in its own panel. Either way, the reader needs

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to be able to see all of the information on the figure.

6. In Figure 4, I recommend that the authors include a panel showing the differences between the modelled and observed rainfall. This would make it much easier for the reader to judge the biases in the model.

7. For Figures 4 and 8, are the AWAP data on the same grid as the Mk3.6 model? There seems to be considerably more spatial variability in the AWAP models than in those for the model. If the AWAP data are on a finer grid, then I recommend that the authors interpolate the data to the Mk3.6 grid so as not to unfairly disadvantage the model.

8. Page 5129, lines 6-7: I do not agree with the authors' statement that "to first order, the anomalous SST gradients off the coast of NWA are simulated by the model." Compared to observations, the Mk3.6 model produces the opposite relationship between SST anomalies along the northern and western Australian coasts and Nino 3.4 SST anomalies. Observations show mostly warm SST anomalies along the Australian coast in El Nino years – associated, as the authors state, with reduced monsoon winds and hence reduced latent cooling – while the model shows mostly cold SST anomalies in El Nino years. The authors need to revise this statement.

9. Page 5130, lines 1-3: The authors point to an "aerosol-induced" increase in Australian rainfall in JJAS, but Figure 9b shows this to be very small and not statistically significant. I count only five land gridpoints at which the increase in rainfall is significant; most of Australia shows no change in rainfall at all. As I am sure the authors are aware, JJAS is the dry season in northern Australia, so at most what the authors have shown is a very minor, statistically insignificant increase in dry-season precipitation. I recommend that the authors remove the discussion of these anomalies. The much broader increase in rainfall south of the equator in the Indian Ocean should be enough to justify their arguments for a southward shift in rainfall in boreal summer.

Further, since the authors discuss the reduction (increase) in ascent north (south) of

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the equator in JJAS at this point in the manuscript, I recommend that they move Figure 19 to this section. The large-scale changes in vertical motion in Figure 19 provide much better evidence for the Hadley Cell contraction than the small, insignificant changes in Australian precipitation.

10. Page 5132, lines 9-20: The authors use their ASIA experiment – in which aerosols are fixed at 1850 values except over Asia, where they vary as observed – to argue that changes in Asian aerosols alone are responsible for some, but not all, of the precipitation increase seen when globally varying aerosols are prescribed. This experiment adds little to the understanding of the aerosol-induced changes, however, because the results are inconclusive. As the authors themselves admit, they cannot determine to what extent non-Asian aerosols affect the Asian monsoon. Asian aerosols are clearly important for Australia, but then Asian aerosols represent a large fraction of global emissions. Given that the analysis of these experiments is limited and the authors conclusions about the impacts of Asian and non-Asian aerosols on the Asian monsoon are hypothetical at best, I recommend that the authors remove the discussion of this experiment from the manuscript. It is at best a distracting side-step from the main narrative of the results. Any further analysis of these results could be reported in a separate submission.

11. Page 5141, lines 14-17: The stronger circulation trends in March could also be due to an extension of the established, intense phase of the monsoon into that season, rather than the positive-feedback process that the authors describe. If the effect of aerosols in Mk3.6 is to extend the duration of the monsoon, then the trends will appear largest in March, since the authors are essentially taking the difference between an established monsoon and a retreating (or non-existent) one. The March monsoon in HIST minus NO_AA may be just as strong as that in February (i.e., not an amplification via a positive feedback), but the trends will appear to be greater because they are being calculated against a "baseline" of a much weaker (retreating) monsoon circulation.

* Technical corrections recommended

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1. Page 5128, lines 1-3: The authors should mention, where appropriate, that they have detrended the data prior to computing the regressions. The figure captions contain this information, but it should be included in the text as well.
2. Page 5128, line 23: The authors do not mean "vice versa" here; they mean anomalies of the opposite sign. "Vice versa" means to reverse the order of the items in the previous statement, not to reverse the sign.
3. Page 5141, line 6: "It's" should be "Its".
4. Page 5142, line 13: Do the vectors in panel (a) represent the mean climatological 10m winds for DJFM from the GHGAS ensemble? This needs to be clarified in the text, as at present it reads as though they are the mean vectors from HIST minus NO_AA.
5. Page 5145, line 22: "it's" should be "its".
6. Page 5149, line 13: Based on the authors' previous statements, I think this should read "a substantial minority projects an increase instead of a decrease".

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5107, 2012.

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