Response to Anonymous Referee #1

We appreciate the Referee's careful reading of the manuscript, and constructive and critical suggestions. We respond to each point below.

(General remarks)

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.

This is a fair comment. We have revised the manuscript to downplay the attribution issue as suggested. The revised version emphasises the opposing effects of changes in aerosols and greenhouse gases in our simulations, while also noting the issues that arise from an (unsuccessful) attempt to attribute the observed rainfall trend.

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.

We take the point about the length of the manuscript (and the other referee made a similar comment). However, we want to retain a reasonable description of the aerosol treatments, since this is the primary reference for a CMIP5 model, and the aerosol scheme is the main thing that has changed since the CMIP3 version of this model. Our solution is to move most of the model description into a Supplement, along with the subsection about the global aerosol burdens and optical depths, and another subsection about the breakdown of the aerosol forcing into direct and indirect components. This way, there is much less for the reader to wade through before getting to the results, but the model description is available for readers who have a specific interest. We also removed a few non-essential remarks from the text.

* 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.

Please see above for our general response to this issue – we agree that because the model cannot reproduce the observed rainfall trend in response to "all forcings", the specific conclusions about attribution should be removed, and we have done so. As already discussed in the manuscript, the “correct” response of Northwest Australian rainfall to greenhouse gas (or other) forcing is not known.

As the authors note, it may be that Mk3.6 understimates 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 authors' hypotheses concerning the obesrved 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.

Thank you for this suggestion. The 11-yr running mean of the NWA rainfall time series in the pre-industrial control run has a slightly larger standard deviation than the observed, detrended time series (0.38 versus 0.32 mm/day). If this measure is accurate, then it appears to create an interesting paradox: The model suggests that the observed trend cannot be explained by natural variability, and yet the response to all forcings also doesn't give the observed result (even in a single ensemble member). In the revised manuscript we suggest two possible answers to this question: errors in the model's response to each forcing, or the observed time series might not provide a robust estimate of the “true” decadal variability.

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.

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.

We added this discussion because it was hard not to notice that in the original Fig. 21(b), the aerosol-induced trend in near-surface wind speed showed a decreasing trend (which would tend to favour increasing SST) almost collocated with the centre of the cyclonic wind anomaly. This was surrounded by a closed loop of increasing wind speed trends (which would tend to favour decreasing SST). What we had in mind was that this pattern of wind speed trends would favour the SST maximum in the location shown in Fig. 21(b), and this *might* in turn favour this as the preferred location for the cyclonic anomaly. Unfortunately, we had a typo in the paragraph about the anti-cyclonic anomaly: we meant to write "**de**crease wind speed to the north, west and south" (not “increase”), and this presumably caused some confusion. Although we still feel that this is a potentially interesting effect, we acknowledge that the postulated feedback is somewhat speculative. In view of this, and the length of the paper, we removed this discussion, and simply added a citation to a recent paper, which makes the point that SST anomalies around northern Australia in summer are probably in part caused by the effect of the atmosphere on the ocean.

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.

We accept this point, and have revised the discussion to add these caveats.

* 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.

We were initially a bit puzzled that the reviewer was so puzzled by this, but on reflection we saw some opportunities to make it clearer, and it is also a good reminder that we must be precise in the words we choose.

Firstly, we added a couple of sentences in the Introduction to explain the general rationale for the idea that aerosol forcing causes a southward shift of the ITCZ or the Hadley circulation (namely, the inter-hemispheric asymmetry in aerosol forcing, or the resultant change in the latitude of the maximum sea-surface temperature).

Then we added a two-panel figure showing the aerosol-induced chance in meridional streamfunction for JJAS and DJFM. This zonally integrated view of the Hadley circulation shows that the change in JJAS resembles a weakening, whereas in DJFM it looks more like a southward shift. This is a useful reminder that either response is possible; in revising the manuscript we ensured that we were careful to accurately describe the simulated changes that we show.

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 straospheric 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.

In common with many GCMs, Mk3.6 has an interactive treatment of the *tropospheric* sulfur cycle, but it is unable to interactively simulate changes in *stratospheric* sulfate due to volcanic eruptions. As stated briefly in the text, the interactive treatment includes a time-invariant or background source of SO2 from continuously erupting volcanoes, but it does not attempt to simulate changes in stratospheric aerosol caused by explosive volcanic eruptions. The latter are treated by time-varying, prescribed distributions of stratospheric sulfate. We added some text to clarify that these are separate treatments.

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?

These simulations are part of the standard CMIP5 protocol, but it is fair to point out that we should have added more information about them, which we have done in the revised manuscript. (Essentially, SSTs and sea-ice are taken as a climatological average from the pre-industrial control run.)

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.

We standardised the descriptions as per the reviewer's first suggestion.

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

We decided that it was too ambitious to put four curves on one plot (including three with shading), so the best course was to remove the NO_AA curve. With only two colours of shading, the revised figure is much easier to read.

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.

Done (and some appropriate text was added to describe the new figure panel).

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.

Actually, we interpolated the Mk3.6 output to the much finer (0.25 degree) AWAP grid. If we had done the reverse, the plotting software would have left too much white space around the interior of the coastline (which it does when missing values exist, and the contouring process attempts to interpolate in space). Although we appreciate the suggestion, we are not worried that the model is being "unfairly" disadvantaged, since it is accepted that a limitation of GCMs is their relatively coarse resolution, so they are unable to resolve the effects of detailed topography and so on. We added a note to the figure caption to explain what we did.

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.

We removed this overly optimistic statement, and now conclude the paragraph with "The distortion of these features in Fig. 8b suggests that, while these competing processes are present in the model, the balance between them is not accurately captured."

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 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.

As suggested, we removed the text about Australian precipitation, and focused our argument on the increase in rainfall south of the equator in the Indian Ocean.

We considered moving Fig. 19, but we were reluctant to break up what we hope is a logical structure of subsections in this section (namely, rainfall changes, then the associated changes in circulation, followed by possible causes of the changes in circulation). In view of the length of the paper, we think the subsection headings are likely to be helpful for the reader. Instead, we added some text at this point to flag that the corresponding circulation changes are shown in a later subsection.

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.

Thank you for this recommendation, which is also a useful way to shorten the paper. We removed this discussion.

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-existant) 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.

We are not convinced by this argument. We think a stronger (wind) trend in March than in February is simply stronger, regardless of the baseline climatology that it is computed against. Further, the reviewer's argument could equally be applied to the early phase of the monsoon (say, December).

On reflection, the stronger wind trends in March are only a part of the reason that we suspect a positive feedback; the circular argument that relates the latent heat source and the cyclonic circulation anomaly in itself is suggestive of a positive feedback. We have modified the text, so that the explanation is hopefully clearer, while also acknowledging that we are not attempting to be definitive about the existence of this feedback.

* Technical corrections recommended

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.

We added this to the text.

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.

Fixed.

3. Page 5141, line 6: "It's" should be "Its".

Fixed.

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.

This figure has been removed from the manuscript.

5. Page 5145, line 22: "it's" should be "its".

Fixed.

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".

Agreed, and fixed.