Response to Anonymous Referee #2

We appreciate the Referee's insightful comments on the manuscript, and respond to each point below.

(General remarks, not addressed elsewhere)

Also, the model's aerosol/cloud-related schemes are described in almost painstaking detail, which makes the paper too long. In light of the primary focus on circulation changes, I do not think that this level of detail is necessary for interpreting the results.

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.

Specific comments:

Abstract L10-17 and Section 4.4: The argument that the anomalous cyclonic circulation is initiated by the aerosol-induced rainfall increase south of the equator is problematic. The DJFM rainfall and circulation changes in response to aerosols and greenhouse gases (GHG) (Figs. 9 and 16) closely mirror each other. This suggests that the simulated responses are driven mainly by model feedbacks, as opposed to the spatial structure of a particular forcing. In this sense, the true explanation cannot be aerosol-specific.

Thank you for this comment, which has given us food for thought. In response to the comments from the other referee, we revised the manuscript so that it now emphasises the way in which aerosols counter the effects of increasing greenhouse gases (GHGs). As we already stated in the submitted manuscript, the mechanism could involve changes in the Walker circulation or the local Hadley circulation, and either of these effects could be primarily atmospheric or involve oceanic changes.

In the revised manuscript, we recognise that we have insufficient information to strongly advocate one mechanism above another. We expanded the discussion of the mechanism by replicating the information in the previous Figs. 19 and 20 for GHGAS as well as HIST minus NO_AA, so that the changes induced by aerosols and GHGs can be better compared. We point out that regarding both the Walker circulation and the local Hadley circulation, the aerosol-induced effect is, to first order, similar but opposite to the GHG-induced effect. This is perhaps less obvious for the (dynamic) effect on the local Hadley circulation than for the (thermodynamic) effect on the Walker circulation, but it was already shown to some extent in the original Fig. 9. The similar but opposite effects on the local Hadley circulation can be explained in terms of the spatial structure of aerosol forcing (which is strong over Asia) and the relatively rapid transient warming of the Asian continent in response to increasing GHGs; thus, for different reasons, both effects are enhanced over Asia. We also added further discussion of possible mechanisms involving the Walker circulation, though we note that these are unconvincing as an explanation for the *observed* rainfall trend, because the Walker circulation is thought to have weakened. (It was this consideration that motivated us to look at possible mechanisms involving the local Hadley circulation, though in hindsight we over-corrected, and placed too much emphasis on this idea.)

In summary, we think the revised discussion represents a more balanced assessment of what can be said about the mechanism.

P5110 L6-8: Why is the hydrological sensitivity of aerosols larger than of GHG? See P5127 of Ming and Ramaswamy (2011) for an explanation.

We added an extra sentence to the Introduction, with references to Ming et al. (2010), Andrews et al. (2010) and Ming and Ramaswamy (2011).

P5119 and 5120: The term A is the sum of sulfate, sea salt and hydrophilic OC. This implies that the different aerosol species have the same CCN efficiency, which is not the case. In particular, OC is much less soluble than inorganic salts (sulfate and sea salt). Please justify this choice, or at least acknowledge the issue.

We added a sentence to acknowledge this simplifying assumption in our model. (Please note that this section is now in the Supplement.)

P5123 L11-21: Is there any upper limit on RH when calculating hygroscopic growth?

Yes, RH is capped at 99% when calculating hygroscopic growth. We think the most likely reason for the high bias in aerosol optical depth is the high bias in aerosol burdens (if this was the motivation for the question). We added a few extra words to the model description (now in the Supplement) to flag that the treatment of aerosol hygroscopic growth is described by Rotstayn et al. (2007).

P5124 L24-26 and Fig. 2d: Statistical testing is need for RFP.

Done (for both the global-mean value and the geographical plot). We appreciate this suggestion, because the noise in the RFP plot is less annoying when only statistically significant values are plotted.

P5126 L7-11: Given the emphasis put on aerosols and the nonlinearity issue, it is logical to run the aerosol-only case. If the computer time is limited, one can drop the no_aerosol case. Please explain.

Given the nonlinearity issue, and the fact that the emphasis of the paper is now focused on the "masking" of the GHG-induced effects by aerosols, the use of HIST minus NO_AA to diagnose the aerosol-induced effect (against a background of global warming) seems to be the best choice. We agree that it would be useful to compare the aerosol-only case in a later study.

P5134 L16-L17: In response to aerosols alone, land cools more than oceans, regardless of precipitation changes. Increased rainfall merely contributes to the cooling.

We doubt that this is correct for tropical Australia in the wet season. The main sources of aerosols there are dust (which mainly responds to rainfall changes), and aerosols from savannah burning, which are heavily weighted towards the dry season in their emissions. In

HIST minus NO_AA, the DJFM trend in aerosol optical depth is actually negative over tropical Australia for these reasons (though it is weakly positive in HIST). We slightly changed the text to say “relative cooling over tropical Australia is caused *primarily* by increasing rainfall”.

P5136 L7-L27: The assertion that these mechanisms discussed in the literature hold for this particular set of model simulations is not supported.

This is a fair point. Regarding the changes in oceanic upwelling and thermocline depth in the equatorial Indian Ocean, we have done plots of these, and can confirm that these mechanisms do occur in our simulations. We now state this result, though we decided not to lengthen the paper by adding another figure. Regarding the changes in the equatorial Pacific Ocean, the mechanisms discussed by diNezio et al (2009) are more subtle, in that they show that changes in upwelling are offset by changes in thermal stratification, and hence the main cause of enhanced equatorial warming is a change in horizontal heat transport. In view of this, we have just noted that “it is plausible that similar mechanisms occur in our runs, though further analysis would be needed to confirm this”.

P5318 L18: The right hand side of Eq. 2 can be thought of as a source of absolute vorticity by “stretching” an air column (see P16 of Ian James’ Introduction to Circulating Atmospheres). The equation is based on the conservation of potential vorticity, not absolute vorticity.

Thank you for pointing out this error. We have corrected the text.