Response to Anonymous Referee #2

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

General Comments:

1. *"The manuscript by Chrastansky and Rotstayn. uses a global climate model to simulate the effect of biomass burning aerosols on climate in equatorial Asia. The model results are, at time, compared to observations, when available. The authors analyzed four unique simulations: two with high fire emissions and two with low fire emissions, and two with climatological SSTs and two with observed. They quantified the forcing (direct and indirect) associated with burning aerosols as well as the response of several climate variables.*

   The overall quality of scholarship in the results and conclusions needs to be strengthened substantially prior to publication in ACP. Results need to be presented in a more quantitative manner in order for the reader to develop trust in the broad conclusions being made. Most of this research has been conducted in similar model studies, which are not explicitly cited in the introduction. Consideration of this manuscript for publication will require more extensive quantitative analysis to ensure that new knowledge has been added to the field. For example, it's crucial in the results section to add two tables explicitly listing the radiative forcing and climate responses for each simulation. Without this, it is difficult for the reader to gauge the climate significance of fire aerosols and to distinguish this paper from similar modeling studies (e.g. Tosca et al., 2010). Currently the authors describe qualitative conclusions but present very little data. Additionally, the authors rely heavily on Tosca et al., (2010) as motivation in the introduction, but do very little to explain how this study is significantly different or better at capturing the climate response to BB aerosols. It is well understood that modeling studies in this region fail to capture very small scale/local meteorological phenomena. This simulations in this study are performed at a low resolution and therefore probably do not do a great job capturing the small scale meteorology (similar to the studies presented). Additionally, the conclusions need to be significantly strengthened. At present, they are too short and do not adequately explain how the research conducted here will enhance prior published knowledge of climate-smoke interactions in the region."

We revised our manuscript substantially with a special focus on better specifying the research question and strengthening the results and conclusions. We noticed from the Referees' comments (Referee #1 and #2) that the aim of our study had not been described in sufficient clarity. We now point out clearly that we quantified the change in the direct and (for the first time) the indirect radiative forcing from Indonesian biomass burning aerosols due to the impact of ENSO-related rainfall and circulation anomalies. Our results suggest a need for realistic atmospheric conditions for radiative forcing estimates in ENSO-influenced regions, which is important for impact assessments. We did not aim, however, to better assess climate responses to aerosol pollution. Rather, the design of our experiments ensured that aerosol feedbacks on the climate were suppressed.

The abstract, introduction and conclusions have been re-written, following the referee's recommendations. We added all of the required tables, from which we believe the manuscript benefitted, and results are described in a more quantitative manner. In addition, we turned Section 3 "Further discussion" into a discussion of "Uncertainties and Limitations" associated with our study.

All in all, we believe that our manuscript is considerably improved by the changes we made. Please see the responses on the specific comments for more details.

Specific Comments:

2. *"The abstract does not explain what is "new" in this study versus other similar studies from the region. A sentence explaining why this suite of simulations better captures the climate response in the region might strengthen the manuscript's case."*

We've re-written the abstract entirely, making sure that the research objective is clearly understandable, and that results and gained knowledge are highlighted.

3. *"Pg. 5254 line 10-15: Why do you only use 1997 emissions in Indonesia, and 2000 emissions elsewhere for the "fire" simulation? Presumably most of the climate response to fire is a local one, but there may be evidence that fire emissions alter global circulation patterns or have other remote affects. If you want to truly capture the regional response to emissions specifically from 1997, it seems prudent to include realistic emissions for the entire globe, and not just single out Indonesia."*

Our aim was not to capture the climate **response** to the aerosol emissions, but rather to (1) calculate the direct and indirect radiative forcing anomalies due to the 1997 Indonesian biomass-burning event, and (2) analyse quantitatively the impact of ENSO –related meteorological anomalies on this radiative forcing. In order to isolate the radiative forcing from Indonesian biomass burning, we deliberately used realistic fire emissions only for the Indonesian region. For other emissions we intentionally used (for all runs) year-2000 emissions from CMIP5, which do not resolve ENSO variations. We agree that the experimental setup suggested by the referee is also plausible, though this would constitute a different study.

We added a few sentences to the Experimental design section to clarify this matter.

4. *"Pg. 5255, line 5: needs a comma between 'direct forcing' and 'the indirect effect'. "*

Done. Note that the paragraph has been moved to the appendix.

5. *"Pg. 5256, lines 10-15: Please include numbers with error bars of total emissions to give the reader an understanding of the forcing magnitude."*

Done. A table has been added summarizing biomass-burning emissions in the Indonesian region. We also added a brief discussion about uncertainties involved in the GFED emission estimates.

6. *"Pg. 5256, line 22-24: 'Figure 2 shows the difference of the July…' – What is the difference? Please provide numbers so that the reader can visualize the forcing magnitude."*

Done. Please see response to comment 5.

7. *"Results: A table summarizing the direct and indirect forcing and a table summarizing the climate variable responses would be immeasurably helpful to the reader."*

Done. We've added the required table to the conclusions section, comparing the direct and indirect forcing derived from the El Niño and La Niña simulation as well as the corresponding forcing anomaly (El Niño minus La Niña) from both experiments. We show results averaged for the Indonesian region, but also for an extended region that includes the central Indian Ocean in order to give the reader a feeling for a larger scale influence.
Note that our study did not analyse climate responses; see also our response to the general comments.

8. *"Pg. 5260, lines 22-25: Perhaps the reason AODs are lower in the model is because emissions estimates from GFED are too low. This should be addressed."*

Yes, GFED fire emission estimates might be too low, mainly because aerosols from gas-to-particle formation are not considered. Uncertainties such as in the burned area estimates, for instance, contribute to uncertainties in the fire emissions as well. Although we've mentioned this in the original version already, we now address those issues more carefully in section 3.3 (Aerosol optical depths).

9. *"Pg. 5265-66, lines (65)20-(66)8: Why is the CERES data not shown? (figure or table) An organized table would help the reader place the comparisons in context and evaluate the accuracy of the model. Cloud-aerosol interactions in most models are inherently biased, and a more comprehensive comparison with observations would build confidence that the results presented here were accurately capturing real-life responses."*

CERES data has already been shown in Figure 13, in which we compare the cloud effective radius anomaly from our model experiments with the satellite retrievals CERES and MODIS.

We added a table presenting average cloud effective radii during El Niño and La Niña conditions as well as the corresponding anomaly (El Niño minus La Niña) from all datasets. The table content is discussed accordingly in Section 3.6.2 (Cloud droplet sizes).

10. *“Pg. 5269, lines 1-9: This is worth mentioning in more detail. Why are BC effects on circulation not considered? Does the model not consider atmospheric absorption due to BC? Please explain this, either here or in the methods.”*

Although the model does treat BC atmospheric absorption, by design of the experiments aerosol effects on the circulation were not considered in the model runs. The text in the question was making the point that the AMIP experiment (using realistic SSTs for 1997 and 2000) implicitly includes some effect of aerosol forcing on the surface, albeit not on atmospheric absorption.

We re-worded the text passage in order to make this clearer to the reader. It reads as follows:

(…) As SSTs used for the AMIP experiment are derived from observations (Hurrell et al., 2008), possible cooling effects in SSTs due to aerosol forcing (Rajeev et al., 2008) should generally be resolved in the AMIP simulations. Owing to the experimental set-up, however, circulation changes due to atmospheric heating caused by BC aerosols were not considered. (…)

Note that we give a full description of the experimental setup in section 2.2 (Experimental design) and more detailed in the appendix.

11. *“Conclusion: At the conclusion of the manuscript still does not have a good grasp on what the actual radiative forcing from Indonesian BB aerosols is. A concise summary of the RF values would help place all the prior research in context and provide a quantitative comparison to other well-known RF values (i.e. from CO2, etc.)”*

Thank you for this comment. We’ve added a summarizing table to the conclusions, which describes the direct radiative forcing for El Niño and La Niña conditions, as well as the forcing anomaly (El Niño minus La Niña) for the direct and indirect effect. For the indirect effect, individual values from the El Niño and La Niña simulation are not physically meaningful and hence excluded from the table. This is also addressed in the manuscript. We show results averaged for the Indonesian region, but also for an extended region covering much of the Indian Ocean in order to give the reader a feeling for a larger scale influence.

12. *“Figures and Tables: Overall, the figures are really good. They present the research in easy-to-understand, informative ways. However, what this paper is lacking is comprehensive Tables. Tables would, as previously mentioned, quantitatively place the research in context.”*

Done. See previous comments.

13. *“Figure 6 (general comment): It seems that even during La Niña years, the AODs were much too low. This is probably due to very low background aerosol in the model. It may be worthwhile to scale your emissions upward so that emissions during low burning years produce AODs that match observations.”*

There are several reasons for AODs being underestimated in our model experiments, such as an overestimation of rainfall and the underestimation of aerosol emissions (see response to comment 8). In the latter case, “tuning” of fire emissions would be an option. This is, however, not trivial as many uncertainties are involved, and this would amount to a different study. Another possible approach to estimate emissions is via inverse modelling (Zhang et al, 2005), though again, this would be a different study.

References:

Taylor, K. E., Stouffer, R. J., and Meehl, 1575 G. A.: An Overview of CMIP5 and the Experiment Design, *Bull. Am. Meteorol. Soc.*, doi:10.1175/BAMS-D-11-00094.1, 2012.

Zhang, S., J. E. Penner, and O. Torres (2005), Inverse modeling of biomass burning emissions using Total Ozone Mapping Spectrometer aerosol index for 1997, *J. Geophys. Res.*, 110, D21306, doi:10.1029/2004JD005738.