Response to Anonymous Referee #1

We are pleased to submit a revised version of our manuscript entitled "The effect of ENSO-induced rainfall and circulation changes on the direct and indirect radiative forcing from Indonesian biomass-burning aerosols". We thank the Referee for giving careful consideration, and believe that the changes we made to our manuscript improved the publication substantially. Our point-by-point responses to the Referee's comments are as follows:

1. *"General remarks:*
The manuscript presents a global climate model study with the main purpose to compare model simulations results of Indonesian biomass-burning aerosols and their direct and indirect radiative forcing for two model set-ups: one driven with sea surface temperature (SST) for the specific years to be investigated (1997 and 2000) and one with climatological SST. The motivation seems rather artificial, as it is school-book knowledge that during El Nino or La Nina years, rainfall patterns and amount are considerably dependent on SST, in particular in Indonesia. In other words, without realistic SST, one should not expect to simulate realistic rain fall amounts and patterns and associated aerosol burden and aerosol forcing. Therefore the last sentence of the abstract presents a well known conclusion.

In the abstract it is stated that the effects on rainfall anomalies on the regional aerosol burdens and radiative forcing have not been investigated, but searching the literature, e.g. Heil et al. (2006) and Aldrian and Susanto (2003) already studied such issues, except the radiative forcing, which however is to a major extent dependent on the aerosol load. Therefore, it remains open which new knowledge is added by the model exercise presented in this manuscript.

As Indonesian vegetation fire aerosols and their radiative effects have been studied extensively, in particular during the El Nino event 1997/1998 a lot more of the available and cited references should be considered in the manuscript for comparisons with model results.

In addition, evaluations of model results are rather sparse, e.g. one-by-one comparison of rain-fall rates as modelled and observed by GPCP – a difference plot is missing or AOD's from different years than 1997 and 2000.

As Indonesia represents the most convective region of the Earth, where several models fail to reproduce realistic rainfall patterns, not only because of coarse grid resolution - this should be addressed in the manuscript as well.

In total, due to the weak motivation, the lacking consideration of already published material and the overall model related discussion with too few observation studies considered, I cannot recommend the manuscript for final publication in Atmospheric Chemistry and Physics."

Yes, there have been several studies dealing with aerosol impacts from Indonesian biomass burning (especially for the strong 1997 El Niño event), including various estimates for aerosol optical depths and direct radiative forcing. Some studies, however, considered meteorological conditions during El Niño for their analyses, while other studies were based on climatological conditions, solely looking at increased aerosol emissions during El Niño. Indirect aerosol forcing hasn't been analysed in this context.
We also agree that there have been studies investigating the influence of meteorological conditions on aerosol burdens. There has been no attempt, however, to quantify the changes in radiative forcing that emerge due to ENSO-induced rainfall and circulation anomalies, and to analyse its importance relative to the effect of changes in emissions. We found that the magnitude of the changes in radiative forcing is substantially stronger when the impact of ENSO on the meteorology is considered. For the indirect effect, the influence of ENSO-related changes in rainfall and circulation exceeds that of changes in emissions.

Hence, our study is innovative in two ways. Firstly, we show for the first time spatial and temporal estimates for the first indirect effect, both with and without the influence of ENSO. Secondly, we demonstrate quantitatively how the impact of ENSO-related circulation and rainfall changes (mediated from El Niño- and La Niña-like SST patterns) alters the magnitudes and spatial distributions of radiative forcing, both for the direct and the first indirect aerosol effect.

We acknowledge that our abstract was insufficiently clear, and apologize for a misleading sentence in the abstract which stated that "effects of these [ENSO-related] rainfall anomalies on regional aerosol burdens (…) have not been investigated". This possibly made Referee #1 believe that the main purpose of our study resembles the study by Heil et al. (2007). Referee #1, however, also stated that the impact on radiative forcing has not been investigated yet. As explained above, the latter is one of our main goals.

We substantially re-wrote the abstract, introduction and the conclusions, in order to clearly explain our research objectives and to highlight the knowledge that is gained. We also made sure that known facts are carefully discussed.

We are aware that GCMs can have problems in simulating rainfall, especially in such a fine spatially detailed region such as the Indonesian archipelago. This is why we performed the comparison with GPCP rainfall. Note, however, that our focus is not an air quality study but rather on investigating the larger scale radiative forcing from Indonesian BB aerosols. We followed the Referee's advice and added a brief discussion on uncertainties related to this matter. The required difference plot for modelled and GPCP rainfall has also been added (please see our response to comment 3 for further information).

2. *"page 5250 line 25 – page 5251 line 5: here only the first indirect effect is discussed. I suggest discussing the other indirect radiative forcing effects as well, to include references to studies of the indirect aerosol effects in Indonesia into the manuscript und to give estimates of the importance of the first, second and third indirect aerosol effect over Indonesia, which represents*

the most convective region of the Earth."

Done. We've included a discussion of direct and all indirect aerosol effects in the Introduction, and stated various estimates for the direct aerosol effect due to Indonesian biomass burning. Indirect aerosol effects in this context haven't been studied yet; however, in the course of our paper estimates for the first indirect aerosol effect are presented. As we explain in the revised Introduction, trying to estimate the second indirect effect in a convectively dominated region with a GCM would be highly speculative, so we have not attempted to do this. Regarding the third indirect effect, the term is not widely used, but we assume the Referee may be referring to a change in geometrical thickness (Christensen and Stephens, 2009). If so, this would be related to the second indirect effect, and attempting to quantify it would also be very speculative.

3. *"page 5252 line 2: the model resolution is relatively coarse in the horizontal and vertical direction. How reliable can it be expected that convection is modelled satisfactory? See (Neale and Slingo, J. Clim. 16, 834-848, 2003)"*

Yes, it is known that GCMs have problems in modelling rainfall in convective regions, which is why we compared the modelled rainfall with rainfall from the Global Precipitation Climatology project (GPCP). The problems are likely to be even more evident when considering the fine spatial detail over a region as the Indonesian archipelago, or the diurnal cycle. However, we don't think that going into a deeper level in comparing observed and modelled precipitation would add much to the conclusions of this paper, given that our purpose is to estimate the broader-scale radiative forcing.
As requested by the Referee, we added a difference plot between modelled and GPCP rainfall anomaly for better comparison. We also discuss now the issues/uncertainties addressed above.

4. *"page 5252 line 7-12, page 5253 line 19-28: except for mineral dust, which is out of the focus of this study, a bulk mass approach is used for the aerosols. More information about the assumed aerosol size distribution needs to be added to the manuscript in order to give the reader the possibility to understand how the direct and indirect radiative aerosol effects are determined."*

We agree that information about aerosol size distribution might be relevant for the reader. We've added to the corresponding paragraph a reference to Rotstayn et al. (2007) who discussed the aerosol size distributions in more detail.
In addition, we added a discussion about limitations of aerosol treatments in GCMs to the section 3.8 (Uncertainties and Limitations).

5. *"page 5252 line 13: please distinguish more carefully between gases and aerosols, e.g. SO2"*

Thanks for paying attention to that. We corrected applicable text passages accordingly.

6. *“page 5252 line 17: which temporal resolution is used? If monthly emissions are used, how do you take into account the variability in between and how important is this for the overall results?”*

The emissions are monthly, so the model does not resolve variability due to emission changes on shorter time scales. To evaluate the importance of this matter would require a separate sensitivity study, which is beyond the focus of our paper. We clarified this in the text and added this issue to the list of uncertainties.

7. *“page 5252 line 24/25: please include explicitly that prescribed oxidants are used, which ones (OH, O3, H2O2 or more?) and mention their temporal resolution as well as from which source these are taken from. Do you take the diurnal cycle of OH into account? Do you use 1997 oxidants concentration for the 1997 simulation? If not, you may be missing the changing oxidation capacity during this event (see Duncan et al., 2003). Please provide more information and discussion.”*

Done. The oxidants used in our model are the hydroxyl radical (OH^-), ozone (O_3), hydrogen peroxide (H_2O_2) and the nitrate radical (NO_3^-). They are prescribed using monthly-mean values representative for the modern-day climate. Oxidation of DMS and SO_2 by reaction with OH^- occurs only during the day, using a diurnally averaged concentration. Uncertainties involved with this approach are now discussed in section 2.1 (Model description) and section 3.8 (Uncertainties and limitations).

8. *“page 5253 line 4, line 16: Does the dry deposition scheme include gravitational settling throughout the whole atmosphere for carbonaceous aerosols and sulphate or is this ignored?”*

Gravitational settling of aerosols is only included for dust. We’ve added this piece of information to the corresponding paragraph.

9. *“page 5255: Rather technical information how the code is used. Please try to explain less code orientated.”*

Done. We replaced the code-oriented text passage in section 2.2 (Experimental design) with a simpler description of the experimental approach. As we believe a more detailed description of the experimental setup might be of interest for some readers, we moved the rather code-oriented part to the appendix.

10. *“page 5257/5258, section 3.2.1.: Model results should be better evaluated, at least a difference plot between GPCP and model results should be included and discussed. I do not agree with the sentence on page 5258 line 20, that the*

model captures the rainfall patters in the Indonesian region very well. Here again, it will be necessary to evaluate convective rainfall patterns separately. Please do not write SO4"

We carefully revised the paragraphs in which we compare modelled and observed rainfall patterns and discussed weaknesses and uncertainties accordingly, and added the requested difference plot between GPCP and modelled rainfall anomaly.

We don't see, however, how the comparison of convective and stratiform rainfall would contribute to our study. The wet scavenging treatment is essentially the same for convective and stratiform rainfall (Rotstayn and Lohmann, 2002, table1). This implies that the total rainfall rate is the most relevant quantity.
We replaced SO_4 at this place.

11. *"page 5259, section 3.2.2.: please compare with Heil et al. (2006)"*

Done. Thanks for pointing to the study of Heil et al. (2007). We added the following paragraph to section 3.2.2, in which we draw a comparison between wind fields from our experiments with wind fields corresponding to an ENSO-neutral (1996) and an El Niño (1997) year as presented in Heil at al. (2007):

"(..) A very similar conclusion was drawn by Heil et al. (2007) who compared wind fields during an ENSO-neutral year (1996) and the El Niño year 1997 in an air-quality study. Although our CLIM simulation exhibits a weaker southerly wind component, the general features of the 1996 wind fields (see their Figure 5) correspond to our climatological simulation. The wind patterns during the dry season in 1997 resemble the El Niño simulation of AMIP (not shown), and are also in agreement with the October mean wind field in 1997 shown in Parameswaran et al. (2004). Heil et al. (2007) stated that the more pronounced (south-)easterly winds associated with El Niño contributed to the increased spatial expansion of the smoke haze layer in 1997, while under ENSO-neutral conditions the aerosol layer is confined much more to the source regions. This is in agreement with our findings."

12. *"page 5262, line 5: Here only September and October averages are shown, before it was July to November. I suggest to use the same averaging period for the results presented in the manuscript."*

We agree that results should be shown for the same averaging time period throughout the manuscript. We decided to use the months July to November and replaced Figures as well as corresponding text passages accordingly.

13. *"page 5262: the interpretation is very model biased. There are numerous references to observations available which should be considered for comparison."*

Done. We included in section 3.4 (Anomalies in the direct aerosol forcing) comparisons with other studies that looked into the radiative impact of Indonesian biomass burning from the 1997 fires. This includes the observational study by Rajeev et al. (2008). The comparison suggests that the AMIP experiment produces much more realistic estimates for the direct radiative impact than CLIM. We did not delve into surface-based measurements of aerosol concentrations, as these would not yield much insight into the radiative forcing.

14. *"page 5267 section 3.7: I suggest to delete this section. I cannot recognise the importance of this section."*

We agree that the purpose of section 3.7 (Pattern correlation) was not stated clearly. However, we find this section is of importance as we here quantify the influence of rainfall on the direct and indirect forcing. Hence, we revised the section so that we now clearly indicate the purpose of this section and summarize the main findings of this section in a concluding paragraph. Note that we renamed section 3.7 into "Quantifying the influence of rainfall".

15. *"page 5268 section 3.8: The authors should mention in much more detail the uncertainties in their model approach."*

We thank the Referee for pointing out this issue. Yes, uncertainties had not been discussed in sufficient detail. We substantially revised section 3.8, in which we now discuss uncertainties and limitations involved in our study. In view of this, we also renamed section 3.8 as "Uncertainties and limitations".

16. *"page 5269 section 4: This section needs considerable improvements as the conclusions presented here do not go beyond the state of the art of knowledge."*

As indicated in the response to the general comments, we substantially revised the concluding section 4. We believe that the findings of our study as well as the gained knowledge are now clearly stated.

17. *"Figure 8 seems to be unnecessary as more ore less the same pattern is shown in Figure 9."*

Although the spatial pattern of the direct forcing anomaly at the surface (Figure 8) is similar to the direct forcing anomaly at the top of atmosphere (shown in Figure 9), there are features that differ. An example is the positive forcing anomaly that is only seen for the TOA forcing in the AMIP experiment. In addition, the spatial distribution of the forcing magnitudes is also of relevance. Hence, we decided to not delete Figure 8.

References:

Christensen, M., and Stephens G.L.: Evidence for a Third aerosol Indirect Effect from Ship Tracks Observed by Calipso, American Geophysical Union, Fall Meeting, 2009.

Heil, A., Langmann, B., Aldrian, E., Indonesian peat and vegetation fire emissions: Study on factors influencing large-scale smoke-haze pollution using a regional atmospheric model, Mitig. Adapt. Strat. Glob. Change, 12, 113–133, doi:10.1007/s11027-006-9045-6, 2007.

Rajeev, K., Parameswaran, K., Nair, S. K., and Meenu, S.: Observational evidence for the radiative impact of Indonesian smoke in modulating the sea surface temperature of the equatorial Indian Ocean, J. Geophys. Res., 113, D17201, doi:10.1029/2007jd009611, 2008.

Rotstayn, L. D. and Lohmann, U.: Simulation of the tropospheric sulfur cycle in a global model with a physically based cloud scheme, J. Geophys. Res., 107, D21, 4592, doi:10.1029/2002jd002128, 2002.

Rotstayn, L. D., Cai, W. J., Dix, M. R., Farquhar, G. D., Feng, Y., Ginoux, P., Herzog, M., Ito, A., Penner, J. E., Roderick, M. L., and Wang, M. H.: Have Australian rainfall and cloudiness increased due to the remote effects of Asian anthropogenic aerosols?, J. Geophys. Res., 112, D09202, doi:10.1029/2006jd007712, 2007.