General Comments:

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.

Specific Comments:

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.

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.

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

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

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.

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

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.

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.

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.

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.)

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.

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.