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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Assessment of co-benefits of black carbon emission reduction measures in Southeast Asia: Part 2 emission scenarios for 2030 and co-benefits on mitigation of air pollution and climate forcing" *by* Didin Agustian Permadi et al.

**Anonymous Referee #2**

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The study investigates the co-benefits of black carbon and PM2.5 emission reductions from climate and health related perspectives. Part 1 summaries the development of South East Asia (SEA) emission inventory, and validation of air quality model, while part 2 summaries the climate and health benefits from the reductions of BC and PM2.5 in 2030 for SEA. Overall, the paper is written within the scope of ACP and in a reasonable quality. Major changes, rerun and more in-depth explanation should be done before accepting for ACP.



General comments: 1) The experimental plan was well established. However, the model implementation (particularly in the air quality part) was not well delivered. It is necessary to redo the simulation to resolve that.

For example (described in part 1), CHIMERE only uses 8 vertical layers (up to 5.5 km). How the authors can consider long-range transport of air pollutants (e.g., PM2.5 and BC), which often happens at the free troposphere (from 2 km to 16 km – tropic). How the authors come up AOD only with 5.5 km column? Although I understand that the vertical profile of aerosol concentration goes down as increase of height. However, AOD is measured based on the entire vertical column. The influence of missing mid- and top- tropospheric (i.e., about 10 km) concentration could be huge, even with low aerosol concentrations. This could introduce a huge bias to the results. Moreover, major biomass burning influence occurs at the free troposphere, as plume-rise normally bought biomass burning BC and PM2.5 up to 6-10km. Without upper layer information, I am not sure how the authors can consider that. I agree with the authors that for the anthropogenic emissions, the influence of upper layer may not have much impact to the surface. That is only limited to the case with "no long-range transport", "no biomass burning", and "no tropospheric downwash.

2) Be honest, model performance is very bad for 30 km x 30 km resolution. For example, in part 1 Table 3, the RMSE for some of the sites is more than 10 degree Celsius. The highest could reach 20.5 degree Celsius. I am not sure whether I can trust these results. Please double check the statistic results.

3) In order to understand the part 2, more description of the regridding process, and what spatial interpolation methods (e.g., spatial surrogate) used in the study for generating the model emissions are important, as the countries in marine time continents are widely spread. How authors use those regional emissions from 50 km to 1 degree emission results (e.g., EDGAR or CGRED) to derive 30 km x 30 km resolution for the simulation.

4) The information from LMDZ/INCA was insufficient. I can't find enough information to understand the process. Any species mapping table, or cross-referencing table is available.

Specific comments 1) P1. L 21, more information on LMDZ/INCA. 2) P2. L21-23, please rewrite the sentence, I don't fully understand. 3) P3. L7, why only 1 year of WRF? Is 2007 a typical average year? Is it a wet or dry year? Normally, I will do three years simulation for any climate related study since it has to take into consideration of the interannual variability. Of course, this study relates more on air quality. I can understand using 1 year data. However, the authors may need to demonstrate 2007 is an average typical year, in terms of temperature, and precipitation. Sometimes, ENSO effect may have a huge impact, and may bias the results in the marine-time continents. 4) P3. L13, global LMDZ/INCAS has 19 layers, how the authors interpolate the results into 8 layers CHIMERE results. Is the 2030 case using 2030 boundary conditions from LMDZ/INCAS. The part 1 paper didn't mention anything related 2030 scenarios for LMDZ model. More information should be provided. 5) Moreover, for the Figure S1, the description should be 2030/2007, not 2030/2006. Also, which case is it for 2030? BAU, RED? Very unclear. Annual concentration or monthly average (Jan, Aug)? 6) P3. L31, Yes, same EFs for 2007 were used for 2030 may contribute a certain uncertainty to the projected emission. Will cause increase or decrease? Can the authors provide more explanation? 7) P3. L31, Is the projection align well with IPCC projection for 2030 for those local projections? 8) P6. L34, recent years, Thailand has started restrict local burning. How this may affect the projection? 9) P8. L25, as mentioned from the general comments, only 8 layers (up to 5.5km) may not cover the entire vertical profile. What may be the impact on this? Also, from the part 1 of the paper, the BC was well underestimated (Figure 8 in part 1 using AERONET data). How this underestimations of BC and PM2.5 would affect the results on the analysis of direct radiative forcing? 10) P9. L19. The authors mentioned the different between BY2007 and 2030 are listed 1.2, 2.4 and 4.3 ug/m3. These results seem falling into the uncertainty range of the results. As shown in Figure 2 to 5 in the part 1, the modeling errors of BC and PM2.5



are huge (1-5 times lower than observed). Can the authors elaborate more on that? How this underestimations may influence the results on health impact analysis.

In P5 L13, "CR data was obtained from Smith who indicates that every increase in PM2.5 by 1 ug/m3 is approximately associated with an 1.006% increase in the risk." How the model uncertainty affects the risk calculation?

11) Table 1. Header for PM1. And PM2. Were not showing properly. 12) Table 2. Co-benefits of emission reduction? What kind of co-benefits? I think the title should be "summary of emission reduction scenarios for the SEA domain". The value of "327 and 472 ug/m3 for hourly maximum seems to be very large. Please double check. 13) Figure 3. Very strange to see areas outside of Jakartar would have the same impacts as Jackartar. As shown in Figure 2, ] high concentrations of PM2.5 and BC are found in Jakartar, not other places in the island. However, the mortality cases in Figure 3 are all red for the island.

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