This study examined changes in PM2.5 and pollution days in China under greenhouse gas warming conditions using century-long CESM large ensemble simulations. They found that increases in PM2.5 concentration and pollution days during 2005–2100 with fixed anthropogenic aerosol emissions and changes in winds and light precipitation could explain it. The topic is interesting and suit for ACP journal. However, the results are not convincible, and manuscript needs major revision before it can be considered to be accepted in ACP.

My main concern is that the authors found decreases in PM2.5/SO4/SOA concentration (Figure S2) and moderate pollution days (Figure S8) over YRD and PRD, but they claimed an increase in pollution as a whole under GHG warming in the manuscript. It looks more like a decrease in pollution over eastern China to me. It is OK to present different trend pattern for different regions. Although the severe pollution days show increase over eastern China, the mean severe pollution days is less than 5/365 and the change is around 2/365 days. These values are too small. I would guess it is more like a model noise. The model strongly underestimates aerosol concentration in China. The author may consider using to a lower threshold. The author stated 'with an increase of approximately 68%'. It is unfair since the PM2.5 concentration only increases by 2%. The mechanism of the aerosol change is also unclear. Why SO4/SOA decreases while BC/POM increases in southeastern China?

Another main concern is that the changes in aerosol concentrations are too small between 2090–2099 and 2006–2015. The results are not convincing without significant test. The authors should add test (e.g., t-test for years and ensembles) to the results and figures to prove that the small values are not accidental. Changes in meteorological parameters also needs significant test. In addition, CESM model strongly underestimates aerosol concentration over China. Using absolute concentrations may cause problem, for example PM2.5>25/75. The authors should be caution about it.

Minor comments:

Line 27: The authors only used one model configuration with fixed anthropogenic emissions. It can definitely be used to rule out the influence of changing emissions. However, for a supplement of the story, I suggest comparing the role of GHG warming and aerosol emission change. I know it is hard to do another simulation, but the authors can easily scale aerosol concentration by emissions using RCP8.5 scenario and roughly predict aerosol concentration under future emissions.

Line 83: The highlight of this study, I guess, it the PM2.5 concentration under global warming. But the PM2.5 change is too small, and the authors showed pollution days instead (68% in abstract), which may not be appropriate and cannot be distinguished to precious studies. The authors may consider re-organize the findings and change the way of presentation.

Line 125-133: I don't understand what is fraction of attributable risk. The author can give an example and illustrate what is it used for.

Line 134: The author used stagnation day defined by Horton et al. (2012). It is definition suit to stagnation in China? Is there any previous study used it for stagnation in China? If not, the authors have to evaluate it using historical data of China.

Line 170: Spatial correlation over eastern China?

Line 174: I don't agree the bias is primarily due to missing species. The bias of aerosol concentration is more complicated in China, which has been reported in many previous studies (Yang et al., 2017a,b). The causes include uncertainties in aerosol emission amount, emission injection height, course model resolution, lack of nitrate, aerosol treatment in model (e.g., aging processes, chemistry, wet removal)…

Line 187: 2% is too small. The authors can focus on different regions and species.

Line 190: Add SOA in Figure 2.

Line 200: Do the future changes in meteorology (winds, precipitation) over China also exist in other models? At least add literatures.

Line 217: I don't think it is a good idea to emphasize and severe days and use 'robust response'. First, PM2.5 > 75 is suit for observations. The simulated PM2.5 is only 1/3 of the observation value. Second, I don't think the change will large than the standard deviation of severe days for different ensemble simulations and years. BTW, do you mean 'positive' response.

Line 226: 'PM2.5 loadings and their associated pollution days still present significant increases'. As mentioned above, I don't think this statement is correct.

Line 228-246: I don't understand what this section is used for. '28% of the pollution days are contributed by the climate change that was induced by GHG warming.' The authors fixed aerosol emission and all changes (100%) in the model should be due to GHG warming. Even changes in pollution days due to changes in meteorological conditions result from GHG warming.

Line 293-295: Again, 'substantially increase' is not correct.

Reference:

Yang, Y., Wang, H., Smith, S. J., Ma, P.-L., and Rasch, P. J.: Source attribution of black carbon and its direct radiative forcing in China, Atmos. Chem. Phys., 17, 4319-4336, https://doi.org/10.5194/acp-17-4319-2017, 2017.

Yang, Y., Wang, H., Smith, S. J., Easter, R., Ma, P.-L., Qian, Y., Yu, H., Li, C., and Rasch, P. J.: Global source attribution of sulfate concentration and direct and indirect radiative forcing, Atmos. Chem. Phys., 17, 8903-8922, https://doi.org/10.5194/acp-17-8903-2017, 2017.