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Comment on acp-2022-27

Anonymous Referee #1

Referee comment on "Fast climate responses to emission reductions in aerosol and ozone precursors in China during 2013–2017" by Jiyuan Gao et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-27-RC1, 2022

Overall comments:

I find the manuscript by Gao et al. interesting and fairly well-written. China's emissions of aerosol and ozone precursors declined after 2013 due to measures to improve air quality, and studies of the climate effects of these emission reductions are clearly of interest. While the topic has been investigated before, Gao et al. applies a state-of-the-art climate model (CESM2) for their quantifications. However, I have concerns regarding the methodological setup of the study and have several questions and comments that need to be addressed. My main concerns are:

- The study uses prescribed sea-surface temperature simulations, which is commonly used to quantify effective radiative forcing, but it is not ideal for characterizing climate responses, such as surface temperature change. While the authors state that they characterize fast climate responses, this is not clear enough in the manuscript – e.g., reading the current title and abstract gives the impression that the total climate response is studied. The fast temperature response is only part of the total response (which involves changes in sea-surface temperatures) but quantifying this requires coupled atmosphere-ocean simulations. Unless the authors want to carry out fully coupled simulations, my suggestion is to concentrate more on the ERF results and de-emphasize or remove the results showing surface temperature changes.
- The choice of using GEOS-Chem to calculate ozone makes the results less consistent and seems a bit odd given that CESM2 has a detailed tropospheric chemistry package (Emmons et al., 2020). Nevertheless, details about the GEOS-Chem simulations, and clarifications of how the GEOS-Chem results are used in CESM2, are needed, especially given the factor 10 difference in the ERF due to ozone changes in this study compared to an earlier study using GEOS-Chem.
- More work is needed to show that the CESM2 simulations realistically reproduce observed aerosols and the aerosol decline between 2013 and 2017. In particular, the natural aerosols that are modelled in CESM2 by default (dust and sea salt) should be included in the calculation of PM2.5.

Emmons, L. K., Schwantes, R. H., Orlando, J. J., Tyndall, G., Kinnison, D., Lamarque, J. F., Marsh, D., Mills, M. J., Tilmes, S., Bardeen, C., Buchholz, R. R., Conley, A., Gettelman, A., Garcia, R., Simpson, I., Blake, D. R., Meinardi, S., and Petron, G.: The Chemistry Mechanism in the Community Earth System Model Version 2 (CESM2), Journal of Advances in Modeling Earth Systems, 12, 4, 10.1029/2019ms001882, 2020.

Specific comments:

Title: Given that the full climate response is not investigated in these prescribed SST simulations, the first part of the title ("Climate impacts of") should be changed to "Radiative forcing due to" or similar. The second part of the title ("emission reductions") should also be changed by specifying that this only applies to aerosol and ozone precursors. The present title implies that the full climate response to all emissions (including long-lived greenhouse gases) is investigated, and this is not the case.

L21-25: I do not think "accelerated" is the right word here (enhanced or increased would be more correct). In any case, the surface temperature changes given here do not include the slow temperature response, only the fast responses over land, and are therefore most probably underestimated. It would therefore be better to change the focus from "climate responses" to "radiative forcing" and give the ERF numbers rather than the temperature changes. Similar modifications could be done elsewhere in the manuscript.

L27-28: Before making this conclusion, I think an investigation of the potential for emission reductions in this sector is needed – please see my comment further down (L227-230).

L32: There are two papers by Yang et al. (2017) in the reference list. Please specify which one.

L37-38: Better to say "emissions of major air pollutants and precursors"?

L47: Rather than citing the whole report, it is better to cite the actual chapter, in this case Forster et al. (2021) (Ch. 7). It is then much easier for the reader to find the relevant information.

L47-48: Effective radiative forcing is such a central term here and I think it should be

defined/explained briefly.

L49: The ERF_ari uncertainty range is -0.47 to 0.04 W m-2 (i.e., not a minus sign in front of 0.04) – see Table 7.8 in IPCC AR6.

L50-51: Should mention that tropospheric ozone is a greenhouse gas and contributes the most to the ozone ERF.

L56-65: Can you explain very briefly for the reader how the direct and indirect radiative forcing differ from ERF, so that it is easier to compare the numbers between the studies?

Section 2: There is no description or reference to the surface measurements of PM2.5 and O3. How were the measurements performed and are they representative of urban conditions? Or rural/background?

L78-79: Why are dust and sea salt not included in the calculation of PM2.5? These are standard output in CESM2 and I suppose they could make a substantial contribution to PM2.5 levels.

L80-84: The CESM2 gas chemistry package (Emmons et al., 2020) could easily have been included and would make the study much more consistent because of two-way interaction between gases and aerosols, and because the same meteorology would have been used for calculating both gases and aerosols. Why was GEOS-Chem used instead? Was it because of computational requirements? There is no reference to the GEOS-Chem model, and more information about these simulations is needed. For example, what aerosol compounds are included in these simulations? How long were the simulations? What resolution is used?

L87-89 / Figure 1: It would have been very useful to see a plot (e.g., in the supplementary) of the time evolution of emissions, for instance from 2000-2019, to see how 2013 and 2017 compare to the other years. Comparison with the newest version of the CEDS emission inventory (https://github.com/JGCRI/CEDS), which better accounts for emission reductions in China (in contrast to the CEDS version used in CMIP6), can also be considered.

L90: I assume the biogenic emissions are only included in the GEOS-Chem simulations?

L92: “present-day level” - please specify which year(s)

L93: Given the weak statistical significance in most of the results, are 20-year simulations long enough? For comparison, Zheng et al. (2020) ran the CESM1 model (fixed SST) for 60 years (analyzing the last 40 years) and got quite robust ERF numbers.

L115-119:

- "lack of nitrate and ammonium representation": I agree that the lack of ammonium nitrate leads to smaller PM2.5 values, but that does not necessarily mean that it contributes to the underestimation of the decrease in PM2.5. Are there any indications in the ammonia (NH3) emission data that there has been a decrease between 2013 and 2017? Ammonium nitrate formation is heavily dependent on the levels of sulphate, and the strong decrease in SO2 emissions implies that ammonium nitrate concentrations would actually increase.
- "absence of natural aerosols in the calculation of modeled PM2.5": I do not think that the lack of natural aerosols would influence the underestimated decrease in PM2.5? But including the natural aerosols would clearly give more realistic absolute concentration values (not changes).

L118-119: Since the model captures the relative differences better than the absolute differences, it could perhaps indicate that the actual PM2.5 concentrations are underestimated in the model. It would be useful to compare the model against observations of PM2.5 concentrations for 2013 and 2017 separately, for instance as a supplementary figure (similar to Fig. 2a). Underestimation of PM2.5 could also be partly caused by coarse model resolution, which I assume is 0.9x1.25 degrees (this should be stated in methods)? Are the observations primarily from urban areas? If so, the model is not expected to reproduce these observations, and one method that could be used to account for this is the so-called urban increment factor (see e.g., Aunan et al., 2018).

Aunan, K., Ma, Q., Lund, M. T., and Wang, S.: Population-weighted exposure to PM2.5 pollution in China: An integrated approach, Environment International, 120, 111-120, https://doi.org/10.1016/j.envint.2018.07.042, 2018.

L123-124: I am not totally convinced. In terms of simulating climate impacts of aerosol reductions, a useful comparison would be aerosol optical depth (AOD) from the model (should be standard output) against satellite observations (MODIS data available from https://giovanni.gsfc.nasa.gov/).

L147-149: How was the ERF separated into aerosol-radiation interactions and aerosol-cloud interactions? Did you apply double radiation calls?

L149-153: Again, I think the statistics would have been better with more years and/or ensemble members.

L151-154 / Figure 4b: There is a strong positive increase in ERF due to O3 changes over the Tibetan Plateau, despite decrease in near-surface O3 in this region (Fig. 2b) and decreasing O3 at height (Fig. 3b). Any reason why?

Figure 7 and Table S2: It would be good to see uncertainties for these numbers.

L215-218: The Table S3 caption states that Dang and Liao (2019) considered ERF_ari, while the text gives the number as the direct effect. ERF_ari includes also semi-direct effects in addition to the direct effect (see e.g., Fig. 7.3 in Boucher et al., 2013) and semi-direct effects are particularly important for BC. Are semi-direct effects included in the number from Dang and Liao (2019)?

Boucher, O., Randall, D., Artaxo, P., Bretherton, C., Feingold, G., Forster, P., Kerminen, V.-M., Kondo, Y., Liao, H., Lohmann, U., Rasch, P., Satheesh, S. K., Sherwood, S., Stevens, B., and Zhang, X. Y.: Clouds and Aerosols, in: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change, edited by: Stocker, T. F., Qin, D., Plattner, G.-K., Tignor, M., Allen, S. K., Boschung, J., Nauels, A., Xia, Y., Bex, V., and Midgley, P. M., Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 571–658, 2013.

L218-220: The factor 10 difference in ozone RF is puzzling given that the model calculating ozone changes (GEOS-Chem) is the same between the studies. I cannot understand that accounting for total column ozone change rather than tropospheric ozone change would make much of a difference (I expect changes in stratospheric ozone to be a minor contributor during this short period). I am also surprised that the uncertainty in ERF due to O3 is so large (0.81+/-0.92 W m-2 in Fig. 4b). How were the GEOS-Chem ozone data implemented in CESM2? The meteorology is different in GEOS-Chem and CESM2, so were the ozone fields implemented by cycling a single year GEOS-Chem run, as monthly mean climatologies, or in another way?

L220-223: Again, I do not think it makes sense to analyze surface temperature changes from these fixed SST simulations. The setup used in Zheng et al. (2020) is more logical - they used fixed SST simulations to calculate forcing and coupled atmosphere-ocean simulations to calculate surface temperature changes.

L227-230: The current contribution from reduced emissions in the residential sector is tiny (ERF_ari of -0.03 W m-2). Can you say something about the potential for further emission reductions, i.e. how much of the BC emissions from the residential sector was reduced and

how much remains?

L235-236: Can the authors speculate how changes in ammonium nitrate aerosols would have impacted climate?

L252-256: It is expected that model data should also be made available, in addition to the model code.

L446-447: Should make clear that model simulations are from CESM2 in a/c and GEOS-Chem in b/d.

L487: How is the aerosol column burden calculated, is it PM2.5 concentrations integrated over all vertical layers?

L487-491: Several of the bars would almost disappear if the scales were not logarithmic. I think it should be made clearer by adding "Note that scales are logarithmic" or similar.