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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The sulfur- and halogen-rich super eruption Los Chocoyos and its impacts on climate and environment" *by* Hans Brenna et al.

Anonymous Referee #2

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Overview: The paper by Brenna et al. simulates how the super eruption Los Chocoyos could affect the atmosphere and climate. Authors use a state-of-the-art Earth system model WACCM6 with an interactive chemistry and aerosol microphysics that allows to take into account all main feedbacks between the system components. Simulations are performed for a variety of scenarios, which is mainly used to analyze the sensitivity of the system to the amount of released sulfur and halogens. The paper also reviews other super eruption modelling studies and even performs a direct comparison of some parameters. This topic and the presented results are indeed interesting and significant for the ACP journal and the related scientific community. The paper is generally well written, the methods are solid, and the figures are of high quality. The main problematic

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point arises from the amount of experiments and subjects which authors tried to fit into the paper, so that some parts are discussed only superficially even though having several related figures. The way the paper is structured also feels not very convenient, because just in few sentences the reader has to jump from one figure to a panel of another figure several pages back, then to the supplements, and back to the initial figure. There are also several wrong references to figures and panels, which shows that navigation was complicated even for the authors. It is tricky, because the figures are combined by the type of analysis, while the text is structured by the type of effects. I understand though how difficult it is to combine such a diversified analysis together and therefore just in an advisory way suggest the authors to think again on the optimization of presented information.

There are also several other issues that have to be addressed before the publication for better clearness and readability:

1. Introduction: when you discuss halogen-rich eruptions estimations you don't mention the iodine, while some studies, like Cadoux et al. (2015) or observations for Kasatochi (Schönhardt et al., 2017 https://doi.org/10.5194/acp-17-4857-2017) reported this possibility. Given that iodine has a very large ozone depletion potential, maybe it is worth mentioning this aspect? Is there any estimate for Los Chocoyos?

2. 126-127: The major effect of the Siberian Traps volcanism was not only halogens and sulfur, but the release of massive amounts of CO2 and feedback with methane. It needs to mentioned, since you discuss volcanic effects on the Earth system in principle, and if you at all mention this case you should not avoid such an important part of its effects.

3. Section 2.1: You present the emission values you used and just refer to other papers for details, but it is not enough, given that these emission estimates are the triggers for your whole research. I suggest to add more information and maybe some discussion of related uncertainty. This would be a very valuable addition also for further studies.

4. 165-167: Gettelman et al. 2019 points that the QBO is generated but weakly due to an insufficient vertical resolution and that it can also impact teleconnections to high latitudes. It needs to be mentioned. Please also update the status of all references that were not complete (submitted, to be submitted etc).

5. 177-179: Not clear what you mean concerning spreading over 6 days. Is it a model result already? Why then is it mentioned in the "methods" section? Or was it some kind of precalculation to have a zonally spread field to avoid artificial mass loss?

5.1: Methods: How did you initiate the ensemble for the Ctr experiment? How many realizations did you perform for it? Was it a single run? If so, how then did you estimate the uncertainty spread for it in all figures? In Figure 6a you show several realizations for the CTR case, how did you obtain them?

6. Table 1: You have a long and complicated list of LCY_full ensemble members here with discrete names, but you never use them again. Consider simplifying this. I think the description of your runs in the "Methods" section is enough. Just intuitively, the reader expects that the information about different QBO and ENSO runs will be widely used later, which is not the case.

7. 184: You need to say something about why you use 1850 forcing. The climate of $\sim$80 kyrs ago was significantly different based on ice cores. You need to specify that your experiments did not intend to reproduce the paleoclimate, but are rather focused on the analysis of a hypothetical eruption under the common era conditions.

8. Section 2.5: Please give a wider description of the ONI index.

9. 209-210: You mean the decrease from 1/e level to 1/e2 and 1/e3, but it is not clear from the way it is phrased. Consider rephrasing.

9.1 225-228: This just indicates that gravitational settling is not important in this specific model. There are many studies, which showed the opposite (Pierce et al. 2010 doi:10.1029/2010GL043975, Weisenstein et al. 2015 doi:10.5194/acp-15-11835-2015,

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Delaygue et al. 2015 https://doi.org/10.3402/tellusb.v67.28582, etc). Even the sub-micron sizes sedimentation would counteract the tropical BDC upwelling thus modulating the global transport and the aerosol lifetime.

10. 266: There is no such information on Figure 3B. It stands for temperature, while you refer to UV.

11. 291: I assume you meant S3 instead of S4.

12. Even though the uncertainty spread of your perturbed experiment already crossed the spread of the CTR experiment, the anomaly is still clear and follows the temperature after year 10.

13. Figure 5c-f: Please name c-d as anomaly and e-f as relative anomaly (or change and relative change).

14. 323: 3D –> 3E

15. 324: S3 –> S4. Please check all figure references in the text.

16. Figure 6: Review the figure caption (also the case for other figures). It is very unclear given the amount of lines and extra objects. Why do you use these two different baselines for B and C? What is a reasoning for this?

16.1: 355: check parentheses and dots

16.2: 355-360: It is worth mentioning that Marshall et al. (2018) also showed that WACCM (not v6 though) calculates the longest aerosol lifetime among participating models. It goes in line with your 352-353 sentence and the fact that your model shows almost no difference in e-folding time between LCY_sulf and LCY_1%sulf.

17. 364: parentheses

18. 399-403: First you say that the multi-model uncertainty of the climate effects is smaller than the sulfur chemistry and aerosol microphysics and we see it on Figure

7 that the dependence is already close to linearity. But then you say that the multi-model effects in ozone response are even more robust, but don't present any number or figures. I suggest to rephrase this part, it is confusing.

19. 404: Consider replacing "released" by "published" or "reported", because the word "released" used many times even in this paper meaning "emitted". In the same sentence with "sulfur mass" it can be confusing.

20. 409: Higher what? I assume you missed a part like "emission estimate".

21. 438: To detect such a signal (<30 years) you better need sub-decadal resolution.

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