Dear VOLMIP/ACP Editors and Reviewers,

We have compiled a detailed point by point response going through all comments of the three reviewers. Our reply is highlighted in italics and light blue.

Best regards,

Hans Brenna and Co-authors

I) Reply to Reviewer Alan Robock:

1. This paper needs to be revised. Because of the way the simulations were carried out, and because of the lack of explanation or justification of the sulfur and halogen emissions used, the conclusions need to be framed as, "We simulated the eruption of Atitlán in recent pre-industrial times, with 523 MT sulfur (or SO2? – it is not clear), 1200 MT chlorine, and 2 Mt bromine emissions. The results may have been similar for Los Chocoyos, but because we did not do the simulation with its initial conditions, and because we do not know what its emissions were very precisely, we cannot say.
As expected, if there were large halogen emissions, the climate response was different that if the volcano only emitted sulfur into the stratosphere." If the authors make those changes and those below and address the 55 comments on the attached annotated manuscript, then it should be acceptable for publication."

Thanks for your constructive comments. We have taken all your general, detailed and supplementary comments in the revised manuscript into account as further answered below.

We added the missing explanations for the model simulations and the volcanic emission and uncertainties. We have taken up your above suggestion and added the following sentences to Section 5. Summary and conclusions, line 412:

"We simulated the eruption of Atitlán for 1850 pre-industrial conditions with 523 Mt sulfur, 1200 Mt chlorine, and 2 Mt bromine emissions. The model results may have been similar for Los Chocoyos 80.800 years ago, as we did not set up the simulations with observed initial conditions and there are uncertainties in volcanic emissions. As expected, if there are large halogen emissions, the climate response is different that if the volcano only emits sulfur into the stratosphere. Overall, we evaluate our model results to show a low climate and environment response given the likely low estimates for our petrological derived volcanic emissions."

It is 523 Mt S (and 1046 Mt for SO2). We have clarified this in the revised manuscript.

2. The introduction is overly long. There is no need to review every paper ever written on the impacts of supereruptions. Only include the ones you will refer to later.

We have taken up your suggestion and shortened the introduction with regards to the impacts of super eruptions.

3. On the other hand, section 2.1 is much too short. The authors give no details about how they determined the emissions from the eruption, nor the errors associated with that determination. And why did they not use ice core data?

i) We have added missing details of the used volcanic emissions and uncertainties in Section 2.1. We have changed it to the following:

"2.1 Los Chocoyos erupted volatile estimates

Using the recently published total erupted mass estimate for the LCY eruption (Kutterolf et al., 2016) and the previously published petrologic estimates of volatile concentrations for sulfur, chlorine and bromine (Metzner et al., 2014; Kutterolf et al., 2015) we calculate a new mass of erupted volatiles for LCY as a starting point for defining the stratospheric injections in our model simulations. The erupted volatile masses as calculated using these estimates (+/- uncertainties) are 523±94 Mt sulfur, 1200±156 Mt chlorine and 2±0.46 Mt bromine.

The determination of volatile injection into the stratosphere during the Los Chocoyos eruption is based on a two-step approach: The first step is the determination of erupted magma mass. Los Chocoyos fall deposits are well exposed on land and within sediment and lacustrine cores on the Pacific seafloor as well as Lake Péten Itzá to create isopach (thickness) maps (Kutterolf et al. 2008a, 2016; Cisneros et al. in review). These maps serve as a basis to determine erupted total tephra volume by fitting straight lines to data on plots of ln [isopach thickness] versus square root [isopach area] following Pyle (1989) and Fierstein and Nathenson (1992) and integrating to infinity as described in Kutterolf et al. (2016, 2008b, 2007). Additionally, outcrops identified in the field, in satellite images, and Google Earth, have been used to document regional thickness variations and finally to determine the volume of the flow deposits by integrating the results of different calculation methods (Cisneros et al. in review). We then converted tephra volume to magma mass following the procedure of Kutterolf et al. (2008b, 2016) by using variable tephra densities from proximal to distal deposits.

The second step is the measurement of volatile contents in both melt inclusion and matrix glasses (see Metzner et al. 2014, Kutterolf et al., 2015). Applying the petrological method (Devine et al., 1984), matrix glass represents the degassed melt after eruption and melt inclusion glass represent the volatile content prior the eruption. The concentration difference between melt-inclusion and matrix glasses yields the volatile fraction degassed during an eruption, and multiplication with erupted magma mass gives the mass of emitted volatiles (e.g. Kutterolf et al. 2015).

Both procedure steps are taken into account in the maximum combined uncertainty for the volatile budget of each volatile, which is ±13% for chlorine, ±18 % for sulfur, and ±23 % (see also Brenna et al. 2019).

Finally, the petrological method might underestimate the volcanic emission due to pre-eruptive, magma fluid partitioning by a factor of 10 for sulfur (Self and King, 1996) and a factor of 2 or more for halogens (Kutterolf et al 2015) as discussed earlier (Metzner et al 2014, Krüger et al 2015; Brenna et al 2019)."

ii) The Los Chocoyos eruption was not detected in ice core records until now as we have written in lines 406-410. High resolution ice core records with high temporal precision during this time window and, important, analysable glass shards to verify a correlation to LCY are needed for this kind of analysis which were unfortunately not available to us yet. Available (GISP2) ice core records currently have a temporal resolution of 40 years and a potential age error of several thousand years at the time period 80,000 years ago (Zielinski et al 1996). Thus, our manuscript shall initiate future work in the ice core community. We have changed the last sentence of these lines to better clarify our point here:

"This model study together with the new dating of the LCY eruption to 80.8±6.7 kyrs and a higher mass loading (Cisneros et al., in review) will hopefully stimulate upcoming studies finding corresponding paleo proxies in ice cores, climate, and archaeological archives with high temporal resolution and precision."

4. Section 2.2 is also too short. They say they use CLM5, but with what settings? 1850 vegetation? Dynamic vegetation? Crop model turned off?

The land surface model in CESM2(WACCM6) is the community land model Version 5 (CLM5) set up under 1850 conditions with dynamic vegetation, interactive biogeochemistry (CN, methane) and prognostic crops. We have added these details to section 2.2 line 171.

5. The metric Mv is used for volcanic eruptions, without ever explaining what it is and why it is relevant for the impact of volcanic eruptions on climate. If it is a geological measure of explosivity, then it is not appropriate. You might want to look at the discussion on pp. 3-4 of Newhall et al. (2018). Is it the same as the M discussed there?

Newhall, Christopher, Stephen Self, and Alan Robock, 2018: Anticipating future Volcanic Explosivity Index (VEI) 7 eruptions and their chilling impacts. Geosphere, 14, No. 2, 1-32, doi:10.1130/GES01513.1.

It is the same metric M as in Newhall et al (2018) calculated after Pyle (2013; Encyclopedia of volcanoes). We have removed the subscript "v" and added the following sentence in the ms to:

"The Los Chocoyos (LCY) super-eruption (Kutterolf et al., 2016) of magnitude M=8 (Pyle, 2013), dated to 80.8±6.7 kyrs before present (Cisneros et al. in review), has been known to be one of the largest volcanic eruptions of the past 100,000 years (Drexler et al., 1980)."

6. How could 1850 initial conditions be representative of the climate and the climate forcings at the time of the LCY eruption?

1850 pre-industrial conditions served as the best feasible model set up for our Los Chocoyos Atitlan eruption model experiment. Ice core records reveal CO2 levels between 220 and 240 ppm versus 285 ppm, no rapid climate transitions and similar orbital forcing 80.800 years ago compared to 1850 Pre Industrial (PI) conditions. Thus, we expect our model set up and experiment to be a good estimate for the paleo climate response of the Los Chocoyos Atitlan super-eruption under 1850 PI conditions.

We have clarified this in the revised manuscript. We have taken up your comment and added the following sentences to 2.3 Model experiments and Section 5. Summary and conclusions:

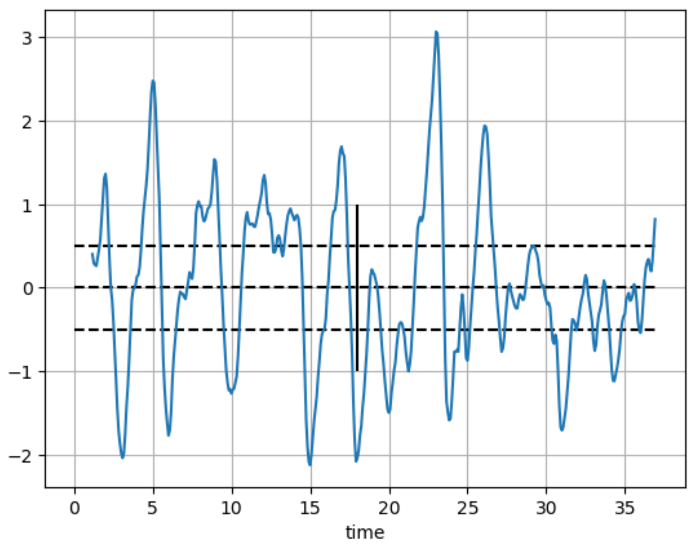
Line 179: "We run the Los Chocoyos Atitlan super-eruption model experiments under 1850 PI conditions which was the best feasible model set up we could achieve. "

Line 412: "We simulated the eruption of Atitlán for 1850 pre-industrial conditions with 523 Mt sulfur, 1200 Mt chlorine, and 2 Mt bromine emissions. The model results may have been similar for Los Chocoyos 80.800 years ago, as we did not set up the simulations with observed initial conditions and there are uncertainties in volcanic emissions. As expected, if there are large halogen emissions, the climate response is different that if the volcano only emits sulfur into the stratosphere. Overall, we evaluate our model results to show a low climate and environment response given the low estimates for our petrological derived volcanic emissions."

7. The authors use ONI without ever defining it or giving a reference. What is it? Is it the same as the Niño3.4 temperature anomaly?

The ONI index is used operationally by NOAA to calculate the ENSO state (https://origin.cpc.ncep.noaa.gov/products/analysis_monitoring/ensostuff/ONI_v5.php). The index uses the Nino3.4 region SSTs. To calculate the index, the average SST anomalies in the Nino3.4 region is filtered using a 3-month running mean. If this index is above or below 0.5 C for at least 5 consecutive months, we have an El Nino or La Nina respectively. We have added these details to the ms in section 2.5.

Below you find the ONI time series for our CTR simulation.



8. The term “pentadal” is used in the text and figures without every defining it. What does it mean? What pentad?

Pentade is a five year period, analogous to a decade. We have added this definition at the first occurrence of the term and in the figure captions as “five year (pentadal)”.

9. There are several unacceptable references from papers to be submitted.

Gettelman et al. (2019) paper is now published. Danabasoglu et al. and Cisneros et al. are now submitted and under review. We have added these details to the revised ms and the reference list. We can provide the papers in review along with our revised manuscript.

Gettelman, A., Mills, M. J., Kinnison, D. E., Garcia, R. R., Smith, A.K., Marsh, D.R., Tilmes, S., Vitt, F., Bardeen, C. G., McInerny, J. Liu, H.-L., Solomon, S. C., Polvani, L. M., Emmons, L. K., Lamarque, J.-F., Richter, J. H., Glanville, A. S., Bacmeister, J. T., Phillips, A. S., Neale, R. B., Simpson, I. R., DuVivier, A. K., Hodzic, A., Randel, W. J.: The Whole Atmosphere Community Climate Model Version 6 (WACCM6), J Geophys Res-Atmos, 121(22), 10,328, doi:10.1029/2019JD030943, 2019.

Cisneros de León, A., Schindlbeck-Belo, J. C., Kutterolf, S., Danišík, M., Schmitt, A., Freundt, A. and Pérez, W.: A history of violence: magma incubation, timing, and tephra distribution of the Los Chocoyos super-eruption (Atitlán Caldera, Guatemala), in review at Journal of Quaternary Science.

Danabasoglu, G., Lamarque, J.-F., Bacmeister, J., Bailey, D. A., DuVivier, A. K., Edwards, J., Emmons, L., Fasullo, J., Garcia, R., Gettelman, A., Hannay, C., Holland, M. M., Large, W. G., Lawrence, D., Lenaerts, J., Lindsay, K., Lipscomb, W., Lofverstrom, M., Mills, M. J., Neale, R., Oleson, K., Otto-Bleisner, B., Phillips, A., Sacks, W., Tilmes, S., Vertenstein, M., Bertini, A., Deser, C., Fox-Kemper, B., Kay, J. E., Kushner, P., Long, M. C., Mickelson, S., Moore, J. K., Nienhouse, E., Polvani, L., Rasch, P. J., Strand, W. G.: The Community Earth System Model version 2 (CESM2), in review at JAMES.

10. In general, the supplemental figures are missing a lot of information in their captions.

They should be understood without having to search the main paper for definitions. All acronyms and terms need to be defined.

All supplementary figure captions are revised and checked for clarity. We added a new Figure S3 according to your Supplementary comment 51.

Revised Supplementary figure captions are:

Figure S1: Zonal mean column ozone and aerosol optical depth (AOD) evolution after the Los Chocoyos (LCY) eruption. (a) Column ozone climatology for the CTR. (b) LCY_full ensemble mean column ozone anomaly. (c) LCY_sulf ensemble mean column ozone anomaly. (d)

LCY_full ensemble mean AOD anomaly. (e) LCY_sulf ensemble mean AOD anomaly. See Table 1 for information about the eruption scenarios and model simulations

Figure S2: Global mean post-eruption five year (pentadal) mean anomaly profiles of (a) ozone concentration, (b) aerosol surface area density (SAD), (c) temperature, (d) short wave heating rate and (d) long wave heating rate of the LCY eruption scenarios. Shading represents the two standard deviation range.

Figure S3: Maps of post-eruption five year (pentadal) mean surface temperature anomaly and climatology (a), precipitation anomaly and climatology (b), precipitation change (c) and NPP change (d) for LCY_sulf. White areas on the NPP maps indicate invalid values.

Figure S4: Hemispheric mean sea ice changes and northward ocean heat transport anomalies after the LCY eruption. (a) Northern Hemisphere (NH), (b) Southern Hemisphere (SH). Northward ocean heat transport at (c) 60°N, and (d) 60°S. To allow running means to extend to zero, the pre-eruption year from the CTR was added to each ensemble member. Shading represents the two standard deviation range.

Figure S5: Global maps of net primary productivity (NPP) climatology for CTR (a), post-eruption five year (pentadal) anomalies for LCY_full (b) and LCY_sulf (c) ensembles. (d) shows the difference between (b) and (c). White areas on the map indicate invalid values.

11. Fig. S1(a) is missing the changes from December to January. Plot January on both sides so it gives the entire seasonal cycle and indicate months with their names, not numbers.

Thanks for the suggestion. The figure has been revised, see Fig. R4 at the end.

12. Fig. S2: What does pentadal mean? It has to be explained.

See our answer above. We have revised the figure caption according to your general comment 10.

13. Fig. S2: Add a vertical axis in height on the right side of the figures.

We have added a height axis to the figure, see Fig. R5 at the end.

14. Fig. S2: How are the anomalies calculated? And why are there no error bars?

The global mean anomalies are calculated using the global mean climatology and the global mean ensemble means over the first 5 years after the eruption. We have added the two sigma range to the figure using the individual ensemble member to get the spread. The revised figure R5 is included at the end of this document.

15. Fig S3: How can 12-month running means start at 0?

The year of the control run before the branch date was added to the time series to allow the 12 month running means to extend to zero. We have added this information to the figure caption, see our reply above.

16. Fig. S4: You have an entire page. Why not fill it with the figures rather than use tiny ones at the top of the page. And change “indicates” to “indicate.” Also, it needs a fourth panel with the difference between panels (b) and (c). They look identical as plotted. Isn’t the difference the important information?

We have changed the orientation of the figure and added a fourth panel showing differences as added to the revised figure caption (see above). The revised figure is included at the end of this document (Figure R7).

Reply to supplementary comments from Alan Robock

1. Line 11; marked text: “vey large amounts”; comment: “How much? What error bars”.

We added the amounts of uncertainties:

“Recent petrologic data show that the eruption released very large amounts of climate relevant sulfur and ozone destroying chlorine and bromine gases (523±94 Mt sulfur, 1200±156 Mt chlorine and 2±0.46 Mt bromine).”

2. Line 12; marked text: “recently released”; comment: “Delete. This is irrelevant”.

Agreed, deleted.

3. Line 14; marked text: “month”; comment: “month of”.

Thanks, corrected.

4. Line 15; marked text: “enhanced modeled sulfate burden”; comment: “???”.

We have clarified this clause to read: “Our simulations show that elevated sulfate burden and aerosol optical depth (AOD) persists for five years in the model”

5. Line 18; marked text: “years ”; comment: “years, ”.

Thanks, corrected.

6. Line 19; marked text: “(NH)”; comment: “delete – acronym not used again”.

Thanks, deleted.

7. Line 23; marked text: “El-Niño”; comment: “El Niño”.

Thanks, corrected.

8. Line 30; marked text: “ESM”; comment: “define acronym”.

We have added the acronym definition to the revised manuscript.

9. Line 33; marked text: “very large”; comment: “very large, ”.

Thanks, corrected.

10. Line 37; marked text: “LCY”; comment: “Why not just LC?”.

This short name was used to be consistent with the previous studies on the Central America Volcanic Arc eruptions, e.g. Kutterolf et al, (2013, 2015, 2016), Metzner et al, (2014) and Brenna et al, (2019).

11. Line 37; marked text: “(Magnitude Mv=8 ”; comment: “What is this scale? Give an explanation and a reference. If it has no relevance to climate impacts, why give it?”.

See our answer to your main comment 5 above.

12. Line 38; marked text: “(Cisneros et al. to be submitted)”; comment: “Not an acceptable reference”.

See our answer to your main comment 9 above.

13. Line 38; marked text: “already 30 years ago”; comment: “??? this paper was 40 years ago”.

Yes agree. We have changed the text, see our reply to main comment 5.

14. Line 39; marked text “Originating”; comment: “???”.

We have changed the sentence to: “The eruption formed the current stage of the large Atitlán caldera in present-day Guatemala.”

15. Line 42; marked text (MT): “km2”; comment (C): “km2”.

Thanks. Corrected to km^2.

16. Line 48; MT: “contributes”. C: “contribute”.

Corrected.

17. Line 52. MT: “von”. C: “von”.

We have deleted the parentheses before “von”.

18. Line 53. MT: “Next,”. C: “???”.

We have changed the sentence to: “This means that a [...]”

19. Line 60. MT: “(M_v=7.9)”. C: “?”.

We have deleted this.

20. Line 65. MT: “kyrs”. C: “kyrs ago”.

Corrected to reviewer’s suggestion.

21. Line 68. MT: “ruled out as unlikely”. C: “Its can’t be ruled out if it is only unlikely.”.

Thanks for pointing this out, we have changed the wording to: “[…], but this is now considered unlikely [...]”

22. Line 70. MT: “very large volcanic eruptions (Mv: 7-8)”: C: “Again, what is this scale? Large in what sense? If it is explosivity, it is not a climate-relevant parameter.”.

See our answer to your main comment 5 above.

23. Line 72. MT: “it’s”. C: “its”.

Thanks, corrected.

24. Line 104-105. MT: “In the climate modeling literature on the Toba super eruption there is a progression from larger climate (and environmental) impacts to smaller as model complexity develop over time”. C: “A trend in results does not mean the more recent results are more correct. Since there are no observations of the Toba aerosols, we don't know how large they got and how good the models are.”

This is true, but it is still interesting since our results represent a break with the recent development. We are trying to represent the views and reasons given by the authors of the previous studies for why this shift is happening. We have changed the manuscript to the following:

“In the climate modeling literature on the Toba super eruption there is a progression from larger to smaller climate (and environmental) impacts as model complexity develop over time. In the more recent climate models one key reason seems to be [...]” .

25. Line 117. MT: “large to very large”. C: “On what scale?”.

We have added the magnitude scale here. Using M>5 as large to very large. See also our answer to your main comment 5 above.

26. Line 150-154. MT: Whole paragraph: C: "You have to show how you did this, and how sure you are. Petrologic estimates are notoriously erroneous. How did you get the error bars? Why didn't you use ice core data? And is it mass of S or of SO2?"

See our answer to main comments 3 and 1 above.

27. Line 156. MT: "(Danabasoglu et al. to be submitted)". C: "Not acceptable as reference".

See our answer to main comment 9 above.

28. Line 166. MT: "5.5e-6". C: "This is Fortran notation. Write it as 5.5 x 10-6. Also give this in height in km."

You're right. This has been corrected. Added "(approximately 140 km altitude)".

29. Line 168. MT: "POP2". C: "Define".

Added POP2 definition to revised manuscript (Parallel Ocean Project version 2).

30. Line 168. MT: "degrees". C: "Use the degree symbol like in the previous paragraph."

Thank you, corrected.

31. Line 169-170. MT: "(Bailey et al., CESM CICE5 Users Guide, NCARdocumentation, PP. 47, June 2018)" C: "This belongs in the reference list".

Agreed. Corrected.

32. Line 171. MT: "Community Land Model version 5". C: "But how is it set up? Dynamic vegetation? Vegetation distribution for what year? Crop model turned off?"

See our response to main comment 4 above.

33. Line 177. MT: "1." C: "1".

Corrected

34. Line 178. MT: "needed to be spread". C: "What needed to be spread?"

This needs clarification. Thus the sentence is changed to:

"The eruption date is set to January, since the eruption season is not known. Injecting this huge amount of mass over one time step in a single grid box was not possible due to model stability. Thus, spreading the injection over longitude (80°-97.5° W) and time (1-6 January) was chosen as a model experiment compromise."

35. Line 184. MT "with constant 1850 forcings". C: "Why? Wouldn't the greenhouse gases and tropospheric aerosols be quite different, not to mention the ice sheets and global climate?"

See our answer to main comment 6 above.

36. Line 199. MT: "Oceanic Niño Index". C: "But what is this? You have to explain how it is calculated and give a reference. Is it Niño3.4? ".

See our answer to main comment 7 above.

37. Line 203-204: MT: "distinguishing between burden anomalies and the same burdens, normalized by the respective maximum values as summarized in Figure 1." C: "I cannot understand what this means."

Thank you for catching this. The sentence has been changed to: "Using our modeling approach results in the atmospheric burdens of volcanic gases and aerosol summarized in Figure 1."

38. Line 205. MT: "normalized". C: "normalized how? Please explain what you did and why you did it. The figures do not explain it."

We have added the following for clarification:

"To compare the decay time of the volcanic perturbations of sulfur and halogens between the different eruption scenarios, we have calculated normalized burden anomalies in addition to standard anomalies. To normalize, we have divided the burden anomalies in each scenario with the maximum burden anomalies in that scenario, providing normalized values between one and zero."

39. Line 208. MT: "life times". C: "lifetimes"

Corrected.

40. Line 208. MT: "remarkably". C:"why?":

This is discussed in lines 220-228 of the original manuscript.

41. 219. MT: "is depending". C: "depends".

Corrected.

42. Line 280. MT: "pentadal". C: "what does this mean".

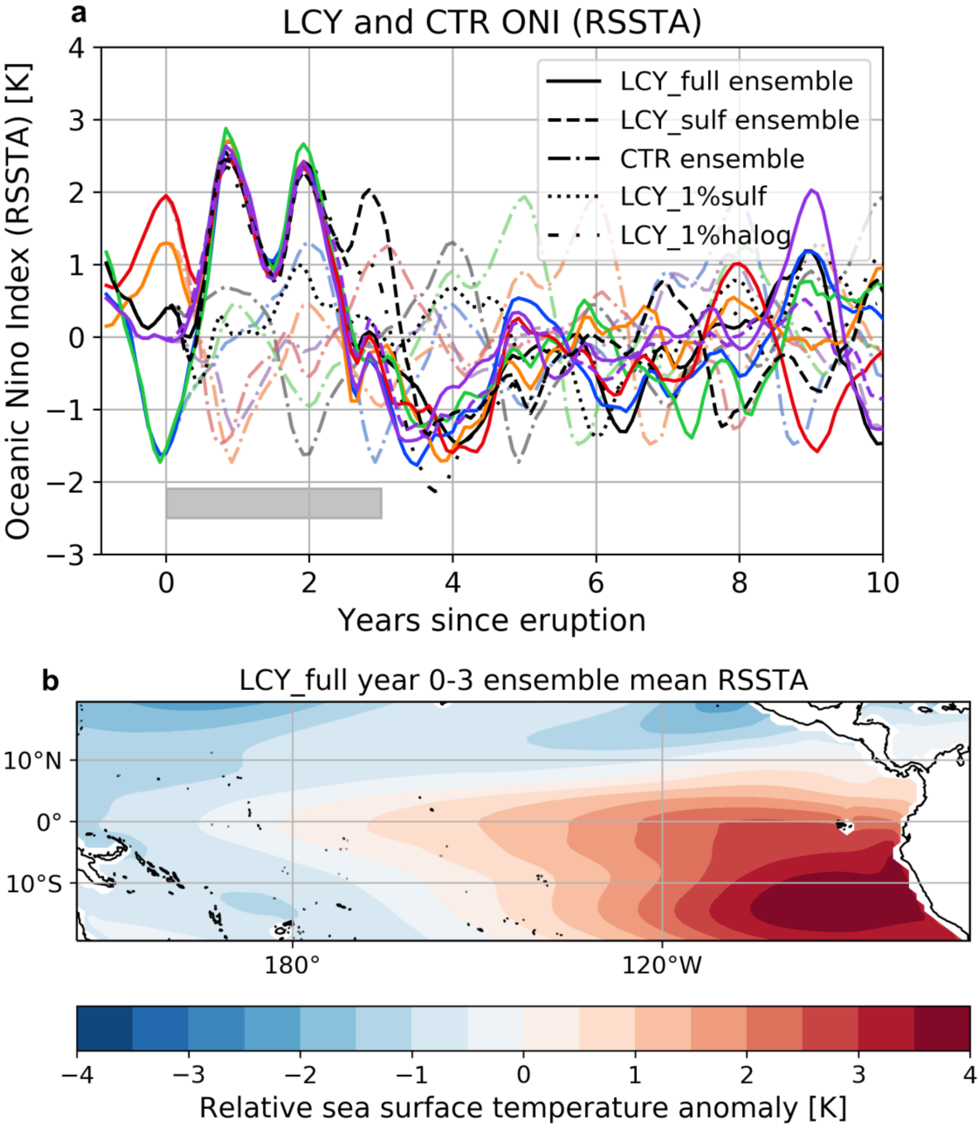
See our answer to your main comment 8 above.

43. Line 318. MT: "a moderate". C: "an [Don't use "moderate." How is "moderate" defined?]"

We agree, and it is not needed. Word "moderate" deleted and sentence corrected.

44. Line 338-339. MT: "which is masked by the strong surface cooling caused by the eruption." C: "Use relative SST, as Khodri et al.(2017) did, subtracting the tropical mean SST from the tropical SST at each point."

Thank you for the suggestion. We have redone the ONI calculations using the relative SST anomalies (RSSTA), following Khodri et al. (2017) (new Figure 6). This quantity isolates the ENSO signal from the volcanic surface cooling. This shows that the simulated LCY eruption causes pronounced El Niño conditions during the first three post-eruption years. The revised Figure 6 using RSSTAs and corresponding text is included below.



*New Figure 6: **ENSO response to the simulated Los Chocoyos eruption and control run (CTR). (a) Ocean Niño Index (ONI) time series based on relative sea surface temperature anomalies (RSSTA) for the LCY_full ensemble, LCY_sulf and LCY_1%sulf (see legend) in full colour. The corresponding model years of the CTR without an eruption (see branch years in Table 1) are indicated with pale colours. (b) Averaged RSSTA over the equatorial Pacific for the first three post-eruption years as indicated by the grey box in (a).***

We have changed sub-section 2.5, and 3.4 to the following:

2.5: Oceanic Niño Index (ONI)

"To select initial conditions for the set-up of the ensembles and to quantify the impact of the volcanic eruptions on the ENSO we calculate the Oceanic Niño Index (ONI) using the model output. The ONI index is used operationally by NOAA to calculate the ENSO state (https://origin.cpc.ncep.noaa.gov/products/analysis_monitoring/ensostuff/ONI_v5.php). The index uses the Nino3.4 region (5° N-5° S, 120°-170° W) SSTs. To calculate the index, the average SST anomalies in the Nino3.4 region are filtered using a 3-month running mean based on centered 30-year periods. If this index is above or below 0.5 K for at least 5 consecutive months, we have an El Niño or La Niña respectively.

For our study we used the full control simulation as the baseline. As the large temperature response caused by the simulated LCY eruption masks the ENSO response initiated by the eruption, we have calculated and used relative sea surface temperature anomalies (RSSTAs) instead of the SST anomalies following Khodri et al. (2017). The RSSTA is calculated by *removing the tropical mean (20°S – 20°N) SST anomaly from the SST anomaly at every point. This quantity better isolates the intrinsic ENSO signal than standard SST anomalies (Khodri et al. 2017)."*

"3.4 El Niño conditions

The ENSO response of the simulations is shown in Figure 6. Even though the initial conditions of the experimental set-up span different ENSO states, there is a rapid convergence to a robust response in the LCY_full eruption scenario. ONI RSSTA values increase above 2 K during the first three years after the eruption. The model ensemble spread is suppressed for five years after the eruption, before beginning to diverge again. The ONI values exceed the range of natural variability in the control simulation with two distinct maxima during post-eruption years 0 (September to November) and 1 (November to January). The LCY_sulf and LCY_1%halog simulations reveal an even longer lasting strong El Niño response lasting into year 2 in accordance with the longer-lasting volcanic forcing (Figure 2).

Maps of RSSTA (Figure 6b) for LCY_full (and LCY_sulf not shown) depict a strong El Niño response shifted to the SH maximizing at 12°S coherent with the southward shift of the ITCZ (Figure 5).

This clearly shows that the simulated LCY eruption causes pronounced El Niño conditions shifted to the SH tropics during the first three post-eruption years."

45. Line 342. MT: "tendency". C: "Again, tendency of results is irrelevant and not a measure of which ones are correct."

Sentence changed to: […], and larger than other recent simulation studies of super eruptions.

46. Line 404. MT: "the super eruption of the last 100 kyrs". C: "What does this mean? Are you saying that Toba was not a supereruption?"

Changed sentence to clarify: "Finally, LCY might have been the eruption of the last 100 kyrs with the largest climate impact since a new, higher, sulfur mass estimate has just been released for it (Cisneros et al in review.) and Toba is estimated to be less sulfur rich than previously assumed (Chesner and Luhr,2010)".

47. Line 407. MT: "no tephra". C: "You don't need tephra to find sulfur deposits. If you can't find the sulfur, you have do doubt your estimates of the emissions."

See our answer to main comment 3 above.

48. Line 644, caption to Figure 1. MT: "horizontal lines". C: "Which line is which number?"

We have changed the legends in this figure. See revised Figure 1 (Figure R1 in the back).

49. Line 644, caption to Figure 1. MT: "1/e2 and 1/e3". C: "1/e2 and 1/e3. ??"

Thank you, this formatting is wrong. Should be $1/e^2$ *and* $1/e^3$*. Corrected in revised manuscript.*

50. Caption to Figure 4. MT: "pentadal". C: "What pentad? This needs to be defined and explained more clearly."

The revised caption now reads:

"Figure 4: Maps of quantities averaged over the first five (pentadal) post-eruption years: AOD (a,b), ozone anomaly and climatology (c,d), ozone change (e,f) and surface UV-B weighted for

DNA damage change and climatology (g,h) for left side LCY_full (a,c,e,g) and right side LCY_sulf (b,d,f,h). "

51. Caption to Figure 5: MT: "Figure 5". C: "Needs another column of the differences between first two columns."

We have added the difference as a third column to the right see Figure R2 at the end of this document. As the difference is important but the three column figure is too small to display we have decided to show only LCY_full and the difference LCY_full-LCT_sulf in the new Figure 5. The LCY_sulf results will go into the supplement as Figure S3 (see Fig. R6). The following text has been added to the manuscript:

L280: "In Figure 5 (a, b) we show post-eruption pentadal average maps of surface temperature anomalies for the scenario LCY_full and the difference to LCY_sulf; LCY_sulf is added to the supplement (Figure S3a). Higher surface temperatures in LCY_full than LCY_sulf cover almost the whole globe except polar regions, which might be slightly cooler since ozone depletion in the stratosphere is a negative radiative forcing on the global climate system (Myhre et al., 2013). [Temperature anomaly patterns ….]"

L314: "Post-eruption pentadal precipitation patterns are shown in Figure 5 (c - f) for LCY_full and the difference to LCY_sulf; LCY_sulf is added to the supplement (Figure S3 (b, c). [Pentadal precipitation patterns …]"

319-320: "Comparing LCY_full and LCY_sulf, the impacts are generally weaker for the first scenario both where we find drying and wetting."

52. Comment to Figure 6. Comment: "What are these blue bars? They are not described in the caption."

According to your suggestion, we have revised Figure 6 calculating RSSTA after Khodri et al (2017). The revised figure 6 and caption is added as Fig. R3 to the back.

53. Comment to panel (b) of Figure 7. Comment: "These two are the same. How can that be?"

Thanks for spotting this. We have corrected the legends of the sub-panels 7 b-d writing now "LCY_1%sulf" for the red diamond marker as for legend 7 a).

54. Comment to panel (c) of Figure 7. Comment: "No dots on figure."

Thanks for noticing. Robock 2009 removed from legend. Added to this panel by mistake.

II) Reply to Reviewer 2

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 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.

Thank you for your constructive comments. We agree that the paper concerns a large number of topics and that navigation can be an issue in papers like this. For the revised manuscript we will make sure that all cross references in the paper are correct to make navigation easier. However, we decided not to change the structure of the paper and figures due to the other two reviewer's comments and as we still think that it is following the model experiment-forcing response-effects flow in a best possible way. Certain parts of the manuscript were shortened as the introduction (Siberian Trap volcanism) and others had to be more detailed (volcanic emissions and uncertainties). We also added more discussion of our results, in particular on the aerosol climate forcing, ENSO, ocean-sea ice and water vapour feedbacks addressing the superficial point (see also our reply to Reviewer 3). We will make careful revisions to the structure so that the paper becomes more easily readable. We have replied to the specific comments below.

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?

Iodine released from volcanoes would have even a larger ozone depletion potential than chlorine and bromine (Solomon, et al 1994), however no direct iodine measurements are available for the Los Chocoyos eruption. Volcanic iodine measurements reported so far in the literature for the volatile release from the Minoan eruption of Santorini by Cadoux et al (2015) are made by interpolation not by direct measurements which gives room for flaws since general obtained experimental iodine ratios like Cl/I, or Br/I, cannot be applied 1:1 to real natural volcanic glass samples.

We add the following sentence to the introduction:

"Potential volcanic iodine injections to the stratosphere (Schönhardt, et al. 2017) would have even a larger ozone depletion potential than chlorine and bromine (Solomon, et al 1994). However, no direct iodine erupted mass measurements are available for the Los Chocoyos eruption."

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.

Following Alan Robock's and your comments, we have decided to remove the Siberian traps volcanism and following discussion from the revised manuscript.

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.

Thanks for your comment. We have added missing details and uncertainties of the used volcanic emissions in Section 2.1 referring also to our previous paper (Brenna et al 2019). See also our response to A. Robock's main comment 3.

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).

We have added the following to section 2.2:

"The quasi-biennial oscillation (QBO) is internally generated and has a period of ~27 months close to observations (Gettelman et al., 2019). However, the QBO amplitude is too weak and the oscillation does not extend into the lowermost stratosphere which can impact QBO teleconnections to the extratropics."

The Gettelman et al. (2019, JGR) paper is now published. Danabasoglu et al. and Cisneros et al. are now submitted and under review (attached). We have added these details to the revised ms and will update the reference list.

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?

See also our respond to A. Robocks comment 5. The sentence is changed to:

"The eruption date is set to January, since the eruption season is not known. Injecting this huge amount of mass over one time step in a single grid box was not possible due to model stability. Thus, spreading the injection over longitude (80°-97.5° W) and time (1-6 January) was chosen as a model experiment compromise."

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?

Thanks for this comment. The CTR experiment is a single run of 70 years with constant PI 1850 forcings and a stable climate, branched from a long control simulation provided by NCAR. The uncertainty estimates are obtained using each year of the CTR simulation as independent realizations of the CTR climate to estimate the range of natural variability. The "ensemble" presented in Figure 6 is generated by matching the branch years of the individual eruption *simulations (see Table 1) with the comparable period of the CTR simulation by using the same colour and line styles. We have added in the revised manuscript that the CTR is a single simulation in section 2.3 and changed the figure legend and caption of Figure 6 for clarification.*

6. Table 1: You have a long and complicated list of LCY_full ensemble members herewith 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.

   Thanks for your comment. We have simplified Table 1 by only listing the ensembles.

7. 184: You need to say something about why you use 1850 forcing. The climate of 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.

1850 pre-industrial conditions served as the best feasible model set up for our Los Chocoyos Atitlan eruption model experiment. Ice core records reveal CO2 levels between 220 and 240 ppm versus 285 ppm, no rapid climate transitions and similar orbital forcing 80.800 years ago compared to 1850 Pre Industrial (PI) conditions. Thus, we expect our model set up and experiment to be a good estimate for the paleo climate response of the Los Chocoyos Atitlan super-eruption under 1850 PI conditions.

We have clarified this in the revised manuscript. We have taken up your and Alan Robock's comments and added the following sentences to 2.3 Model experiments and Section 5. Summary and conclusions:

Line 179: "We run the Los Chocoyos Atitlan super-eruption model experiments under 1850 PI conditions which was the best feasible model set up we could achieve. "

Line 412: "We simulated the eruption of Atitlán for 1850 pre-industrial conditions with 523 Mt sulfur, 1200 Mt chlorine, and 2 Mt bromine emissions. The model results may have been similar for Los Chocoyos 80.800 years ago, as we did not set up the simulations with observed initial conditions and there are uncertainties in volcanic emissions. As expected, if there are large halogen emissions, the climate response is different that if the volcano only emits sulfur into the stratosphere. Overall, we evaluate our model results to show a low climate and environment response given the low estimates for our petrological derived volcanic emissions."

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

   See our response to reviewer Alan Robock comment 7 above.

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.

   We have clarified in the revised manuscript. The sentence now reads: "The following e-folding times (decrease from 1/e to $1/e^2$ and from $1/e^2$ to $1/e^3$) are shorter, a bit less than 1 year. After ~5 years most of the sulfur has been removed from the atmosphere."

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

We agree and we have removed the statement and citation which makes the statement general. The sentence now reads: "This indicates that gravitational settling is playing a minor role as a removal mechanism for the aerosol mass in this model, and removal processes will tend to happen on the transport time-scale of the stratosphere."

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

    Thank you for noticing. Reference to Figure 3b removed.

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

    Thank you for noticing. Figure reference updated.

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.

    Unclear comment as no line is provided. No action taken.

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

    Thank you. Figure updated

14. 323: 3D –> 3E

    Thank you for noticing. Figure reference updated.

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

    Thank you for noticing. Figure reference updated.

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?

    We have revised Figure 6 and other figure captions according to Alan Robocks review (see our reply to his supplementary comment 44 and general comment 10 and our revised figure 6 (Fig R3) at the end of this document).

16.1: 355: check parentheses and dots

Thank you for noticing. We have corrected it.."(see discussions by English et al., 2013 and Marshall et al., 2018)"

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.

We agree and have added: "[…] longer aerosol life time, larger radiative impacts and larger surface cooling per injected sulfur mass to the stratosphere than those studies (English et al., 2013; Metzner et al., 2014; Timmreck et al., 2010). A model intercomparison for the Tambora eruption revealed that version 5 of WACCM also has the longest aerosol life time among compared models (Marshall et al., 2018)."

17. 364: parentheses

Thank you for noticing. Corrected.

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 multimodel 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.

Thank you for your suggestion. This part was indeed confusing and we have deleted it from the revised manuscript.

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.

Thank you for your suggestion. "Released" changed to "reported".

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

Thank you for noticing. The words "mass estimate" were lost at some point.

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

Thank you for this comment. We have corrected it.

III) Reply to Reviewer 3

The paper simulates the climatic and environmental effects of the Los Chocoyos super eruption using an advanced Earth System Model with the interactive bin aerosol module. The specific feature of this research is the effect of the volcanically emitted halogens on the ozone layer and the volcanic effect in general. The subject of the study is scientifically intriguing and timely. The chosen approach is scientifically sound. I suggest the paper could be published after a major revision.

General comments: The paper is a little superficial. The authors choose to discuss multiple aspects of the simulation but did it relatively shallow. The discussion would benefit from the relevant references. It is not like the authors do not have any references, but in many places, it would be better to make the text more strict and reference proper prior studies. The authors have to formulate their science questions explicitly and make a stronger focus on their primary research subject, i.e., the ozone depletion and its effects on temperature and precipitation. The discussion of the other physical effects is sketchy, and the mechanisms are not well explained. The text has to be cleaned up and corrected from grammatic errors.

Thank you for your constructive comments. We agree that the paper concerns a large number of topics and that we did not address all aspects in great depth. Thus, taking yours and the other two reviews into account we have carefully revised the paper by streamlining the text and figures and discussing relevant prior studies mainly in Sections 1, 3, and 4. See also our answers to your specific comments below. We have added the scientific aim explicitly at the end of the Introduction. The primary goal of this paper is to investigate the combined effect of the sulfur and halogen rich Los Chocoyos super-eruption on climate and environment, which is a complex topic. Following your detailed suggestions, we have added more background on the physical effects and discussion for aerosol-climate forcing, modeling Toba, ENSO, ocean-sea ice, and the water vapor response to address the superficial point. The text has been cleaned up and streamlined according to all three reviewers' suggestions and the grammar has been checked by a native speaker.

Specific comments:

L23: "southward"

Thanks for the comment. Corrected

L38: Correct the sentence

Taking also the other two reviewers comments into account, we have changed this sentence to the following:
"The Los Chocoyos (LCY) super-eruption (Kutterolf et al., 2016), of magnitude M=8 (Pyle, 2013), dated to 80.8±6.7 kyrs before present (Cisneros et al. in review), has been known to be one of the largest volcanic eruptions of the past 100,000 years (Drexler et al., 1980)."

L47: "block", "cool"

Thank you for noticing, corrected in revised manuscript.

L72: Bekki did not have ocean and did not account for the cross-tropopause water vapor transport

We agree and have changed the paragraph (Line 70-72) to the following to address our point more clearly; red highlights the changes we introduced:

"A thorough investigation of the climatic and environmental impacts of very large volcanic eruptions (M: 7-8) requires the use of comprehensive coupled climate models or, ideally, Earth System Models (ESMs).There are several studies published with different model complexities, mostly focusing on the Toba eruption and its sulfur impact on the atmosphere and climate. […]"

L103-104: English et al. (2013) do not account for aerosol radiative effect at all

We have updated the sentence to: "In their model setup, neglecting aerosol radiative effects, they simulate even lower peak AOD values (~2.6) […]"

L105: "smaller ones"

Thank you. Corrected.

L117: Please reference proper studies

We have added Zanchettin et al (2014) and changed the following text with regard to this comment:

Added to line 93-94:
"Another model study of Toba, presented in Timmreck et al. (2010, 2012) and Zanchettin et al (2014), simulated a smaller climate impact with peak cooling of 3.5 K lasting up to 10 years from injected sulfur compared to the analogous simulations in Robock et al. (2009)."

We have exchanged line 116-120 with:
"[….South America and Africa.]
Recent studies proposed a sea ice/ocean mechanism which prolong the volcanic induced short, abrupt surface cooling and sea ice increase to longer time scales (decadal) with the ocean sustaining the signal by buffering and transporting the cooling poleward (Miller et al., 2012; Zhong et al., 2011). In addition, Zanchettin et al (2014) simulated an interhemispheric respond to different volcanic forcings with Pinatubo to Toba strength with Arctic sea ice expansion for all cases and an Antarctic sea ice expansion and subsequent contraction only for the super-eruptions.
We are not aware of studies of super-size eruption effects on the El Niño Southern Oscillation (ENSO), whereas the effects of large to very large volcanic eruptions have been widely discussed in the literature. There is an ongoing debate (Stevenson et al.,2017) that tropical volcanic eruptions […]"

Zanchettin, D., Bothe, O., Timmreck, C., Bader, J., Beitsch, A., Graf, H.-F., Notz, D., and Jungclaus, J. H.: Inter- hemispheric asymmetry in the sea-ice response to volcanic forcing simulated by MPI-ESM (COSMOS-Mill), Earth Syst. Dynam., 5, 223–242, https://doi.org/10.5194/esd-5- 223-2014, 2014.

L120: Please reference Predybaylo et al. and Pausata et al

We have added Predybaylo et al. (2017) to the list of references for an El Nino response in line 123. Pausata et al 2015/2016 investigate high latitude eruptions effects which is not our target here.

L144-147: Please formulate science questions explicitly

We have added the primary goal and scientific aim explicitly starting from line 145:

[… super volcanic eruption.] The primary goal of this paper is to investigate the combined effect of the sulfur and halogen rich Los Chocoyos super-eruption on climate and environment. In particular we study the impacts of Los Chocoyos by varying eruption composition and size on: i) atmospheric burden of volcanic gases and aerosols; ii) ozone and UV); iii) climate and environment; iv) ENSO. Finally, we compare with other model studies before we give a summary and conclusion. In a forthcoming paper […]"

L149: "eruption"

Thank you, corrected.

L144: Why 10% of halogen mass?

We assume you mean L174. We use 10% injection efficiency for halogens based on our previous arguments in Krüger et al. (2015) and Brenna et al. (2019). 10% is a reasonably conservative estimate for halogen injection efficiency based on observations and simulations of volcanic plumes, yielding ranges from 2-25% (Brenna et al 2019). To better address this point we have added the following to the manuscript: "[…] which we consider a reasonably conservative estimate for halogen injection efficiency based on observations and simulations of volcanic plumes, yielding ranges from 2-25% (see further discussions in Brenna et al. 2019 and Krüger et al. 2015)"

L177-178: Wrong sentence

See our response to Alan Robock and Reviewer 2 above. The sentence has been changed to:

"The eruption date is set to January, since the eruption season is not known. Injecting this huge amount of mass over one time step in a single grid box was not possible due to model stability. Thus, spreading the injection over longitude (80°-97.5° W) and time (1-6 January) was chosen as a model experiment compromise."

L198-200: What is ONI? Why don't you take the existed Nino3.4?

See our reply to reviewer Alan Robock and Reviewer 2 above.

L214-219: What about the tropopause layer warming that will lead to increasing the water vapor flux into the stratosphere?

This effect is included in WACCM6 which allows more water vapour, hence HOx, to enter the stratosphere based on volcanic aerosol heating of the tropopause after 6 month up to year 3 after the eruption. This analysis is part of a second paper to be submitted in the next months. We have added the following sentences to line 218:

"This OH depletion effect may be partly offset by an increase of water vapour and hence HOx into the stratosphere due to the volcanic aerosol heating of the tropical tropopause. However, as the tropical tropopause layer is warming after 6 month up to year 3 after the eruption (not shown here), we evaluate this effect to play a minor role during the first half year after the eruption when the SO_2 conversion mainly takes place."

L225-228: This is not consistent with Timmreck et al. (2010). Maybe you underestimate the size of sulfate aerosol particles?

We have changed the manuscript to the following:

"After the eruption of Pinatubo, aerosol radii were estimated to be approximately 0.5 μm (Russel et al 1996), compared to 0.2 μm for our LCY_1%sulf scenario and 0.7 μm for the other scenarios, which might indicate that the aerosols are too small in our model. […]. This indicates that gravitational settling is playing a minor role as a removal mechanism for the aerosol mass in this model, and removal processes will tend to happen on the transport time-scale of the stratosphere."

L259: Toohey was not the first who studied this effect

Thanks for pointing this out.. But indeed, this is the first study we are aware of who shows a strengthening westerly wind (positive Southern Annular Mode) effect for very large eruptions so we have added the following:

:

"which acts as a transport barrier for the volcanic aerosols for very large eruptions (Toohey et al,. 2013)."

L266: Is it an increase in 5.45 times, or it is an addition of 545% of mass?

The surface UV flux increases 545%. Sentence changed to: "Global average UV increase over the five-year period is 545 %."

L310-316: The interhemispheric asymmetry of the aerosol plume maybe experiment and model-dependent.

We are assuming that you refer to line 300-306 here. Interhemispheric asymmetries of the aerosol plume have been indeed simulated also by other models and experiments in a systematic way (e.g. Toohey et al 2013, 2019) and reflect our basic understanding of the aerosol transport through the Brewer Dobson Circulation. Model dependent differences occur and are already discussed in Section 4.

L330-339: References are needed. If you talk about the impact on ENSO, why don't you talk about the impact on the overturning circulation?

According to Alan Robocks comment we have revised Figure 6 (see Fig. R3 in the back) and adapted the text accordingly. We added Khodri et al 2017 to this paragraph and refer to other ENSO papers in the introduction.

We have included the impact on ENSO as we started from different ENSO initial phases for the LCY experiments. We have also included ocean heat transport anomalies in Fig. S3 which we think is important for the atmosphere/sea ice/ ocean feedback mechanism. Changes of the meridional overturning circulation in the ocean would be indeed interesting but are beyond the scope of our paper here.This have been investigated by Stenchikov et al 2009, Otterå et al 2010, Zanchettin et al 2012 among others.

L353: English et al. (2013) do not account for radiative effect of aerosols and do not calculate climate response. So it is a wrong reference in this place

Thanks for your comment, we have removed the reference here.

L367-369: Hansen et al. discussed this 30 years ago. Please reference

Thank you for the comment. We added Hansen et al 1980, Timmreck 2012,and Metzner et al 2014 here.

L409: Correct the grammar

Thanks for noticing. The sentence has been corrected

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Figures

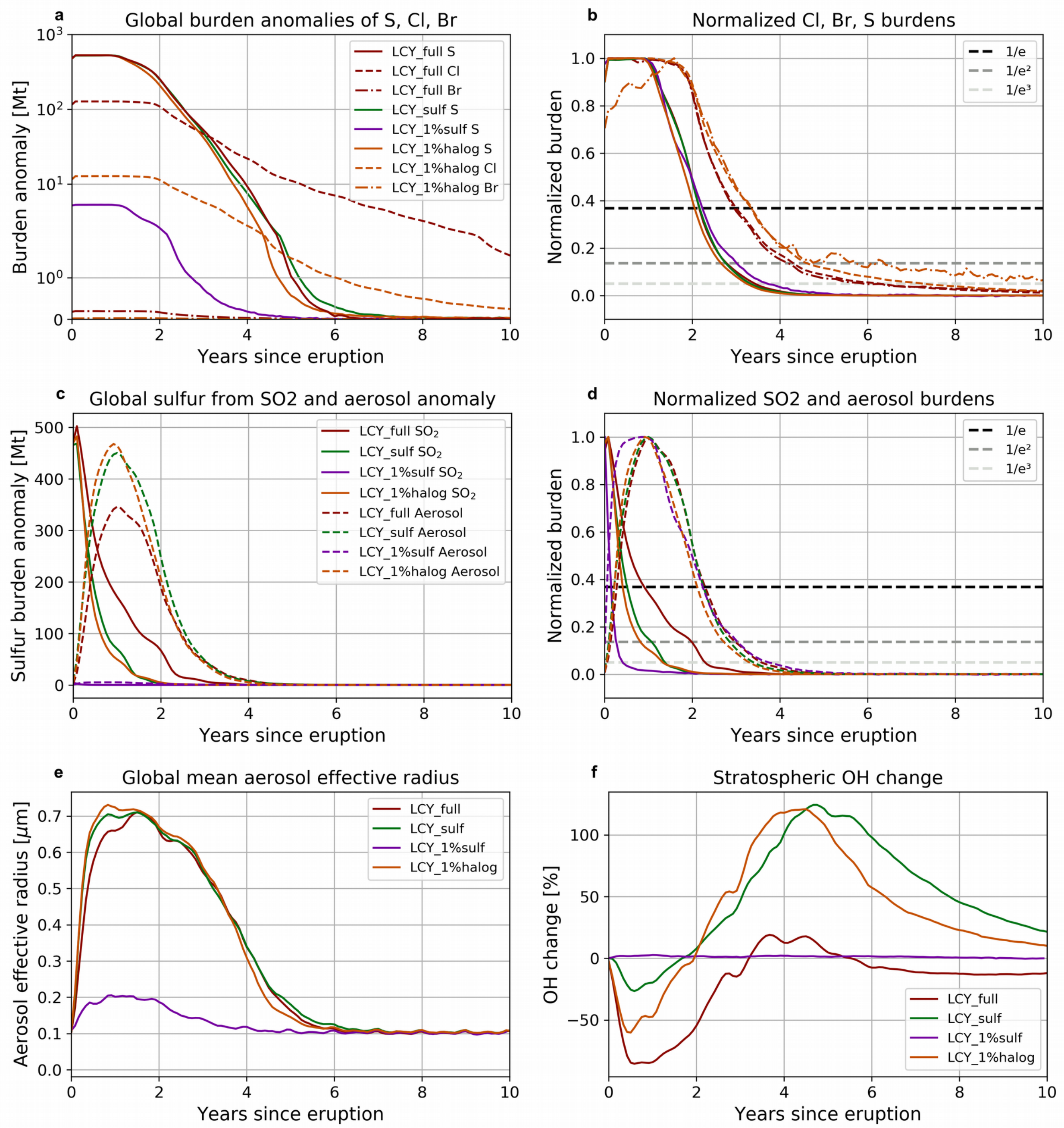

Figure R1: Revised Figure 1. Cleaned up legends (b, d, f).

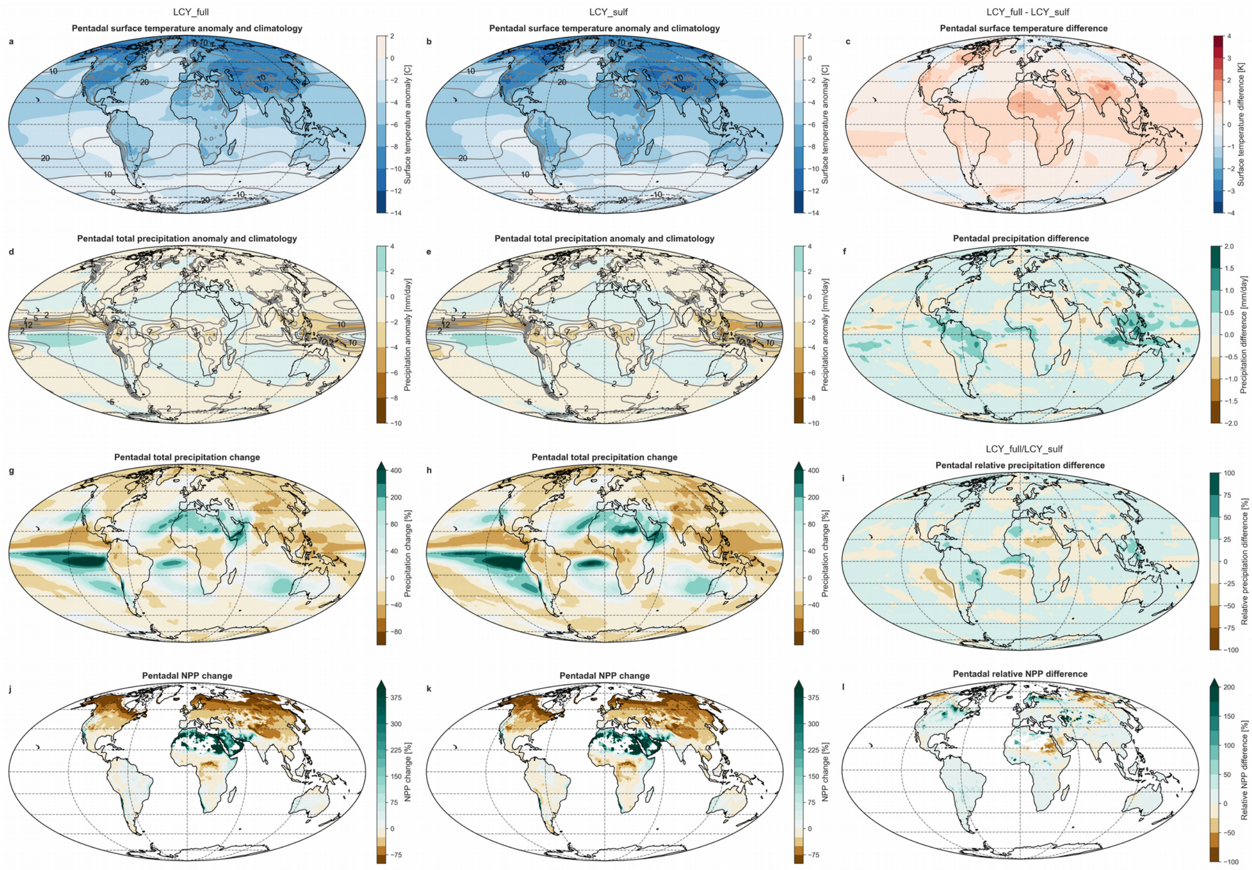

Figure R2: The rightmost column shows the difference (c,f) and relative difference (i,l) between the other two columns.

The revised Fig 5 will show only the left (LCY_full) and right (LCY_full-LCY_sulf) column. LCY_sulf will go into the Supplement as new Figure S3.

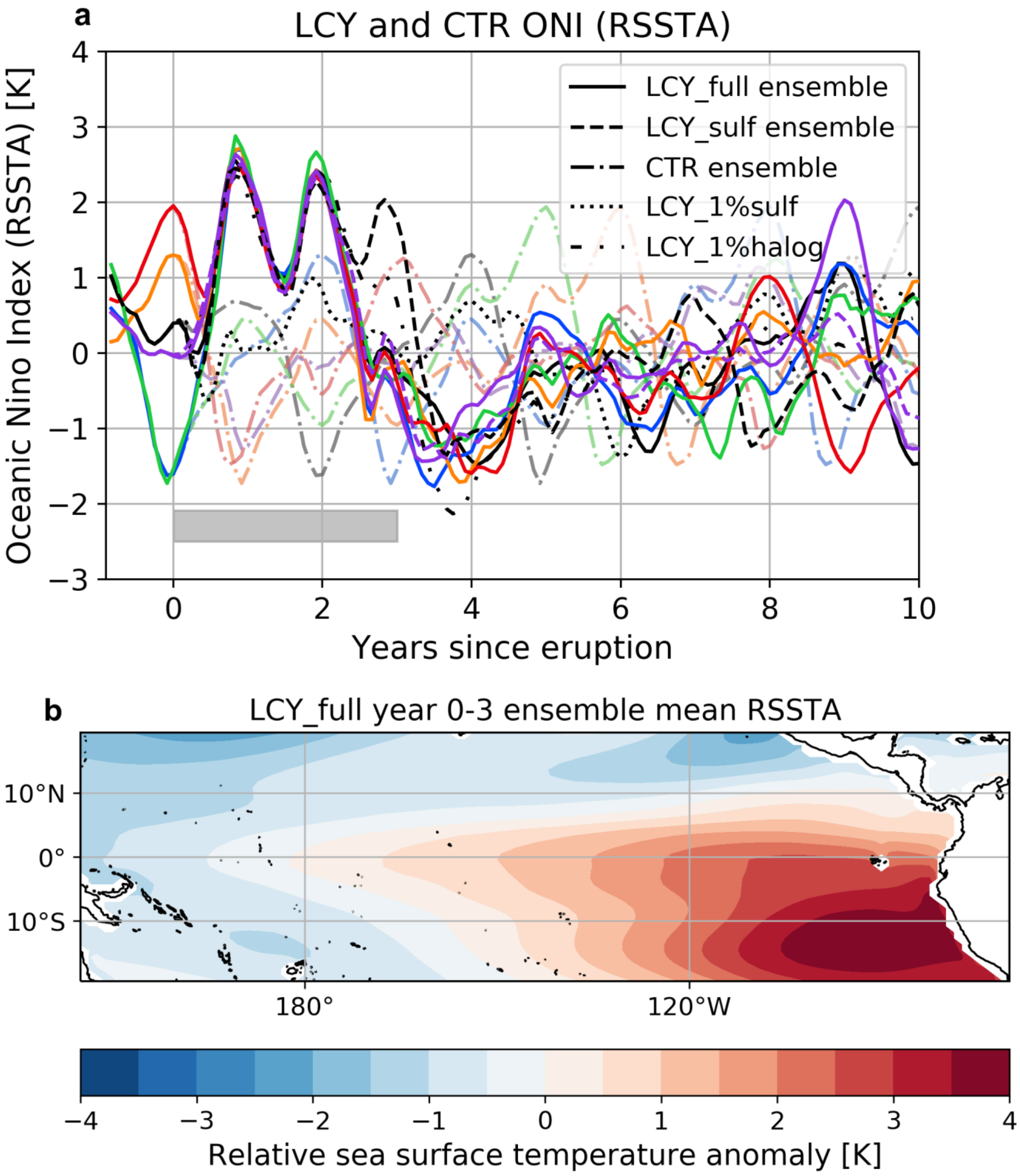

Figure R3 new Figure 6:

***Figure 6**: ENSO response to the simulated Los Chocoyos eruption and control run (CTR). (a) Ocean Niño Index (ONI) time series based on relative sea surface temperature anomalies (RSSTA) for the LCY_full ensemble, LCY_sulf and LCY_1%sulf (see legend) in full colour. The corresponding model years of the CTR without an eruption (see branch years in Table 1) are indicated with pale colours. (b) Averaged RSSTA over the equatorial Pacific for the first three post-eruption years as indicated by the grey box in (a).*

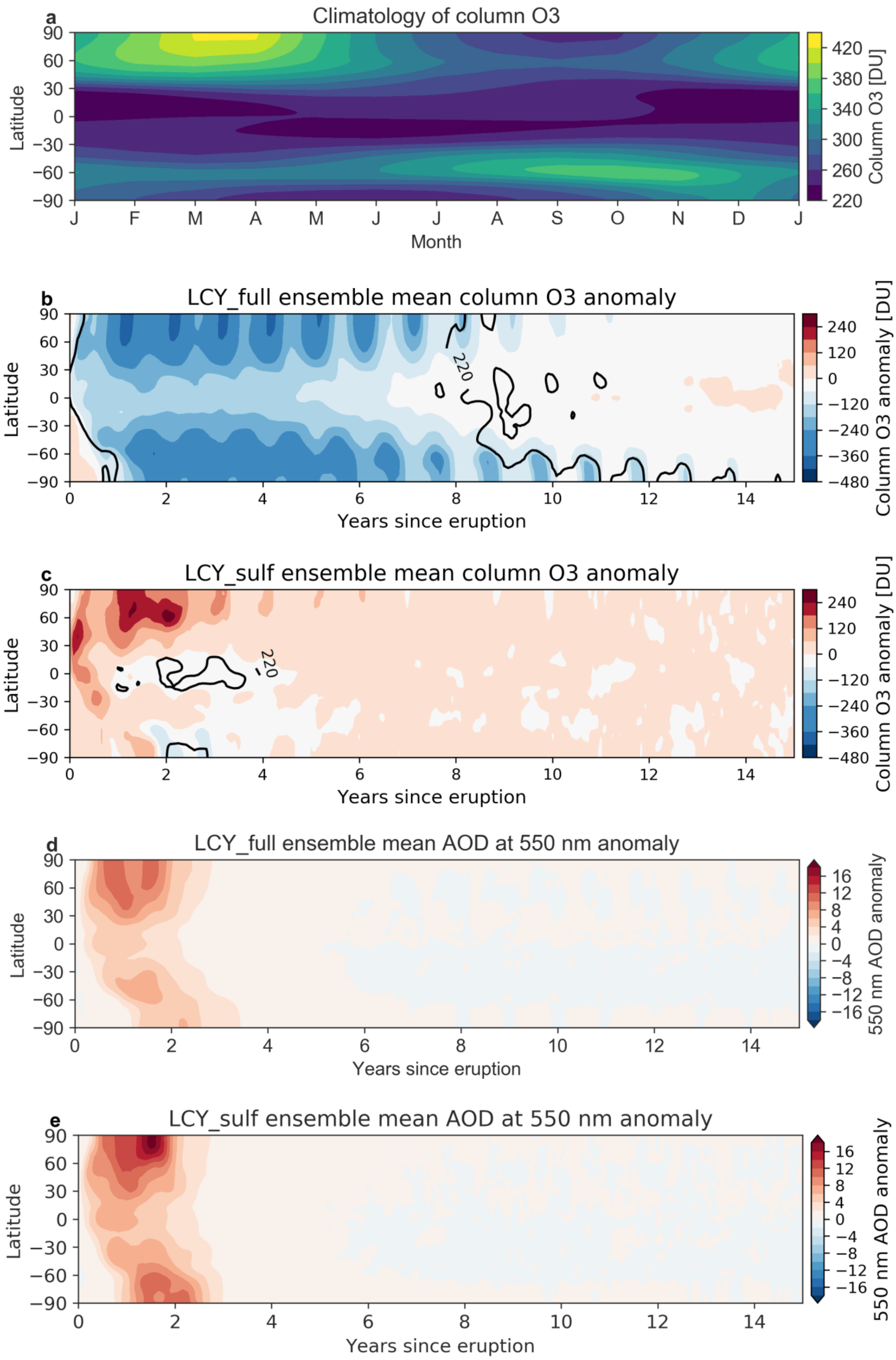

Figure R4: Revised Figure S1. Panel (a) is now plotted from Jan to Jan.

Figure S1: Zonal mean column ozone and aerosol optical depth (AOD) evolution after the Los Chocoyos (LCY) eruption. (a) Column ozone climatology for the CTR. (b) LCY_full ensemble mean column ozone anomaly. (c) LCY_sulf ensemble mean column ozone anomaly. (d) LCY_full ensemble mean AOD anomaly. (e) LCY_sulf ensemble mean AOD anomaly. See Table 1 for information about the eruption scenarios and model simulations.

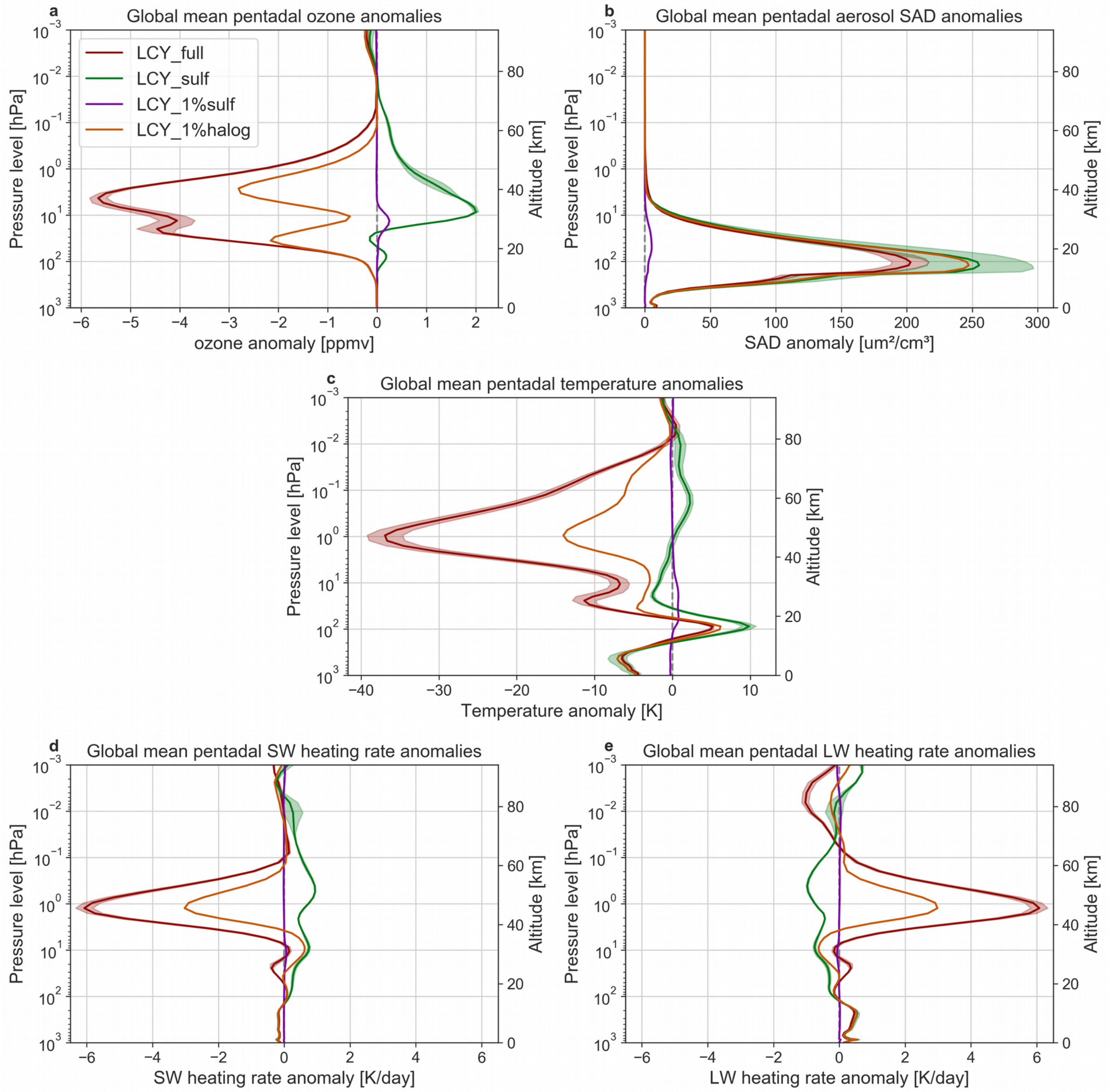

Figure R5: Revised Figure S2. We have added uncertainty ranges and an altitude scale.

Figure S2: Global mean post-eruption five year (pentadal) mean anomaly profiles of (a) ozone concentration, (b) aerosol surface area density (SAD), (c) temperature, (d) short wave heating rate and (d) long wave heating rate of the LCY eruption scenarios. Shading represents the two standard deviation range.

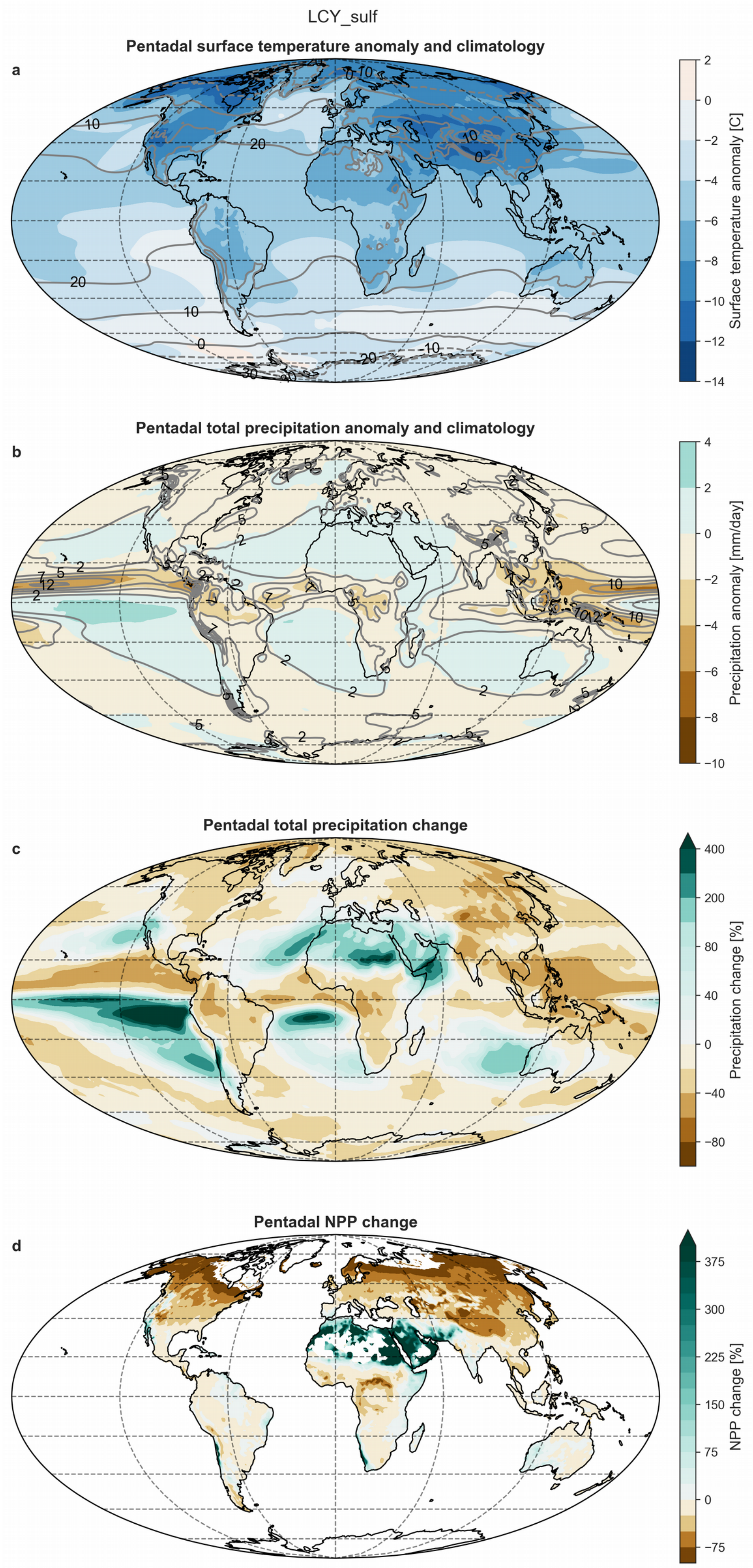

Figure R6: New Figure S3.

Figure S3: Maps of post-eruption five year (pentadal) mean surface temperature anomaly and climatology (a), precipitation anomaly and climatology (b), precipitation change (c) and NPP change (d) for LCY_sulf. White areas on the NPP maps indicate invalid values.

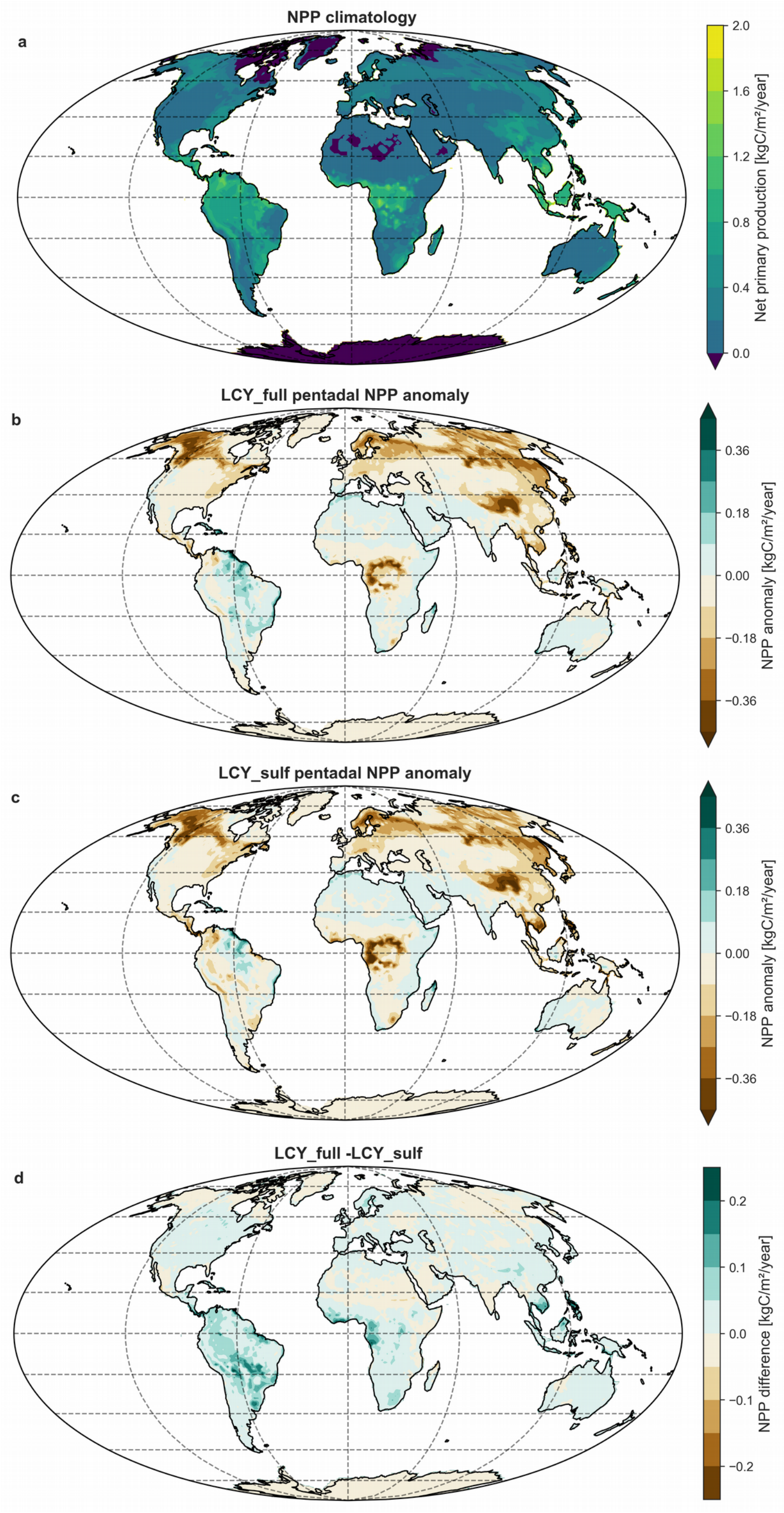

Figure R7: Revised Figure S5 (formerly S4).

Figure S5: Global maps of net primary productivity (NPP) climatology for CTR (a), post-eruption five year (pentadal) anomalies for LCY_full (b) and LCY_sulf (c) ensembles. (d) shows the difference between (b) and (c). White areas on the map indicate invalid values.