Answers to Reviewer1

Simulation of ash clouds after a Laacher See-type eruption
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We thank the reviewer for the kind and thoughtful comments which greatly improved the manuscript and figures. We followed the recommendations on changes in the figures, e.g. the location of the volcano in Fig 1 (now Fig2). We added a new figure (Fig 1) to the introduction showing the observed ash lobes and changed the order of the first figures in the supplement. We considered all other comments carefully and changed the text accordingly. Overall, the we changed the wording of many sentences due to the request of reviewer 2.

*The questions and comments of the reviewers are in **bold**, answers in italic and new or changed text is marked blue.*

General comments

This study explores the potential ash clouds after a Laacher See-type eruption using a series of model simulations with different sulfur and fine ash emissions and different injection altitudes. The study picks meteorological conditions in order to best match the ash deposits and analyses the dispersion of the ash and sulfate aerosol, and the radiative impact of the sulfate aerosol. The study finds that the ash cloud rotates, which is also dependent on the altitude of the injection and that the ash also impacts the dispersion of the sulfate aerosol and consequent aerosol lifetime and radiative impact of an extra-tropical eruption.

The study is interesting, multi-disciplined, and well-structured, and I recommend publication with some minor comments.

Specific comments

The importance of ash for changing the sulfur distribution is clear and this could be emphasized in the conclusion as a necessity for future studies.

Thank you. We agree that this should be emphasized a bit more. We added a few sentences to the first paragraph in the conclusion: This shows the importance of ash for the sulfate distribution after a strong extratropical volcanic eruption. Therefore, fin ash should be taken into account in future studies — a recommendation that differs from our previous results in Niemeier et al. (2009).

However, I would also like to see some discussion on the importance of SO2 scavenging by ash and how this may impact your results (presumably this process is not included?) e.g., Zhu et al. 2020.

Zhu et al. (2020) showed that the reaction of ash with sulfur, SO2 uptake on ash, causes a faster decrease of the sulfur concentration in the two months after the eruption. This reaction is not included in our model. This sulfate uptake would have an impact of the sulfate concentration. The general statement of our paper that ash adds a southerly component to transport would not change. The conclusion that the sulfate lifetime increases over a simulation without ash may change.This is difficult to estimate from the paper as they show results of the first days after the eruption only. On the other *hand, we parameterize the depletion of OH very roughly only, a process that increases the SO2 lifetime.*

We changed the model description to: To simulate the evolution of a volcanic cloud HAM was adapted to a stratospheric version (Niemeier et al., 2009). The initial conversion of SO2 into H2SO4, is simulated with a simple stratospheric sulfur chemistry scheme, which is applied above the tropopause (Timmreck, 2001; Hommel et al., 2011). We prescribe reactive gases (e.g. ozone, nitrogene oxides, hydroxyl radical (OH)) and photolysis rates of OCS, H2SO4, SO2, SO3, and O3 on a monthly mean basis. Therefore, we can parameterize the depletion of OH only: reduction of OH by 90% for the first 10 days, by 50% until 30 days after the eruption. The uptake of SO2 on ash (Zhu et al., 2020) is not included in our simulations. For these simulations, only sulfur sources relevant for stratospheric background concentration were taken into account: DMS was emitted (Stier et al., 2005) and OCS concentrations are prescribed at the surface and transported within the model. Emissions of other sources and other species are set to zero. Details on the specific stratospheric setup of HAM are described in Niemeier and Timmreck (2015). Impact of the model resolution on the results are discussed in Niemeier and Schmidt (2017) and Niemeier et al. (2020).

We partly discuss the impact of SO2 uptake on ash in our discussion (Section 3.3) already and added a short sentence on SO2 lifetime: Observations after the eruption of El Chichon (e.g., Woods and Chuan, 1983; CHUAN and WOODS, 2013; Pueschel et al., 1994) found ash in the atmosphere that was mantled with sulphuric acid, which could be relevant for the simulated sulfate composition in the stratosphere (Zhu et al., 2020; Muser et al., 2020). Our simulations neglect this effect, resulting in a possible slight overestimation of the SO2 lifetime. ....

Additionally, what is the role of the heating of the sulfate in changing the transport?

Heating of sulfate aerosols causes a similar impact as described for ash: a southerly component is added to the transport. Aquila et al. (2012) and Timmreck and Graf (2006) have shown that without aerosol heating the volcanic cloud is transported more quickly to the pole. We added a paragraph on previous work on fine ash in the introduction. Part of the paragraph are some sentences on the role of sulfate heating.

Sulfate aerosols absorb terrestrial and near-infrared radiation. The consequent heating of the volcanic cloud enhances transport towards the equator when aerosol-radiation interaction was incorporated into the models (Timmreck and Graf, 2006; Aquila et al., 2012).

The simulations have two phases of the eruption, but there is little discussion of the impact of the separate phases. Can the influence of the two phases be seen in any of your results? Does it make a difference simulating the separate phases rather than the emissions all at once?

The second eruption is smaller, one third only, and is assumed to raise only to 220 hPa. This low altitude has two consequences: the injected sulfur does not interfere with the sulfur cloud of the first eruption as this is still at higher altitude and, the lifetime of the aerosols is shorter. One can see the second eruption in Figures 5 and 8 in the burden of July in form of small maxima over mid and southern Europe. We added some sentences at the end of Section 3.2.1: The impact of the second eruption is less strong, mainly because of the lower altitude of the eruption, but also because of the smaller eruption mass. The lower altitude avoids a strong interaction with the sulfate of the first eruption. Otherwise enhanced coagulation would cause larger particles. However, the second eruption adds to the sulfate burden as can be seen in results of June in Figure 5.

The authors state the importance of these simulations for risk assessments for future volcanism and also for understanding the social-ecological consequences of this eruption but do not give many details. Could the environmental and climatic impact (temperature, precipitation?) as predicted by these simulations be explored to support these statements?

Our simulations were performed with an atmosphere only model. To give more information on the climate impact, beside the radiative forcing, the simulations have to be performed with an Earth-system model. This is planed for a future part of the related project. Therefore, we have only touched this topic at the end of the Conclusion: Our simulations provide tantalising hints regarding the likely climatic and environmental impacts of the LSE, yet it remains difficult to asses these impacts fully from general circulation models alone. Instead, it stands clear that the impact on climate of both the Late Pleistocene Laacher See eruption itself as well as any future eruption scenarios have to be calculated with a fully coupled atmosphere-ocean model that, for the ancient eruption, takes account of contemporaneous land-sea relations including the fast and abrupt climate changes that occurred during the transition from the glacial to the interglacial. For future eruptions, such modelling efforts similarly need to account for the rapidly changing climatic boundary conditions of the Anthropocene.

The reconstructed ash is mentioned a lot in the introduction but not displayed until Figure 2. Could this be referred to in the text or included as a separate introductory figure, perhaps also with the reconstructed lobes?

Thank you. We agreed and added a new Figure (Fig1).

The introduction also states that signals of this eruption in ice cores are elusive but then in section 3.2.4 mentions studies that have attributed some spikes to this eruption. This seemed a bit inconsistent. Some of the text is difficult to read with missing words e.g.

We state in the introduction that a clear chemical signal or actual tephra shards from this eruption remain unclear. In section 3.2.4 we show sulfate deposition and the text is related to sulfur. We state that there are some candidates, some spices, but they have not been identified to be caused by LSE. We changed the text in section 3.2.4: This finding may guide the identification of Laacher See eruption signals in ice-core data. Previous identification attempts were anchored in assumed dates of the eruption and most commonly looked towards major spikes around the 13ka BP mark . Baldini et al. (2018) ascribe a large sulfate spike at 12,867 BP in the GISP2 (Greenland Ice Sheet Project) ice-core record to the Laacher See eruption. In contrast, Svensson et al. (2020) point at four large bipolar sulfate spikes clustered around 13 ka BP. It remains unclear whether the prehistoric LSE should be associated with one of these major spikes, one of the minor spikes in the adjacent decades, or whether we can at all reliably link any of these sulfate spikes with this eruption. Increased age control on the eruption through, for instance, refined dendrochronological analyses may allow a more confident assignment of sulfate spikes to this particular eruption.

L136-142 - please check throughout.

We changed the text in Section 2.2.1:

Mid- to high-latitude eruptions might not reach as high into the stratosphere given that the erupted column reaches a buoyancy level with the local environment at lower altitude. We therefore also consider two scenarios with lower injection altitudes: 60 and, 100 hPa for SO2, and 80 and, 120 hPa for fine ash, keeping the vertical offset between the sulfur and ash emission layers constant.

L7 - also add the ash injection magnitudes here

Review asked for many changes in the abstract. We changed the related sentences: Our experiments are based around a central estimate for the Laacher See aerosol cloud of 15 Tg of sulfur dioxide (SO2) and 150 Tg of fine ash, across the main eruptive phases in May and a smaller one in June with 5 Tg SO2 and 50 Tg of fine ash. Additional sensitivity experiments reflect the estimated range of uncertainty of the injection rate and altitude and, assess how the solar-absorptive heating from the fine ash emitted in the first eruptive phase changed the volcanic cloud's dispersion.

L14 - 'Resulting in a stronger transport' than what? *We added:* compared to cases without injection of fine ash

L25 - how big is 'some' distance?

Hundreds of kilometers. We added: , e.g. in southern Scandinavia

L81 - please add the length of the simulations

We added to Section 2.2.1: The simulation were started from a control simulation and lasted for 1.5 years after the eruption.

L136 - it is not clear to me why this might be

Within mid-latitudes the tropopause and pressure levels are at lower altitude. Therefore one might assume that the erupted column reaches a buoyancy level with the local environment at lower altitude. We changed the sentence in Section 2.2.2 to: The 1991 Mt. Pinatubo-type eruption was a tropical one. Mid- to high-latitude eruptions might not reach as high into the stratosphere given that the erupted column reaches a buoyancy level with the local environment at lower altitude. We therefore also consider two scenarios with lower injection altitudes: 60 and, 100 hPa for SO2, and 80 and, 120 hPa for fine ash, keeping the vertical offset between the sulfur and ash emission layers constant.

L146 - and also the tropospheric meteorology for deposition

Yes, but the impact is less strong. Withing the troposphere the particles are washed out quickly. Our test simulations showed a strong relation between stratospheric transport and the position of the deposited ash. We changed the text in Section 2.2.2 to:

The initial distribution and subsequent evolution of the volcanic cloud depends on the meteorological conditions of the stratosphere at the time of the eruption (Marshall et al., 2019; Toohey et al., 2019). This is particularly pronounced in mid-latitude eruptions but holds also true for tropical eruptions (Jones et al., 2016). Fine ash deposition patterns reflect the ~~long-range~~ transport of volcanic ash over several hundred kilometers, which is mainly determined by the meteorological situation in the lower stratosphere at the time of the eruption. Test simulations aimed at finding an appropriate injection day showed that the meteorological conditions in the troposphere were less important.

Figure 1 - please add a symbol for the location of the volcano

We did, in Fig3 as well and changed the colors in Fig 3b.

L172-L177 - Can you justify this? Would it be better to continue each simulation to find the best meteorological condition for the second phase considering that the first phase changes the dynamics? Why are only LSE1 and LSE3 chosen for Figure A1?

No, we cannot really justify this decision. It was our aim to show that it is, in principle, possible to simulate the observed ash lobes. It was not our aim to show this for every case. Additionally, it was very difficult to find appropriate conditions for the right transport pattern for both eruption pulses. Therefore, we did not try to find an appropriate condition for all simulations. As stated above, the second eruption pulse is smaller and less important for the final sulfate distribution. In different parts of the text, e.g. abstract, we state now that our experiments were designed around a central experiment LSE1-30. The text changed to: The meteorological situation that gave best agreement to the empirically known MLST-C tephra deposits were obtained for June 20th. This emission timing was chosen for all simulations despite the fact that after the firs teruptive phase LLST, the dynamic conditions changed (as a result of the ash radiative effects, Figure A2) and the deposition structure of the second explosive eruption phase was only reproduced in LSE-30.

We decided not to show too many details and on single results. Therefore we do not show results of LSE2 (LSE60) in Fig A1 (now A2). We shortened the text in this sense as well, mostly Section 3.2.1.

L196 - can these plots show the same regional area, or can the deposits be marked on the model panels? It is hard to compare the model distribution directly with panel b.

We understand your problem. However, adding the deposits would only add another layer to the plot which would decrease the visibility of the streamlines. It was our intention to show the volcanic cloud and the rotation. This would be more difficult if we double the plotted area, as the area of interest would be very small in the plot. We decided to keep the figure unchanged. We changed the caption of Fig. 2 to:

Note the different area in Figures e) and f) which can be seen as an extension of the area in Figure d).

L199 - how much is the heating?

The heating is about 1 to 1.5 K/d in the first days after the eruption in simulation LSE2. We added a reference to Fig A3 to the sentence, as suggest later.

L206-208 and throughout (e.g. L190) - It would be useful here to have the injection altitude in brackets after each simulation name, so you do not have to refer back to Table 1. Sometimes this is done but the other way around e.g. L266. Also on SI figures e.g. A2/S2.

We changed the names of the experiments: LSE-30, LSE-60, LSE-100, LES-30-low, LSE-30-strong, LSE-30-May15 etc.

Figure 3 - can the simulation name, ash altitude and altitude of the streamline be printed together or to the left side of the rows rather than above each separate plot as at first it was a little confusing as to what was being shown. Also, what about there sults for the other simulations -

these do not appear to be discussed. Are the results consistent?

We had to add this slightly strange description on top of each single plot in order to align the plots. We changed the wording slightly and hope it is less confusing now.

L241 - extra-tropical 'northern hemisphere eruption'

Done

L251 - Reff not explicitly shown in Marshall et al. 2019 although inferred. Perhaps better to say 'or suggest' or similar for this study.

Thank you for mentioning this, you are right. We changed the text in Section 3.2.1 to: This is in line with previous studies: Toohey et al. (2019) show that effective radii of volcanic sulfate particles are smaller for an initial injection at 100 hPa compared to an injection at 30 hPa. Stratospheric Aerosol optical Depth and volcanic net radiative forcing results in Marshall et al. (2019) suggest a similar behaviour.

Figure 5 and others - I don't think the 'plotted values' sentence is needed, and these values seem inconsistent with the color bar intervals. Please check.

We agree and changed the captions.

L288 - what do you mean by the absorption in the near infra-red is important? Why specifically ECHAM-HAM?

Absorption of radiation is treated differently in different models. In ECHAM the absorption in the near infra-red is strong, not only in terrestrial wave length. To avoid confusion, we deleted this sentence.

Figure A3 comes after A4, but perhaps should be introduced earlier with the magnitude of this heating stated, for example L199.

Thank you, we followed your suggestion.

L313 - Please add here what the radii are in this study for comparison.

Larger than 0.7 μm. *We added to the text:* *> 0.7 μm*

Figure A2 - please check the x label and ticks

Done

Figure 10 - Can you be more explicit with the titles - NHET burden, NHET forcing, global burden, global forcing. Not hovmoller (also Figure A5 caption)

Done. We changed the caption to: Area average of the ensemble mean of sulfate burden (left) and net radiative forcing....

L317 - except for LSE9? Looks different in the first 6 months.

We added a 'most' *to the text.*

L318 - What about LSE7, which has the largest burden? Is the meteorological condition therefore more important than injection altitude?

The different results of LSE1, LSE6 and, LSE7 (LSE-30, LSE-30-May15, LSE-30-May22) show that the meteorological condition is very important. LSE6 and LSE7 show very different results even the initial transport in May seems to be similar (Fig.9) We discuss this differences in Section 3.2.2. Simulations LSE1, LSE6 and, LSE7 result in higher global burden than LSE2 and LSE3 with lower altitude injec-

tions. The lifetime of the volcanic cloud is shorter in LSE6, the decline of the global burden is faster, than in LSE2 and LSE3, because the particles are larger and sediment faster. This example shows that transport and aerosol microphysics interact.

We changed the text:

The shortest sulfate lifetime, i.e. the fastest decay rate of all simulations with fine ash, show LSE-30-May15, and LSE-60. Both simulations show a strong poleward transport (Figures 5 and 8). LSE-30-May22 shows the latest decay of the maximum values of the global burden because of a strong equatorward component of the transport (Figure 8).

L319 - how is lifetime defined? It is hard to see this on the figure. What about LSE10?

We use lifetime as a synonym for the decline of the global burden curve - earlier decay to low global burden values stands for a shorter lifetime. We added a short explanation to the text: The shortest sulfate lifetime, i.e. the fastest decay rate of all simulations with fine ash, show LSE-30-May15 ....

L321 - also depends on the spatial evolution - is this also at play?

Yes, 'the strong equartorward component of the transport', stands for spatial evolution.

L325 - Tg SO2. Could you add at the end of this sentence why this is or signpost to the discussion.

SO2: thank you for the correction. We followed your suggestion and added: This is mainly caused be the stronger southward component in transport (see also the Discussion).

L329 - I think you can be a bit more explicit here e.g. 'caused by transport dynamics and consequently the amount of aerosol that moves into the southern hemisphere'

Thank you. We agree and followed your suggestion.

L335 - is 'decrease' correct here? Negative radiative forcing?

Negative radiative forcing stands for cooling, the more negative the forcing, the stronger the cooling. Therefore, more negative forcing values describe a decreasing forcing.

L343 - is this with respect to the control/climatology?

No, the deposition values in Figure 11 are absolute deposition values. We discussed this fact and came to the conclusion that an anomaly is difficult to calculate. Circulation in a situation without volcanic eruption differs from the situation with eruption. Thus the deposition of background sulfate will not be the same with eruption than without eruption. Additionally, ice-core data include deposition of background, here natural sulfate of natural sources, as well. however, we realized that Figure 11 (now Fig.12) is not correct. We have not taken into account the monthly mean values of the original data when summing the deposition. We changes the figure accordingly.

L360 - The 100 eruptions is not strictly true - 30 eruptions were simulated, but an infinite number of eruptions can be sampled from the emulator. Perhaps remove '100'. It is a bit unclear whether you're saying the lifetime increases or decreases and the exact comparison that is being made to the emulator study - is this for an equivalent eruption at 50N and 15 Tg? This study also considered eruptions in July.

Thank you. We were so impressed by the amount of simulations that 100 can to my mind without *checking the real number again. We removed the 100.*

L437 - consider moving the text related to LSE3 simulations having a more realistic injection scenario to the discussion.

We moved the following sentences to the Discussion: For our reference scenario LSE1-30 with an injection of sulfur and ash at 30 hPa and 50 hPa, we find conditions for simulating a realistic scenario in one of three years only. At lower altitudes the wind in the stratosphere is more variable and one may find more days with wind patterns that allow an ash deposition comparable to the LSE lobe also slightly later in the year. Hence, our LSE3-100 simulation with an injection height of sulfur and ash at 100 hPa and 120 hPa respectively might present, under present day conditions, a more realistic injection scenario for the LSE eruption. It also reflects the circumstances that no volcanic ash reached Greenland. The deposition pattern of fine volcanic ash also indicates that our strong emission scenario, LSE5-strong with an injection of 100 Tg SO2 and 1000 Tg of fine ash is not likely.

Technical corrections

Abbreviations are not introduced in abstract.

We changed the abstract accordingly - no abbreviations are used.

Appendix vs. SI figures - duplicated? Will be a supplement at the end.

13 ka vs. 13,000 vs. 13,000 ka - please check throughout! Thank you, we checked this and use 13 ka BP.

Table 1 - June 20 'th'. May 7th missing space for LSE3. June 20th also needed for Figure A1/S1. Done, text and figure were changed.

L13 - add hyphen between ash and cloud?

L23 - previously 'been' suggested Done

L53 - 'as well as' -> 'but'

L133 - of of Done

L160 - without 'a' Done

L170 - rile -> role? Done

L214 - towards 'the' south or turn 'southwards' Done

L236 - remove 'also' Done

L245 - the 'sulfur' injection rate Doe

L248 - 'at' 100 hpa Done

L277 - were -> where Done

L278 - LSE9 Done

L285 - simulations Done

L306 - 'due to' Done

L310 - studies -> study, reaches -> reach Done

L317 - In sum - in general? Done

L322 - aggravated? Increased/magnified/larger would be better here We choose increased.

L334 - Opposite -> In contrast, comparable -> comparably Done

L371 - above -> section X

L404 - assumptions Done

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