Review: “Utilizing a Multi-Proxy to Model Comparison to Constrain the Season and Regionally Heterogeneous Impacts of the Mt. Samalas 1257 Eruption” by Laura Wainman et al.

A: General comments:

The authors aim to further constrain the date of the mid-13th century Samalas eruption in Indonesia, the largest volcanic eruption in the Common Era. For this they use ensembles of aerosol-climate model simulations forced with SO_2 injections in either July 1257 or January 1258, the most widely used dates for this eruption. They find that the simulated global climate response for the July 1257 eruption scenario produces better agreement with collated proxy reconstructions for 1257 to 1259 and argue on those grounds that the eruption likely occurred in summer 1257, in agreement with previous papers.

Tropical eruptions often remain difficult to date exactly, because of a lack of documentary records from direct observations and the scarce available information preserved under tropical climates in the areas surrounding the volcanos. The seasonal timing of eruptions can have important effects on the global climate impact. This paper is a welcome contribution to ongoing efforts in this research field. It is well written, with a clear structure and outline of the scientific hypothesis, especially considering this is a master thesis. The setup of the aerosol-climate model experiments appears appropriate (for a non-expert) and I acknowledge that the authors also have collated and included a large set of proxy records from different archives. I would like to invite the authors to address in their revision a number of points (outlined in detail under *Specific Comments*) focusing on three main topics:

1) **The framing:** The previous arguments and references used to argue for a 1258 eruption are somewhat outdated going back to the early 2000s when ice-core records were biannually resolved, large-scale tree-ring records were not fully developed and the source of the eruption had been unknown; in the last years the research community has already largely converged towards accepting a date in 1257, which wasn’t really challenged.
2) **The rationale:** A more general discussion would be welcome to which extent we wanted (or should avoid) to constrain external climate forcing through comparisons of simulated and reconstructed climate response. I noted two specific examples below in which the desire for a high model-proxy agreement has led to speculations of dating errors in tree-rings and in ice cores. In both cases these speculations were later rejected based on climate-independent geochronological data (i.e. radiocarbon, tephra). So there is an eminent risk of overfitting climate forcing records by aiming to minimize model-data disagreement. A short discussion on the risks (beside the potential) would be helpful.
3) **The implications.** I have some reservations regarding your interpretation of the ice-core records (outlined in detail below) and also the implications this study has for the use of ice cores to infer volcanic forcing in the past. Existing limitations to constrain the exact timing of tropical eruptions are dominated by the poorly constrained time-lag of sulfate deposition on the ice sheets following a volcanic eruption, and to a less extent by the resolution of the ice-core records. This time-lag is also highly variable among different state-of-the-art aerosol models. I therefore argue that the resulting spread in stratospheric aerosols and the spatio-temporal climate fingerprint will depend foremost on the choice of the aerosol-climate model. Given these model uncertainties I currently do not see a strong case to constrain the climate-independent ice-core estimates of volcanic forcing with one specific aerosol-climate model.

B: Specific comments:

L35: I doubt that these two references provided the necessary record length, spatial representation and completeness to put the size of the sulfate deposition into a 2500-year context.

L40: please replace Wade et al. (2020) with Schneider et al., (2015).

L45: I am not aware of anybody citing January 1258 as a potential date, and the only citation in this paragraph (Stothers 2000) speaks of early 1258. Please clarify.

L51-52: I find it unlikely that any radiocarbon date on pyroclastic material would provide the necessary precision to contribute to the discussion of the actual calendar year of the eruption. All the more so as the paper describing it is already 20 years old.

L54: Besides TRW anomalies the Western US tree-rings (Salzer and Hughes, 2007) also had frost rings in 1257 and 1259 which combined provide strong evidence of a volcanic source for the cooling and which may help to constrain the age of the eruption.

L57: The statement "peak sulfate deposition in ice cores is recorded for 1259" lacks a reference and it is also not true for all ice cores. Some ice cores have the peak sulfate levels in January 1259 (NEEM), others in November 2018 (WDC06A). See Table 1 below. For the timing of the eruption the timing of the peak sulfate level is anyhow not of great relevance but rather the timing of the start of volcanic sulfate deposition which is dated between 1257.4 and 1258.2 in four different records from Greenland and Antarctica (Plummer et al., 2012; Sigl et al., 2013).

L90: How would spring (April) or fall (September) eruption scenario differ from January and July?

L92: I would refrain from calling the database most complete; for your research question you don't need the most complete dataset but the most suited data.

L140: Your selection criteria should not be annual resolution but annual age precision. Only tree-rings and documentary records fulfil this requirement and all ice cores in which the Samalas eruption had been securely identified. For these reasons, I would strongly advise you to remove the lake sediment records and the Svalbard record from your analysis all of which contain no clear signatures of Samalas and are not dated to 1258 (±0). On the other hand, many more than the three used ice cores (GRIP, CRETE, DYE3) would be readily available for analysis both from Greenland and from Antarctica.

L157-159: A more comprehensive dataset from Greenland would potentially be more skillful than a set of only three ice cores. The PAGES2k Consortium (2017) database contains at least 9 ice cores from both Greenland and Antarctica with annual resolution encompassing the age of Samalas (PAGES2k Consortium 2017).

L159-169: As stated above I don't think that any of these climate information provided for these records can with certainty be linked to the years 1257-59.

L276: Please cite the original reference (Vinther et al., 2010).

Figure 3: What does the absence of symbol indicate (e.g. Japan and Greenland in 1259; one in China in 1258)? No proxy- or documentary evidence available? Tree-ring and ice-core records are continuous and should have data in both 1258 and 1259. Please explain.

Inclusion of the available ice-core records from Antarctica would increase the database for the Southern Hemisphere (currently two records) substantially and better justify to speak of a “most complete globally resolved“ database.

L315-325: Frost-rings in 1259 in SW USA support cold conditions during the growing season.

L328: Sulfate at GISP2 was analyzed at a biannual resolution. The age of the sample encompassing the rise and peak concentration value of 381 ppb is 1257.7 to 1259.6. Unless there is a higher resolved volcanic tracer available from GISP2 I can’t see how one could constrain the peak sulfate fallout at this site to early 1259.

Figure 4: Maybe it would be helpful to also add the SAOD for Western Europe for the recommended PMIP4 forcing by Toohey & Sigl (2017) which is based on an assumed eruption date of July 1257.

L347-358: Your comparison focuses strongly on the timing of peak deposition of sulfate in both UKSEM1 scenarios versus that from ice cores. Based on previous aerosol-model inter-comparison projects it appears there is a large spread among state-of-the art climate models regarding the timing and hemispheric spread of sulfate across these climate models (Marshall et al., 2018) with this disagreement been linked to model physics and chemistry (Clyne et al., 2021). How sensitive are your specific results to the choice of the climate model? Would you get different results if you used a different climate model? Looking at Figure 9 in Marshall et al., (2018) it becomes evident that the spread of sulfate depositions all forced with the same SO_2 injection varies widely across the models, whereas sulfate deposition is rather comparable in timing, duration and magnitudes in ice cores from hemispheres.

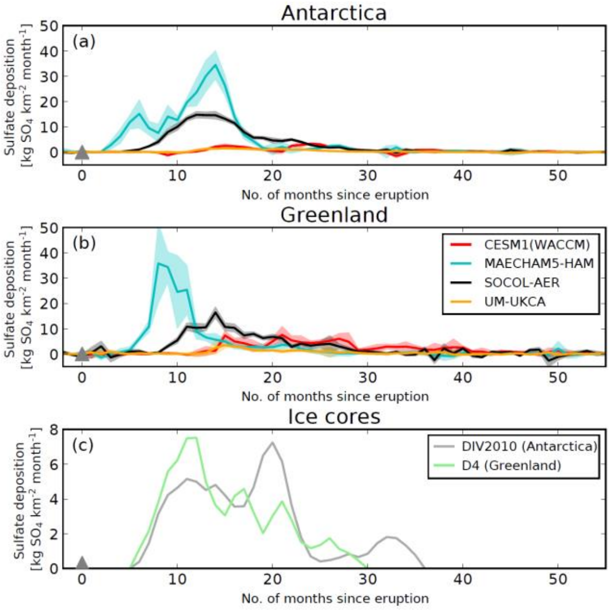

Figure 9. Simulated area-mean volcanic sulfate deposition (kg SO_4 km^{-2} $month^{-1}$) to the Antarctic ice sheet **(a)** and Greenland ice sheet **(b)** for each model (colours). Each ice sheet mean is defined by taking an area-weighted mean of the grid boxes in the appropriate regions once a land–sea mask has been applied. Solid lines mark the ensemble mean and shading is 1 SD. **(c)** Deposition fluxes from two monthly-resolved ice cores (DIV2010 from Antarctica and D4 from Greenland). The scale is reduced in **(c)**. The grey triangles mark the start of the eruption (1 April 1815).

L352-355: I don't fully understand this sentence, which has partly to do with the terminology. Resolution is the time interval contained in a sample (in this case monthly, assuming constant snowfall rate throughout the year); the dating precision is +/- 1 year, which means that one can shift the curve by adding or subtracting a year, but not 6 months as the dating is based on annual-layer counting of seasonal tracers. As noted above the mean age of the initial sulfate rise in four high-resolution ice cores (NGRIP, NEEM, WDC06A and Law Dome) is winter 1257/1258. Assuming a time lag of 6 months between the eruption in Indonesia and the start of sulfate rise in the polar ice cores (as was observed following Tambora 1815, Marshall et al., 2018) gives a best ice-core eruption age of summer 1257. A shift to summer 1256 or summer 1258 would be consistent with the ice-core age uncertainty, but inconsistent with the tree-ring cooling peaking in 1258.

L355: For better comparing the magnitude of sulfate deposition across hemispheres and across the two scenarios and any potential asymmetry in the deposition it would be helpful to provide a table with the total depositions from Samalas in models and ice cores.

It would also be helpful information to know to which extent the timing of initial sulfate, peak sulfate varies among different aerosol-climate models using for example the experiments done for Tambora.

L380: Maybe add: … with the bipolar ratio *of simulated sulfate depositions* between northern and …

L384-387: Here would be a good opportunity to put your results into context with a recent study (Guillet et al., 2023) using another aerosol-climate model (IPSL), tree rings and documentary evidence of lunar eclipses to constrain the age of the Samalas eruption (May to August 1257 is their most likely age).

L407: A stronger simulated cooling in the southern hemisphere (SH) must be expected since your model favors aerosol transport to the SH. With the absence of abundant well dated climate proxies for the SH (apart from ice cores in Antarctica) it will remain impossible to resolve the model/proxy disagreement previously discussed for the SH (Neukom et al., 2014).

L410-411: Since you invoke a potential connection between potential post-volcanic climate change and migration in the 13th century you may want to expand this to include two more large volcanic eruptions during this period in 1269 and 1275 both suspected to be located in the SH.

L432: Please consider removing this idea put forward on chronological errors in tree-ring chronologies or provide the right balance by also citing the numerous studies that have univocally rejected this speculation on globally missing tree rings (Anchukaitis et al., 2012; Buntgen et al., 2014; Esper et al., 2013; St. George et al., 2013).

L438: Stoffel et al. (2015) have reconstructed temperatures for the past 1,500 years and largely focused on the Samalas 1257 and Tambora 1815 eruptions. Other studies have extended the focus to the Common Era (Büntgen et al., 2020; Büntgen et al., 2016; Sigl et al., 2015)

L459/462: Annual resolution alone is not important; annual dating precision is the key requirement of a proxy in order to analyze volcanic climate impacts.

L466-470: A strong asymmetric distribution of sulfate is however not supported by the numerous ice-core records which show quite comparable magnitudes in deposition following Samalas and other known tropical eruptions (e.g. Tambora, Cosiguina, Krakatau) independent of their eruption season. (Table 1) How consistent is this interhemispheric asymmetry for different seasons across aerosol-climate models?

Table1: Ice-core indicated parameters of volcanic sulfate deposition for known tropical eruptions

Event	Krakatau, 6°S	Cosigüina, 13°N	Tambora, 8°S	Samalas, 8°S	Source
Eruption Date	Aug 1883	Jan 1835	Apr 1815	Jul(?) 1257	
Sulfate Greenland [kg km^{-2}]	18	19	38	105	Toohey & Sigl 2017
Sulfate Antarctica [kg km^{-2}]	10	10	46	73	Toohey & Sigl 2017
Ratio Greenland/Antarctica	1.7	2.0	0.8	1.4	Toohey & Sigl 2017
Start SO4 (NEEM)	1883.6	1835.7	1815.6	1258.2	Sigl et al., 2013
Start SO4 (NGRIP)	1883.5	1835.3	1816.1	1258.1	Plummer et al., 2012
Start SO4 (D4)	1884.0	1835.0	1815.5	N/A	(McConnell et al., 2007) ; Marshall et al., 2018
Start SO4 (WDC06A)	1884.0	1834.7	1815.4	1257.9	Sigl et al., 2013
Start SO4 (LawDome)	1884.5	1836.7	1815.8	1257.4	Plummer et al., 2012
Start SO4 (DIV2010)	1884.0	1835.8	1815.8	N/A	(Sigl et al., 2014); Marshall et al., 2018
Peak SO4 (NEEM)	1884.3	1836.0	1816.3	1259.0	Sigl et al., 2013
Peak SO4 (NGRIP)	1885.0	1836.2	1817.0	1259.4	Plummer et al., 2012
Peak SO4 (D4)	1885.0	1836.2	1816.3	N/A	McConnell et al., 2007; Marshall et al., 2018
Peak SO4 (WDC06A)	1885.0	1836.7	1816.5	1258.9	Sigl et al., 2013
Peak SO4 (LawDome)	1885.0	1836.9	1817.0	1258.3	(Jong et al., 2022)
Peak SO4 (DIV2010)	1885.0	1836.9	1817.0	N/A	Sigl et al., 2014; Marshall et al., 2018
Dec. Date Eruption	1883.6	1835.0	1815.3	1257.5	
mean delta t (Greenland) [months]	1.1	3.7	5.5	7.3	Start of SO4 deposition minus eruption date
mean delta t (Antarctica) [months]	6.7	8.3	4.5	1.3	

L 475: High temporal resolution is readily available for ice core records and volcanic eruptions located close-by or upwind of the ice sheets are already dated to the season using these ice cores (Veidivötn 1476/77, Paektu 946/47, Eldgja spring 939, Churchill 852/53, Okmok II 44/43 BCE; (Abbott et al., 2021; Mackay et al., 2022; McConnell et al., 2020; Oppenheimer et al., 2018)). The main limitation to estimate the timing of tropical eruptions stems from the limited number of observations from known tropical eruptions (see Table 1), variability in the transport time (1-8 months), and the disagreement in aerosol-climate models regarding stratospheric aerosol transport times between the tropics and the polar regions (see Marshall et al., 2018; Clyne et al. 2021).

L491-496: You may want to discuss here not only the potential but also the risks of using the agreement of model simulation output and proxies to constrain source parameters used as original forcing input; as one might easily run into a conformation bias, where one tends to favor solutions that best agree with our expectations.

The widely rejected "missing tree-ring hypothesis" (Mann et al., 2012) had its origin in a seemingly mismatch between climate simulations and proxy reconstructions which had subsequently been resolved (see PAGES 2k Consortium 2019, Nature Geoscience) by improved estimates of volcanic forcing (Toohey & Sigl 2007) and better climate reconstructions with inclusions of more tree-ring MXD records (Stoffel et al., 2015; Schneider et al. 2015; Wilson et al., 2016; Anchukaitis et al. 2017). In a similar way, the accuracy of ice-core dating has also been challenged because of an apparent mismatch between strong tree-ring indicated cooling in the high-latitudes of the NH in 1453, and strong global volcanic forcing in 1458 (Esper et al., 2017). This hypothesis has also been rejected by recognizing evidence for two major eruptions 5 years apart and constraining the date of the larger signal in Greenland using tephra from an historic eruption in Iceland occurring in 1477 (Abbott et al., 2021). Both examples highlight the risk of using model-data agreement as the only diagnostic tracer for the correctness of the underlying records.

L498-503: I would omit or rephrase this section. The VSSI as reconstructed by ice cores is free of any climate response constraints, unlike for example previous reconstructions that had tuned the dates of volcanic forcing reconstructions (i.e. in 1453) to tree-ring indicated cooling extremes (Gao et al., 2008; Gao et al., 2006). Moreover, since the VSSI is in Toohey & Sigl (2017) based on 17 individual ice cores from both hemispheres a bias in the magnitude is much less an issue as for reconstructions that are based on single ice cores (Bader et al., 2020; Kobashi et al., 2017; Zielinski et al., 1994). Sampling stratospheric sulfate deposition in ice cores from both hemispheres simultaneously, is ideal for obtaining representative estimates of magnitude despite possible asymmetric sulfate distribution. To my knowledge, there are no known well dated large volcanic eruptions from the tropics which would not have been recognized as such in the polar ice-core records.

If in the future, existing disagreements in aerosol-climate models regarding the global spread of sulfate aerosols can be resolved it may eventually become possible to further constrain together with the existing high-resolution ice core records important source parameters such as the eruption season, latitude and/or plume heights. In any case it appears necessary to cite key studies analyzing the fate of stratospheric sulfur following major eruptions across the models (Clyne et al., 2021; Marshall et al., 2018; Quaglia et al., 2023).

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