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Reply on RC3

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Author comment on "Uncertainties in the atmospheric loading to ice-sheet deposition for volcanic aerosols and implications for forcing reconstruction" by Ya Gao and Chaochao Gao, Clim. Past Discuss., https://doi.org/10.5194/cp-2021-123-AC3, 2022

Reviewer #3□

- L10: "CMIP5 and CMIP6 volcanic forcing" is ambiguous, since there were different forcings used in those 2 projects and within each project, for example for simulations of the historical era and on paleo timescales. More specificity is required.

Response: *The sentence has been changed to "This study revisits the Pinatubo-based LTD applied in the last Millennium volcanic forcing reconstruction that has been untilzed in the CMIP5 and CMIP6 simulations."*

- I do not believe the Monte Carlo procedure used herein constitutes a "model". A model is a mathematical framework that describes a system.

Response: *We agree and the reference to Monte Carlo has been changed to "Monte Carlo random sampling method/procedure".*

- L24: the second part of this sentence ("limitations in the conversion factor") is a tautology, needs more explanation.

"This inverse reconstruction involves substantial uncertainties, due to the discrepancy in ice core volcanic deposition measurements and perhaps more importantly, the limitations in the conversion factor to transfer the ice core observation into the stratospheric volcanic sulfate loading."

- L36: Reference needed for Pinatubo aerosol loading estimate

Response: The two references for the original 15 Tg Southern Hemispheric sulfate aerosol loading estimate (listed below) have been added in the revision.

Bluth, G. J. S., C. C. Schnetzler, A. J. Krueger, and L. S. Walter (1993), The contribution of explosive volcanism to global atmospheric sulphur dioxide concentrations, Nature, 366, 327 - 329.

Krueger, A. J., L. S. Walter, P. K. Bhartia, C. C. Schnetzler, N. A. Krotkov, I. Sprod, and G. J. S. Bluth (1995), Volcanic sulfur dioxide measurements from the total ozone mapping spectrometer instruments, J. Geophys. Res., 100, 14057 - 14076.

- L41: Again, need to be specific about the CMIP experiments which use this information.

Response: *The sentence has been changed to "the conversion factors have been utilized in the ice-core-based last millennium (850–1850CE) volcanic forcing reconstruction of Gao et al. (2008) and Toohey and Sigl (2017), which has been widely used in the CMIP5 and CMIP6 model simulations, respectively."*

- L52: "overlooked" is too strong, this is largely the motivation for the Toohey et al. (2013) and Marshall et al. (2018) studies.

Response: *The word has been changed to "occasionally studied by Gao et al. (2007) using mainly observation data, by Toohey et al. (2013) and Marshall et al. (2018) using model simulations"*.

- L52: What is meant by "high depth resolution"?

Response: *We meant to say ice cores with "long term high resolution volcanic sulfate record". This ambiguity has been clarified in the revision.*

- L68: L cannot be either the mass of SO_2 or sulfate aerosols, of course you will get very different answers if you use one or the other (without some sort of conversion).

Response: *We thank the reviewer for pointing out the possible confusion. The unit has been limited to the one used in this study as copied "where L is the stratospheric volcanic mass loading (in Tg of sulfate aerosols)"*

- L78: This sentence says that the authors have performed an extraction of the volcanic sulfate flux from these two ice cores independent of the work of Sigl et al. (2015). It would be helpful then to briefly present how the flux values the authors compute compare to those of Sigl et al. (2015).

Response: *Sigl et al. (2015) applied essentially the same methodology as Gao et al. (2007) therefore this study too, except for a stricter signal extraction threshold, i.e.,*

31yr trend + 3×MAD in Sigl et al. (2015) vs. 31yr trend + 2×MAD in this study. We compared the magnitudes of the Tambora depositions in NEEM2011S1 and Tunu2013 using the two thresholds and found no difference. A brief comparison of the flux values has been added in the revision.

- L86: Not quite clear if here again the authors have performed an independent estimation of the volcanic sulfate flux for these Antarctic cores? If so, a comparison with Sigl et al., (2015) would be quite useful.

Response: *Yes, we performed the same independent estimation for these Antarctic cores. This has been described more clear in the revision, and a brief comparison of the flux values with Sigl et al., (2015) has also been added as copied below:*

"Sigl et al. (2015) applied essentially the same methodology as Gao et al. (2007) except for a stricter signal extraction threshold, i.e., three times the 31-year MAD instead of twice the MAD as Gao et al. (2007) and therefore also in this study. We compared the magnitudes of the Tambora, Pinatubo, and Agung volcanic depositions in these additional ice core records using the two thresholds and found almost no difference, except for NUS07-2 and NUS07-7 where the 3MAD threshold results in lightly smaller signals."

- L91: How comparable the magnitudes of sulfur emission from Pinatubo and Agung are is arguable. In any case, does it really matter if they are, and does it matter that Agung is at a similar latitude as Tambora? If so, why?

Response: *In this section, we are trying to discuss the possible impacts of eruption magnitude and latitudinal locations on the conversion factor, with the very limited number of volcanic eruptions with observational information. When we say "they are the two recent events with comparable magnitudes" we are thinking about the order of magnitude as they are both smaller than Tambora (let alone Samala) but larger than the small eruptions of the later decades. Agung is at a similar latitude as Tambora but significantly smaller in magnitude, therefore may provide a case to detangle the influence of location vs. magnitude.*

Nevertheless, in reality the locations and magnitudes of Tambora, Agung, and Pinatubo do not appear to matter much, given the other sources of influence (for example, the hemispheric partitioning) involved in the LTD. So we limit the referencing to these two events as "We also calculate the LTD factor of the 1963 Agung and 1991 Pinatubo eruptions, because they are the two recent tropical events with moderate magnitudes and some available observations."

- L112: Where does the 60-80 Tg SO_2 range come from? Self et al. (2004) quotes 53-58 Tg SO_2, and Gertisser et al. (2012) don't seem to provide any independent estimate.

Response: *The 60-80 Tg SO_2 range was actually borrowed from Marshall et al. (2018), because we are interested in compare our ice-core based results with those from the multi-model estimations. Thanks to the reviewer's question, we realize there is ambiguity in our description of the eruption and how we choose the size values. Therefore, in the revision we have modified the description as:*

"The April 1815 eruption of Tambora (8.25° S, 118.00° E; Figure 5) is one of the largest explosive eruptions in the Common Era (Self et al., 2004; Stoffel et al., 2015) and also the most widely studied eruption in terms of ice core observation, model simulation, proxy reconstruction, and climatic and socioecological aftermaths (Luterbacher and Pfister, 2015; Raibie et al., 2016; Gao et al., 2017; Brönnimann et al., 2019)."

"Self et al. (2004) estimated a total of 53-58 Tg of SO_2 were released into the

stratosphere by the Tambora eruption in 1815. Marshall et al. (2018) adopted the best estimate of 60 Tg SO_2 with a possible range of 30-80 Tg SO_2 in their multi-model simulations, after combining petrological, ice core, and aerosol process model estimations (Self et al., 2004; Gao et al., 2008; Stoffel et al., 2015). In order to better compare our results with the multimodel simulations, we take the best estimation of Tambora eruption size of 60 Tg SO_2as the total amount of sulfate gases injected into the stratosphere and divide the values equally into each hemisphere."

- L125: There's a logical problem here. If the flux to Antarctica and Greenland is similar, to assume this means the aerosol partitioning is symmetric assumes a similar LTD factor for the two ice sheets. But then you use the assumption of even partitioning to calculate the LTD factor for the two ice sheets. This is circular.

Response: *The referencing to similar deposition flux in Antarctica and Greenland has been removed.*

- L145: The results stated here seem rather obvious results of the resampling procedure, but miss the point that the width of the distributions increases for smaller sample sizes, meaning that the uncertainty of a single sample (of some few ice cores) increases as n decreases. That the SD of the LTD decreases as 1/sqrt(n) simply confirms that the typical standard error of the mean (SD/sqrt(n)) is a suitable assumption for the data, but it is not clear if this has any physical meaning or utility for the present purposes.

Response: Taking the reviewer's comments, we have emphasized the result that the width of the distributions increases for smaller sample sizes and how it relates to the uncertainty of the LTD estimation. The implication for the present purposes is also addressed by comparing the Monte Carlo sampling results for the number of cores available to representative periods of IVI2 or Sigl 2015 reconstructions and those for Tambora or Pinatubo deposition. Revisions addressing the two issues are copied below:

"The results show that, the distributions of LTDT with different ice-core sample sizes are approximately normal, and the width of the distributions increases for smaller sample sizes. This means that the uncertainty associated with a single sample increases as the number of ice core samples decreases."

Secondly, the standard deviation of LTDT decreases with 1/sqrt (N), confirming that the typical standard error of the mean (SD/sqrt(n)) is a suitable assumption for the data. In Figure 4 we also plot the standard deviation of the conversion factor if only the number of ice-core available to representative IVI2 or Sigl 2015 reconstructions were used solely, from which we can see significant reduction of the uncertainties as the number of ice core records increases to Pinatubo or Tambora level.

- L150: To say that "the precision of LTD values is related to the limit in the number of cores" is trivial, this is basic statistics. If you want to say "the precision of LTD values is ONLY related to the limit in the number of cores" then I would argue this is incorrect, because it depends on the random error of each ice core (from measurement noise or other factors), which is likely quite variable between different ice cores. If an ice core is particularly noisy, then adding it to the composite may increase the overall uncertainty. This analysis tells us nothing about the potentially very different errors of the individual ice cores.

Response: *The sentence has been removed. Instead, we focus the discussion on the* *changing nature of the standard deviation w.r.t the number of sampled ice cores. We also added in Figure 4 the Monte Carlo sampling results for the number of cores available to representative periods of IVI2 or Sigl 2015 reconstructions, so the discussion is more meaningful for the present purposes.*

- L151: The convention of quoting precision is unclear to me, I am used to percent precision for a value which is x+/-y as 100*y/x, so a smaller percent precision means more precise. That doesn't seem to be the case here.

Response: *The quoting precision here and those in Table 3 referring to the percentages of sampled number of ice core records with respect to the total available number of ice core records for Tambora. For example, 65% here is obtained by dividing 22 (the number of records sampled in this particular draw of Monte Carlo procedure) with 33 (total number of Antarctic ice core records). Therefore, a larger percent number means a higher number of available ice core records.*

However, we feel from the reviewer's comment that this may induce confusion, and the sentence does not deliver much valuable information. Therefore, it is deleted in the revision. Explanation of these values in Table 3 has been added to the caption.

- L182: I think your argument here has to do with aerosol particle size distribution, but this needs to be explained more clearly.

Response: *Yes, it is related to the aerosol particle size distribution among the three eruptions. Specific explanation and related references have been added in the revision.*

- L193: "A series of…"?

Response: *Changed to "Series of nuclear bomb tests"*

- L216: If BTD is the same thing as LTD, please use the same name for it.

Response: *We prefer to keep BTD, because it can be easily differentiated from the ice-core based conversion factors and easily referenced back to its original source. If we change to LTD, then we have to add sub-notations to differentiate it from the ice-core based LTD, and we are afraid the sub-notations will be confused with the sub-notation of volcanic eruptions.*

- L267: This last statement makes no sense. If you use the LTD derived from Tambora on the ice core values for Tambora, you will of course get a loading estimate that is equal to the loading you used to calculate the LTD! You might as well just use the original loading estimate.

Response: We agree with the comment and have revised the last statement as

“The results obtained from this study is a step forward to bring the conversion-induced uncertainty into the reconstruction framework, which hopefully also build a baseline for updating and improving the conversion of volcanic icecap-deposition to atmospheric-loading.”

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-123/cp-2021-123-AC3-supplement.pdf