

We thank the reviewer for their very supportive comment and for the suggestions given to improve our submission.

Reviewer comments in blue italic, our response in black normal.

Line 381: The eruption latitude vs. polar asymmetry relationship derived from ice core data is slightly different than the modeled eruption latitude vs. polar asymmetry equation. The authors explain this as caused by the overly strong stratospheric transport toward the NH in UM-UKCA. This (or in the conclusions) could be a good opportunity to call for future research, e.g. a modeled latitude vs. polar asymmetry equation using a different model. An uncertainty of ~40 ° latitude seems important. Could the authors comment on how this contributes to forcing uncertainty from this dataset?

This is a good point on model; we believe the conclusion should focus on more important messages, but added the following statement in section IV.2: “Assessing the deposition asymmetry-eruption latitude relationship simulated by other models could give greater confidence in Equation 3 whilst being a test of model skill at simulating stratospheric aerosol processes.”.

Forcing uncertainty is yet to be quantified and the effect of eruption latitude is complex as it will contribute to difference in forcing spatial distribution (in addition to the timescale and magnitude of global mean forcing) which would in turn affect simulated climate response. To acknowledge more explicitly the role of latitude uncertainty, the last sentence of section VI.2 on key uncertainties now read “The creation of an uncertainty product for the CMIP7 volcanic emission inventory will be key to documenting these uncertainties in detail and quantifying how they propagate to aerosol optical properties, including uncertainties on volcanic injection mass (sections III and IV.5), latitude (section IV.2) and height (section IV.4).”

Line 423: Assuming an eruption date of January 1 may not be the best choice of arbitrary date. For example, I believe Gao et al. (2008) assumed April 1 to choose the most “neutral” seasonality possible.

Although discussions exist on what the best arbitrary date, it is clear that a date early in the year is most reasonable given typical delay of 6 months between tropical eruptions and the beginning of deposition across polar ice sheets (e.g. see Marshall et al., 2018 on the example of Tambora 1815). Some of the past eruptions may have occurred in the year before sulfur rises in the ice-core records, and January 1st is a reasonable arbitrary date. Most importantly, the manuscript acknowledges important uncertainties on dates, including +/-1 year uncertainty for ice-core dating.

Line 450: say what the “Expected values” of 0.25-0.33 are based on (theoretical modeling? I am not familiar with these papers)

Yes this comes from theoretical modelling, we now made this more explicit: “The fact that the exponent of 0.12 is much smaller than values expected from analytical modelling of idealized buoyant plumes rising in absent or strong wind field (0.25-0.33, ...).”

Line 482 “We instead choose a SO₂ mass enabling to match...” this phrasing doesn’t make sense

We improved the phrasing of this sentence as follow: “Consequently, we instead choose a default SO₂ mass enabling to match: i) the time-averaged volcanic SAOD associated with small-to-moderate magnitude eruptions in the pre-satellite era; and ii) the satellite-observed time-averaged volcanic SAOD over 1998-2023, during which only eruptions of VEI ≤ 5 occurred and with a SO₂ mass ≤ 2 Tg SO₂.”

Figure 7 is nice. I wonder what is the “climate impact” of the missing small-to-moderate eruptions from before the satellite era (figure 7c)? For example, if you estimate anthropogenic aerosol ERF by comparing a piClim-control and piClim-aer (Collins et al., 2017) with and without small-to-moderate eruptions, how different is the ERF? If this has not yet been estimated, should this be suggested as future research?

This is a good suggestion and we made an explicit recommendation at the end of section VI.2: “Last, future studies should quantify the radiative forcing and climate impacts associated with differing small-moderate magnitude eruption representation between CMIP6 and CMIP7 (v2.2.1), and between CMIP7 (v2.2.1) and future dataset versions with further reduction of biases discussed in this section.”

Line 600: requesting that modelers document these parameters is an important point. Could you also state this point in the abstract, summary, and/or maybe note it in a dataset read me file? Consider also adding the suggestion from line 802 into the summary

Excellent point. We added the following sentence both in the abstract and conclusion: “Modelling groups using the dataset should document in detail any deviation from provided parameter values and recommended emission implementation.”

On our suggestion from previous line 802, we added the following sentence to the conclusion: “We also discuss ideal requirements for these source datasets in terms of provided data and documentation.”

Line 686: should say “variability” (currently “variabilti”)

Good catch, corrected.

Summary: this was a helpful section, but should be split into multiple paragraphs to make it more readable.

Excellent suggestion, we split it into three paragraphs.