GMD 2nd post-review TE review of "Simulating volcanic emissions in MESSy – New submodel for Explosive Volcanic ERuptions (EVER v1.1)" by Matthias Kohl, Christoph Bruehl, Jennifer Shallock et al.)

We thank the editor for the additional review. We went through the comments carefully, and applied them in the manuscript. Below, we report the editor's comments (grey, bold) along with our replies (blue). We only report comments, where we add further explanation or (slightly) deviate from the editor's recommendations. All comments not listed below were implemented precisely as suggested.

Further minor revisions (line numbers from Track Changes MS)

1) Lines 582-583, and follow-on paragraphs of discussion on lines 584-600.

I don't get the point being made on lines 582-583, the manuscript arguing there that emissions constrained to a single gridbox tend to "potentially result in localized and exaggerated mixing ratios of SO2".

I can see there is a follow-on point referring to "when volcanic plumes span areas exceeding one gridbox", but the logic here was unclear to me.

I'd argue that point sources tend more to more likely be under-representing the local (to the volcano) SO2 mixing ratio, within grid-box average values.

For coarse-spatial-scale global models, there is the issue of whether the model can represent regimes where a depletion of oxidants can affect predicted sulphate production (for example), and more generally variations between the concentrated emitted species (at the plume-scale) viz-a-viz the larger-scale volcanic enhancement in the model, that determining the magnitude predicted impacts.

There is quite a good depth of Discussion already within section 5, and suggest to re-word the main points here into just 1 paragraph, rather than the 3 paragraphs here on lines 584 to 600.

We wanted to refer to the following scenario here: Stratospheric plumes of strong volcanic eruptions can cover several horizontal columns in reality, e.g. Hunga Tonga (2022). When confined to one column, this may lead to overestimated local SO_2 mixing ratios and non-linearities. We agree, that there is also the effect of underestimating local SO_2 mixing ratios when averaged over grid boxes, however, this cannot be solved within fixed horizontal resolution. We combined everything into one paragraph, and omitted the addressed formulation to avoid misunderstandings:

" However, column or point emissions come with inherent limitations as well. First, emissions are constrained to a single grid box or columns of grid boxes in this study. In detailed studies of strong eruptions, we additionally recommend exploring the effects of emissions over multiple columns and an extended time period to avoid non-linearities due to very high local concentrations (see also Sect. ...). Second, volcanic activity typically extends beyond a single day, with SO_2 emissions occurring over prolonged periods, occasionally reaching the stratosphere

(see Sect. ... for the Nabro eruption). While related discrepancies dissipated in the mid-term for the Nabro volcano, this may not be necessarily the case for other volcanic eruptions. Third, the exact timing and geographical location of the stratospheric entry point cannot always be accurately estimated. Our sensitivity analyses uncovered short- and mid-term disparities when such information is lacking. "

2) Lines 322-325 – Please revise this new sentence added here "shall not evaluate the microphysics". Further to the main comments above, since the topic of this manuscript is re: the emissions sub-model, there should be no expectation of requiring to validate the simulated aerosol. What I suggest here, is to refer to a "self-consistency check" for the model, in the sense that global models may tend to focus mainly on evaluating predicted aerosol, and may be less concerned with accurately predicting volcanic SO2. I realise that's perhaps what you mean by "additional evaluation", but since this study is not a model evaluation paper (in that sense) better to be clear this is more a case of analysing the aerosol predicted from the validated SO2, than it is validating the predicted aerosol.

Perhaps that is what you meant by "evaluation", but again, given reviewer 2's comments, suggest to word this more towards a "self-consistency check" for predicted aerosol (or similar alternative label than evaluation or validation).

We added this disclaimer as a response to reviewer 1, who was expecting a more proper evaluation and study of the model's microphysics. We agree with the editor, that this should not be expected here. We reformulated to:

" The additional evaluation of the simulated aerosol optical properties serves as a self-consistency check for the model, for context within the climate impacts of the resulting stratospheric aerosol, and as a comparison to the simulation done by Schallock et al. (2023), who used a similar setup. "

3) Lines 204 to 209 – Need to explain re: the default emissions-altitude, where IASI heights are used

The revised manuscript now refers to "with the same vertical distribution", but it's actually not been explained what reference-altitude is given in these cases where IASI can't observe the plume-top height.

From checking the file in the Supplement, I see the default emissions-altitude is 15km, but this is not stated currently within the manuscript (to my reading). Please add "at 15km" at the end of line 205, and also re-wording the earlier part of the revised sentence (re: IASI and "or those not observed") as this is still slightly unclear, and should be worded more concisely.

Suggest "For eruptions before 2007, or other volcanic SO2 plumes not observable from IASI, we distribute the column SO2 at 15km, with the Gaussian x-km vertical dispersion." (or similar).

Re: the latter, the text "all injections optimized using the IASI observations are marked accordingly" needs to give an indication of how many eruptions are where this further optimization from IASI. You can also delete "on the date provided by the emissions inventory from Schallock et al. (2023)", as that's implicit.

Also, again from checking the "ever_historic_stratVolcanoes" namelist, I saw the text-flag "IASI optimised" is given for eruptions 544 to 548 (2019 Raikoke), 550 (2019 Ulawun) and 568 (2020 Taal), and then please also add to note "for example 2019 Raikoke, 2019 Ulawun, 2020 Taal" where a revised observed plume-height has been found from the original inventory (Schallock et al., 2023).

We do not use the IASI top plume altitude here, and we also do not use a default altitude of 15 km. In line 200-202, we specify the vertical distribution and altitude we use:

"The SO_2 mass is then distributed vertically in a Gaussian profile centred 1 km below the maximum altitude (sigma of 2 km, confined to the vertical extent of the maximum plume altitude down 2 km, truncating the Gaussian distribution at $\sigma/2$) recorded in the emission inventory, over 6 hours around the identified date and time of peak mixing ratio as default."

and before:

"From this analysis, we extract the space-time point exhibiting the maximum stratospheric SO_2 mixing ratios observed by IASI as the optimal estimate for both, timing and geographical location for injecting the plume into the stratosphere."

We only use the (horizontal) geographical location and timing from IASI, and the plume altitude from the emission inventory from Schallock et al. (2023). We apologize that this was misleading, as we did not specify "horizontal" explicitly. We slightly reformulated to clarify:

" ... From this analysis, we extract the horizontal space-time point exhibiting the maximum stratospheric SO_2 mixing ratios observed by IASI as the optimal estimate for both, timing and geographical location for injecting the plume into the stratosphere.

The SO_2 mass is then distributed vertically in a Gaussian profile centred 1 km below the maximum altitude (sigma of 2 km, confined to the vertical extent of the maximum plume altitude down 2 km, truncating the Gaussian distribution at $\sigma/2$) provided in the emission inventory from Schallock et al. (2023), at the horizontal geographical location derived from IASI observations, over 6 hours around the identified date and time of peak mixing ratio. ... "

Regarding the namelist in the supplement, we reformulated as follows:

"The 54 strong injections with optimized horizontal geographical location and timing of the stratospheric entry point (e. g. Raikoke 2019, Ulawun 2019, Taal 2020) are marked accordingly."

9) Lines 265-267 – The wording here needs reducing: "To comprehensively evaluate volcanic SO2 emissions with the EVER submodel and the impact of simplifications and adjustments to emissions data, and we conducted a reference simulations along with a series of sensitivity simulations of stratospheric SO2".

Suggest to simplify to "For the Nabro eruption, we carried out a series of EMAC simulations to assess the sensitivity to difference emissions source parameters".

Putting "Nabro" earlier in the sentence so the reader can realise these sensitivity runs are for this one particular eruption.

The subject of the manuscript is EVER, so that doesn't need to be stated, but

since the model is not mentioned at any point in section 3.2.1, do need to specify EMAC there.

The "We applied a horizontal resolution..." needs to be impersonal tense, to "These simulations applied a horizontal resolution..." or similar.

10) Line 267 — Change "We applied a horizontal resolution of T63..." to "For the stratospheric injecting case, the simulations were at T63 horizontal resolution

Depending on how change 8 specifies the model resolutions in section 3.2, this sentence could be reduced, but actually I think good here to re-iterate as many readers may go straight to read this subsection.

We combined both comments and reformulated to:

"For the Nabro eruption, we carried out a series of EMAC simulations to assess the sensitivity of stratospheric SO_2 burdens to varying emissions source parameters. The simulations are performed at T63 horizontal resolution (approx. 190×190 km at the equator), with 90 vertical levels up to 0.01 hPa and a model timestep of 8 minutes."

11) Line 270 — The text here says "MIM1" but shoudn't this be simply "MIM"?

We actually mean MIM1 here. This is the version 1 of MIM (we state this now clearly in the manuscript) based on Jöckel et al. (2016) and Pöschl et al. (2000). There is also a version MIM2 (Taraborrelli et al., 2009). For that reason, the differentiation is necessary. However, we switched the citation from Jöckel et al. (2006) to Jöckel et al. (2016), as it is explicitly called MIM1 there as well (including a comprehensive evaluation).

13) Line 305 — Change "fixed lower integration limits" to "fixed lower plume-altitude"

We actually do not refer to the lower plume-altitude here. The plume altitude distribution is given above. The lower integration limit is only used her for the calculation of stratospheric SO_2 burden and sAOD, as lower altitude, from which we integrate/sum over the SO_2 mass and the aerosol extinction. This is only the definition of the SO_2 burden and sAOD here. We slightly adjusted the wording in the revised manuscript to clarify this.

18) Lines 337 – Change "In addition to the evaluation of explosive volcanic eruptions..." (remember it's not the eruptions being evaluated here) to "As well as evaluating emissions from explosive eruptions..." .

And then delete "following" later in the sentence so it's focused to "analysing a series of"

To avoid repetition of "evaluate", we reformulated to:

"In addition to emissions from explosive volcanic eruptions, we evaluate the new submodel's capability to simulate emissions of degassing volcanoes by analysing a series of eruptive fissures ..."

20) Line 345 – change "reducing the high computational cost due to the increased horizontal resolution and decreased timestep" to "with a decreased timestep to reduce the computational cost at this increased horizontal resolution".

We actually had to decrease the timestep to 2.5 minutes to account for the Courant-Friedrichs-Lewy criterion, and this additionally increases the computational cost. We reformulated the paragraph in combination with the previous comment to:

" Simulations for the degassing case are performed at a horizontal resolution of T255 (approximately 50 × 50 km at the equator) and 31 model levels (up to an altitude of about 30 km) to capture the tropospheric transport. For these simulations, we use a simpler chemical mechanism (compared to the T63 simulations) in the global model covering the basic tropospheric chemistry (as we do not focus on the stratosphere here), including O_3, OH, NOx, NOy and basic sulfur chemistry (see supplement for details), to reduce the computational cost at this increased horizontal resolution and decreased timestep of 2.5 minutes. ... "

22) Lines 362-363 – Add a full-stop after "simulated SO2 columns" and resume with a new sentence "For each day in...", then being able to add "(the first month post-eruption)" to clarify why June 2018. Also replace "and subsequently use this linear relationship to derive" with "a linear relationship is used to derive".

Actually, putting a full stop here would change the meaning of the sentence. The fit is performed for data at each day in June 2018, so we changed "for each day" to "at each day" to make this clearer. June 2018 is chosen as the emissions were most intense in that month. We added "(the month of most intense SO_2 emissions)" here.

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