Review for «New submodel for emissions from Explosive Volcanic ERuptions (EVER v1.1) within the Modular Earth Submodel System (MESSy, version 2.55.1)» by Kohl et al.

The submitted manuscript contains the description of a new submodule within MESSy. The submodule is described in detail in section 2. Section 3 provides a description of the model setup for the validation of the submodel. For model validation the 2011 Nabro and 2018 Kilauea SO_2 emission plumes were evaluated with different sensitivity simulations and with a fine-resolution simulation, respectively. In chapter 4 the explosive volcanic SO_2 injections of the time period 2007 to 2011 was simulated based on a 3-D observational emission dataset which was improved based on the findings of chapter 3.

The submitted manuscript describes a new submodule for emission of volcanic substances and provides some validation for the evolution of SO_2 plumes after volcanic eruptions. However, in my point of view, the manuscript has some substantial flaws, which I explain below and which need to be addressed. Even though topic wise the manuscript would be a good fit for publication in GMD, I do not think that quality wise this manuscript deserves publication in this journal.

The main environmental impact of large explosive volcanic eruption on a global scale (heterogeneous chemistry, cooling of the surface, local warming of the stratosphere) are effects from sulfuric acid aerosols and not from SO_2. The manuscript almost entirely focuses on the simulation of SO_2 plumes and thus, a crucial aspect of volcanic emission plumes is missing. I know that for the accurate simulation of aerosol burden and their effects it is important to accurately simulate the SO_2 plume in the first place, however, if this was the goal of this study this needs to be motivated and explained in the introduction.

To me many important aspects of a paper are not sufficiently explained: What is the novelty of this study? What can the model do what other models can't do? Why is it important to simulate the emissions of SO_2 accurately? Why do we need yet another model which is capable of simulating volcanic injections? What have other models shown and how does your model compare to other models?

I think the manuscript does not provide/discuss a substantial part of literature relevant for the simulation of volcanic emission plumes. There have been many studies, which highlight important aspects discussed in this manuscript, which are not even cited (e.g. Brodovsky et al. 2021 or Quaglia et al. 2023). Usually, the 1991 Mt. Pinatubo eruption is used as a basis for model validation (e.g., Quaglia et al. 2023, which even includes EMAC simulations). Why did you not perform a simulation of Mt. Pinatubo to validate your model? Probably because this has been done extensively already, but it would allow comparing also with other models.

The introduction could focus more on topics relevant to the rest of the manuscript. E.g. why do you explain ozone chemistry and ash in such detail if it is not relevant to the manuscript? The manuscript provides a very broad discussion about the general impact of volcanic eruptions on climate and atmospheric chemistry, but it does not discuss the problems and current limitations when modeling volcanic eruptions (e.g., spatial & temporal resolution, model agreement/disagreement of past model studies among each other and with observations, sectional versus modal microphysics modules, previous studies which addressed the vertical/horizontal distribution of volcanic emission plumes). Since you submit to "Geoscientific model development" I think a more technical motivation/introduction would be appropriate. I think the introduction should clearly motivate the research so that it is clear to a reader why the submodule was developed and why there is need for the research provided. In my point of view this is not the case.

Your conclusions just summarize what you have done in your work, including a brief recapitulation of some qualitative results with a brief outlook for further potential applications of the sub-model in the end. However, I think the conclusion section should summarize the key findings of the paper (also some quantitative statements, not only qualitative) and put them into a broader context. The conclusions should demonstrate the importance of the paper and convey the larger implications of the paper to the research field. Additionally, I also find it important to address the limitations of the research/model provided (or more specifically: of the presented sub-module) and highlight aspects identified in your work which require further research and development. I don't think these points are provided in the current version of the conclusion section of the manuscript.

Similarly, the abstract just summarizes what you did in this study, but you do not provide any results or conclusions or broader implications to the research fields in the abstract, which I think is an essential part of the abstract.

An important aspect which is missing in the analysis and discussion of your results is the role of chemical loss of SO_2. In addition to dilution and transport, a (maybe even more) important aspect which determines the SO_2 lifetime is chemical SO_2 loss (i.e., mainly oxidation with OH and O3). How is this represented in MECCA, and how does this influence your modelled results. You also write that for the Kilauea study, you applied "simplified chemistry". What does this mean? Since chemical loss is very important for the SO_2 lifetime you need to discuss how this affects the results in your study. How is the reaction with O3 and OH represented in MECCA? Were the O3 and OH fields also nudged to observations like written in the model description (probably not)? How does this influence the modelled results?

Another aspect which is not discussed is the importance of aerosol microphysics. You show an in-depth analysis of the SO_2 plume evolution and then in chapter 4 you suddenly come up with AOD analysis. However, there is an important step missing in between: Aerosol Microphysics. You write that you are using GMXE as an aerosol microphysics module and represent the aerosols using a modal approach. However, you do not provide any information about resulting aerosol burden, aerosol size distribution ect. You only show AOD and extinction (without indicating the wavelengths under consideration). Thus, the whole aerosol microphysics (which is a very important aspects when simulating volcanic emission plumes) is treated as a black box in this manuscript. In my point of view, it is crucial to also show resulting aerosol burden and compare them with observations as well as which other models (e.g. see Brodowsky et al. 2021).

Another aspect which comes too short in the discussion section is the influence of the spatial and temporal resolution of different processes. It is well known that these aspects are very important for realistic representation of volcanic plumes. While for the troposphere the horizontal resolution is more important in the stratosphere the vertical resolution is more important. For aerosol microphysics the microphysical timestep should be set small enough to realistically simulate nucleation and condensation. You could mention these aspects in the discussion of your results.

I also find the manuscript too long. Many aspects which are discussed in the introduction and submodel description are not relevant to the storyline or are not picked up again in the discussion. I suggest shortening substantially and putting part of the text (e.g. description of code and namelists) into the supplement. Also, the structure could be improved. For example, the model description and setup and description of the observations (sect. 3.1 and sect. 3.2) could be a chapter for its own or part of chapter 2. Because the setup described there is also used in chapter 4. The different events simulated here (i.e., Nabro, Kilauea and the 2007-2011 period) seem a little disconnected to each other. Why don't you show a full analysis of only one event (e.g. Nabro), but in more detail including sulfuric acid aerosol burden ect.

More detailed comments can be found below.

Brodowsky, C., Sukhodolov, T., Feinberg, A., Höpfner, M., Peter, T., Stenke, A., & Rozanov, E. (2021). Modeling the sulfate aerosol evolution after recent moderate volcanic activity, 2008–2012. *Journal of Geophysical Research: Atmospheres*, 126, e2021JD035472. https://doi.org/10.1029/2021JD035472

Quaglia, I., Timmreck, C., Niemeier, U., Visioni, D., Pitari, G., Brodowsky, C., Brühl, C., Dhomse, S. S., Franke, H., Laakso, A., Mann, G. W., Rozanov, E., and Sukhodolov, T.: Interactive stratospheric aerosol models' response to different amounts and altitudes of SO_2 injection during the 1991 Pinatubo eruption, Atmos. Chem. Phys., 23, 921–948, https://doi.org/10.5194/acp-23-921-2023, 2023.

Timmreck, C., Mann, G. W., Aquila, V., Hommel, R., Lee, L. A., Schmidt, A., Brühl, C., Carn, S., Chin, M., Dhomse, S. S., Diehl, T., English, J. M., Mills, M. J., Neely, R., Sheng, J., Toohey, M., and Weisenstein, D.: The Interactive Stratospheric Aerosol Model Intercomparison Project (ISA-MIP): motivation and experimental design, Geosci. Model Dev., 11, 2581–2608, https://doi.org/10.5194/gmd-11-2581-2018, 2018. a, b, c, d

Mills, M. J., Schmidt, A., Easter, R., Solomon, S., Kinnison, D. E., Ghan, S. J., Neely, R. R., Marsh, D. R., Conley, A., Bardeen, C. G., and Gettelman, A.: Global volcanic aerosol properties derived from emissions, 1990–2014, using CESM1 (WACCM), J. Geophys. Res.-Atmos., 121, 2332–2348, https://doi.org/10.1002/2015JD024290, 2016.

Detailed Comments:

Line 1: What is a methodological study? Isn't every scientific study methodological?

Line 14/15: Suggesting to change "solar geoengineering" to "solar radiation modification", a more appropriate term.

General Comment on the abstract: The abstract mostly reflects what was done in this study, but there is no mentioning on results and conclusions, which I think is a key component of an abstract. Thus, I suggest adding some quantitative results and conclusions/broader impacts.

Line 19/20: "On the on hand… on the other hand" is normally used for opposing arguments. The ones mentioned here are more additive. I suggest reformulating. Whether it is "substantial" or not, depends on the magnitude.

Line 25 and lines 32-34: Do you have references for that?

Line 25/26: Do you have a reference for this?: "The composition of volcanic plumes exhibits considerable variability and depends on the intricate mixture of chemical species in the magma"

Line 27-31: I suggest combining the two sentences (i.e., list the example of Hunga Tonga in the first sentence).
Line 32-34: a definition of "long-term" would be good. sulfuric acid aerosols and their precursors are usually removed from the stratosphere within 2 years. I don't think this is long-term in terms of climate. Maybe chlorine species would have a long term effect.

Line 34-36: This is somehow confusing. In the first paragraph you speak of "the most explosive volcanic eruptions" and now you write of emissions per year. Do you still speak of large explosive volcanic eruptions or do these numbers also account for degassing non-volcanic eruptions? I suggest being more precise here to what exactly these numbers refer to.

Line 37/38: I suggest changing the term "sulphate" with "sulfuric acid", since technically speaking, a sulphate is a solid (e.g. CaSO4). Also add references to this statement.

Line 36: Maybe add "under volcanic conditions", otherwise other sulfuric acid precursor gases are also important. I know SO_2 is poisonous, but isn't the effect on acid rain mainly a result of uptake of sulfuric acid aerosols (and not primarily SO_2)?

Line 46: "up to" or "over"? But not both.

Line 81: What is a “horizontal gid box”? What is the difference between a horizontal or vertical grid box? Do you mean “vertical column of gird boxes”?

Line 85: Are you only aiming at providing or are you providing? I suggest reformulating to “We provide …”, or reformulate in another way if you don’t. Same for the last sentence of the paragraph: “This was achieved through the following three steps of work: ”. Don’t undersell your research.

Line 90: “… of vertical emission distributions … “

Line 116: You only use GMXe aerosol microphysics module in this study. Why do you introduce the other two submodules too? This only causes confusion.

Line 127: What do you mean by “linear columns”?

Section 2.1: I think the description of the new submodel is too technical. It is probably not useful for most readers of this study. I suggest making the description of the new sub model more general and provide a more technical description (e.g. how the name list works, what the different name list parameters are) in the supplement.

Section 2.2.1: The title of this section is “Primary emissions”, but the subchapter is specifically on direct aerosol emissions. Maybe specify this in the title.
However, why is this subchapter important? In this manuscript, only SO_2 injections are evaluated. Maybe you can skip this subchapter or put it into the supplement.

Section 2.2.2: This could be picked up later in the paper.

Section 3.1 There is no mentioning of the microphysical, chemical and dynamical time steps applied in the models used in this work. A recent study has highlighted the need for appropriately setting the microphysical time step when simulating volcanic eruptions.

Section 3.2: Maybe this section can be shortened. Are such detailed descriptions of all the different satellite and technical details such as their resolutions required?

Section 3/4: I assume the model description provided in section 3.1 also applies to section 4, right? And some of the described observations (satellites) in 3.2 are only used in section 4, right? To avoid confusion, I suggest separating the model description and observations from chapter 3 and create an own chapter for this. Or maybe something similar, just improve the structure of the paper, it is confusing sometimes.

Line 203-207: Why is this information important at all? You are not looking at aerosols in this chapter, but only at SO_2 plumes. Is SO_2 also treated within GMXe?

Line 200: Nudged to which variables? Wind and temperature?

Line 222: Maybe just write: “Namelist setup, chemical mechanism and runscripts can be found in the supplement.” However, I think this belongs in the data availability statement not in the main text.

Line 216-219: Why is this information important? In this chapter (chapter 3) you are only focusing on SO_2 plumes, but no aerosol optical effects. I suggest skipping.

Line 284/285: "Especially the second stratospheric plume on June 16 could comprise remnants of the tropospheric plume, that are uplifted"
This sentence is confusing to me: What do you mean with tropospheric plume? The volcanic plume or the monsoon?

Line 323: There is no specification of the emission in Mills et al. 2016 so far. This pups up here a little abruptly, since this was not discussed in the introduction or anywhere prior to here.

Line 334: "The column amount estimation assumes that all SO_2 of the plume is centered at the respective altitude depicted in Fig. 4 "
To me it is not clear how this explanation should explain the discrepancies between observed and modelled column amounts. Can you explain further?

Line 338: "From Fig. 4, it seems that the simulated columns slightly broaden over time compared to the observations, with the plume appearing to sink." I am confused here. Figure 4 shows altitudes not column SO_2. It also seems like the simulated plume (reference) is higher up compared to the observed plume. Do you mean the observed plume appears to sink? It is hard to see any broadening of the plume in Figure 4.

Line 339/340: The simulated (reference) SO_2 column distribution in Figure 5 <u>is broader</u> compared to the observation... not narrower. This is confusion.

Line 361/362: "However, IASI faces limitations in capturing the long-term evolution of volcanic plumes due to the dilution of the emitted SO_2, leading to column amounts that fall below the instrument's detection limit»
Do you really know dilution is the main process that SO_2 concentrations fall below the detection limit? If yes, do you have references for this? Isn't chemical loss (SO_2 oxidation via OH and O3) equally or even more important on longer time scales? How is this represented in the model and how does this affect the long-term evolution of the SO_2 plume?

Lines 416-422: "The overall slightly faster decline observed in the simulation compared to the observations may be a consequence of the absence of primary particles, such as volcanic ash, in the simulations, resulting in a discrepancy between simulated and observed particle size distributions."
What processes should be the reasons for that? I guess you mean that ash could result in self-lofting of airmasses due to absorption of radiation and thus local heating? Or what other processes do you have in mind? It is important to name them since this is not clear from how it is written now.

"Alternatively, the simulated particle sizes may grow excessively large too quickly, leading to an overestimation of sedimentation efficiency»

You do not show any simulated particle sizes. You show and write about SO_2 plumes. SO_2 is a gas. Gases are mainly subject to diffusion & transport and in the case of SO_2 more importantly: chemical loss, ... but definitely not sedimentation. What about chemical loss? How does this affect the dissipation of the plume compared with observations?

"Whether this discrepancy arises from nucleation rates versus condensation efficiency, the overall representation of the size distribution with only four modes, or the limitation to one horizontal grid box will be the topic of upcoming studies."
SO_2 concentrations are definitely not affected by nucleation and condensation rates. Chemical loss of SO_2 is dominated by reaction with OH and O3. These reaction result in formation of SO_3, which then together with H2O forms H2OS4 gas. H2SO4 gas has a very low vapor pressure and immediately forms sulfuric acid aerosols via condensation or nucleation. Have a look at Feinberg et al. 2019 and the stratospheric sulfur cycle presented in there. It should get obvious that nucleation and condensation rates as well as aerosol size distributions do not affect the chemical SO_2 lifetime/burden.

Feinberg, A., Sukhodolov, T., Luo, B.-P., Rozanov, E., Winkel, L. H. E., Peter, T., and Stenke, A.: Improved tropospheric and stratospheric sulfur cycle in the aerosol–chemistry–climate model SOCOL-AERv2, Geosci. Model Dev., 12, 3863–3887, https://doi.org/10.5194/gmd-12-3863-2019, 2019.

Line 441: What "data"? Simulated or observed?

Line 341/342: Ahaa... it only becomes clear that you were talking about the initial plume on June 14 until now. The statements you make in this paragraph are only true for the initial plume on June 14. You really need to be more precise here... The statements of this paragraph are not valid for June 17.

Figures: I suggest assigning letters a, b, c, d ... to the subpanels of figures to enable better referencing.

Figure 444: It is hard to see any difference in agreement/disagreement with observation in Figure 8 of the June 7 and 10 data compared to for example June 5 and June 15. I suggest plotting <u>the differences compared to the observations</u> in the middle and lower panel. This would highlight the differences.

Line 448-451: Most of this can go into the figure caption.

Line 556: I disagree with that. The observations and the model does not "exhibit similar patterns" between 0 and 25N. The QBO signal is much more pronounced in the model compared to the observations. And the observed extinction is very different compared to the modelled ones in absolute numbers.

Line 459: Why do you think the observations are wrong? It could well be your model which is wrong. Why don't you optimize your model to improve agreement with observations?

Line 460: With "implemented emission rates" you mean the observations, right?

Line 461: I guess you considered the "emission rates" from the observations, right?

Line 465: "The coefficients a_{d-i} and the background SO_2 column amount, $SO_{2\,(col,BG)}$, represent the free parameters in the linear predictor and were determined through a least squares fit"
To what is "least square fit" referring to? What is it fitted to? "Least square fit" to the observed total column SO_2? If yes, then it should be obvious that the simulations in the end agree with the observed total column SO_2.

Line 466: What is a "stochastic gradient descent"? It would be helpful to describe this in one sentence, other wise it is just a black box to most readers. The code provided below does not help, since this is rather technical. This can go to a supplement.

Line 470: Why would you expect increases in spatial correlation if you only improve the emission rates?

Line 473: What do you mean with "effectively". I disagree with this statement. You only get good agreement in total column SO_2 when tuning the emissions in your model to fit the observational data. I think most models get better agreement with observations when tuning their emissions.

Line 474-478: You did not investigate any sensitivity to spatial resolution. Thus, you cannot make this conclusions. Delete this part, or show evidence for this conclusions.

Line 477/478: This is the most critical result which I think you must discuss more. You only get good agreement with total column SO_2 observations if you tune the emission rates according to your simulation results. I know that this is a common problem for models simulating volcanic eruptions (e.g. Mt. Pinatubo), but you should highlight this. critically discuss it and derive the right conclusions.
I also think "analysis" is not the right word here. More precise would be "tuning".

Line 520/521: The magnitude of the signal would not change if the satellite signals were only delayed compared to observations. Isn't it mainly the sensitivity of the measured satellite signal? Please be more precise here.

Line 522-529: Here again: What is the impact of chemical loss of SO_2? Also did you compare your model to background sulfur cycle (see Brodovsky et al. 2024)? It might make sense to fist perform same simulation as in this study to compare to other models and observations.
Brodowsky, C. V., Sukhodolov, T., Chiodo, G., Aquila, V., Bekki, S., Dhomse, S. S., Höpfner, M., Laakso, A., Mann, G. W., Niemeier, U., Pitari, G., Quaglia, I., Rozanov, E.,

Schmidt, A., Sekiya, T., Tilmes, S., Timmreck, C., Vattioni, S., Visioni, D., Yu, P., Zhu, Y., and Peter, T.: Analysis of the global atmospheric background sulfur budget in a multi-model framework, Atmos. Chem. Phys., 24, 5513–5548, https://doi.org/10.5194/acp-24-5513-2024, 2024.

Line 530: Here you suddenly start talking and comparing AOD resulting from these volcanic eruptions. So far you talked and compared SO_2 plumes. It would be great to first see some sulfuric acid aerosol size distribution or how the sulfuric acid aerosol plume/burden evolves in the aftermath of these volcanic eruptions (see Brodowsky 2021). This is what defines the AOD downstream not the SO_2 plume. There is an important part missing here when going from SO_2 plumes to AOD. Without this intermediate step it is hard to say where the discrepancies between model and observations are coming from. It is just guessing since aerosol formation and distribution in the model appear like a black box to the reader…

Also, crucial information is missing about the wavelengths to which the AOD and extinctions shown in Figure 11 and 12 are referring to.
Why did you only look at 0 ◦ to 25◦N and 45°-80°N? and not other regions?

Line 537-544: This paragraph needs references and is somehow handwaving.

Line 539/541: I think this reads a little hand wavy here. Please be more specific. A paper which addresses some potential effects is Vattioni et al. 2024.
Vattioni, S., Stenke, A., Luo, B., Chiodo, G., Sukhodolov, T., Wunderlin, E., and Peter, T.: Importance of microphysical settings for climate forcing by stratospheric SO_2 injections as modeled by SOCOL-AERv2, Geosci. Model Dev., 17, 4181–4197, https://doi.org/10.5194/gmd-17-4181-2024, 2024.

Line 542-544: "This phenomenon could potentially be addressed by distributing emissions across multiple horizontal grid boxes and releasing the SO_2 over an extended time period."
Why would you do this? In section 3 you showed good spatial agreement with observations, so why change the spatial distribution? What I think could help might be changing the horizontal resolution. It seems you again are looking for the error in the emission scheme/observations instead of in within the model. Your suggestion would reduce the SO_2 concentrations and thus the H2SO4 concentrations downstream. This reduces condensation and especially aerosol nucleation rates. But why should this be justified?

Line 546/547: "This anomaly could be attributed to an overestimation of transport from higher latitudes to the tropical stratosphere, or a general overestimation of the emissions."
Why should this be the case? Isn't the transport in this region going exactly into the other direction (from the tropics to higher latitudes)? And there is also a tropical "transport barrier".

Line 549: What differences are you talking about? Difference compared to what?

Line 552/553: "The interaction with the South Asian monsoon anticyclone potentially causes differing transport to lower or higher latitudes, respectively."
Weren't the simulations nudged towards observed wind fields?

Line 554/555: This sensitivity needs to be addressed by showing some plots in the supplement with different cutoff altitudes, since this defines whether the model agrees with observations or not…

Line 556-561: I would make it clear in this paragraph (and for the whole discussion of AOD from line 530 onward) that here you are talking about the sulfuric acid aerosol plume and the AOD resulting from these aerosols, whereas so far in the paper you talked about the SO_2 plume. The two SO_2 and sulfuric acid aerosol plumes likely look different.

Line 565/566: The first sentence of the discussion is not true. You do not show anything related to "aerosol formation". You only show comparison with AOD observations, but this does not tell you anything about aerosol formation processes.

Line 575: You do not show "aerosol burden" here. Thus, you can not make any conclusions about this.
Do you mean "forecasted" instead of "examined"? They can be examined, but just not immediately.

Line 589-593: You did not analyze how to "adequately simulate stratospheric aerosol burden". You cannot make any conclusions about stratospheric aerosol burden, if you do not show aerosol burden in the manuscript. The first sentence is confusing. What do you mean with "differences" in the first sentence of this paragraph? A difference compared to what? Again, what is the importance of chemical loss of SO_2 in the whole analysis? This could also be discussed here. You cannot make conclusions about the "sulfate" lifetime with the analysis shown in your manuscript.

Line 594-605: I agree that the horizontal extent of the emissions can influence the simulations.
"…emissions are constrained to a single horizontal grid box in this study…" I know what you mean, but this reads wrong. You also applied column emissions and vertically gaussian distributed emissions, which do not inject into "one single grid box". I would change this to "…emissions are constrained to a single grid box or columns of gid boxes in this study…" or make this clearer in a different way (e.g. what is the difference between a horizontal and a vertical gid box?) To me a grid box is a grid box… whether it is vertical or horizontal.

"…leading to non-linearities in the model that diverge from reality…" This statement needs references. Why is this important? What non-linearities are you talking about? I recommend highlighting the impact on aerosol formation/microphysics from this artefact (e.g. Vattioni et al. 2024).

In this paragraph you should also discuss the effect of the vertical and horizontal resolution of the model, since it is known that this can affect simulations of volcanic plumes.

"This concentration can lead to lower SO_2 and aerosol mixing ratios in the mid- to long-term, as aerosols grow excessively large and subsequently sediment out of the stratosphere, as observed following the Nabro eruption.» You provide an explanation for lower aerosol mixing ratios, but what would be the reason for differences in SO_2 mixing ratios? Also this sentence (and the whole paragraph) needs references, since you don't show this with your results.

Line 623-625: What about the importance of the stratospheric entry point?

Line 634: "emission" is written twice.

Line 691: Conclusions last paragraph: This paragraph should be put into future tense (and or conjunctive), since like it is written know one could think that this is already provided or underway.

Figure 1: It is not very helpful showing code in the main manuscript since this will not be helpful to most readers. If at all I would put this into a supplement.

Figure 2: From just looking at the figure caption it is not clear what the "plume" refers to: SO_2, Aerosol in general, ash or sulfuric acid aerosol? Maybe specify in the caption what the IASI satellite measures.

Figure 3: "amount" is not very specific. I would call the unit by its name (SO_2 column).

Figure 4: The caption could be clearer. What do you mean by "shortly after"?
If you compare observations to the altitude of the "maximum SO_2 mixing ratio", only one altitude should be displayed in your plot, since there is only one maximum in the vertical column, right? I am confused here.
Maybe change the last sentence to: "In the simulations SO_2 was only injected into the stratosphere, except for mills_et_al".

Figure 5: See comments on Figure 4. Why don't you compare to OMI as well?

Figure 6: Maybe replace "zonally" with "zonally averaged". And also "study" with "simulations". I would skip "approximately" or be more specific. Does the date provided refer to the 5-day average or to the date of the eruption? There is no space between the first and the second panel, and the black line covers the "0". Please correct this.

Figure 7: The first sentence of the caption can be skipped or integrated into the second one.
What is the unit of the x axis? The format mm/dd is not used universal (in Europe dd/mm is more common). Thus, I suggest writing Jun 15, Jul 1, Jul 15 and so one, to make this clear.
Y-Axis label: It is "SO_2", not "SO2".

Why don't you show the spatial correlation for 15/6?
Concerning the "stratospheric cutoff altitude": Do you mean tropopause? If not, why don't you use the tropopause altitude? If yes, I would name the tropopause by its name. Why did you choose these altitudes? Did you check the sensitivity of your assumptions? Looking at the satellite data and your simulations, you can see that 3 days after the eruption a considerable amount of the plume is exactly around 30°N. Thus, slightly changing the "stratospheric cutoff altitude" might have an impact on the results shown here. This could for example be done, by providing plots with 1km higher and lower "stratospheric cutoff altitudes".

Figure 8: To me it is very hard to see any difference between the middle row and the lower row. Maybe it makes more sense to show the difference between the middle row columns and the lower row columns to better display the improvement (if there is any).

Figure 10 and 11: Same comment as on Figure 7. What does "stratospheric cutoff altitude mean" and how sensitive are results to this definition?
Change to: "… using the EMAC model with the new *EVER* historic volcanic setup (red) and…"
The axis label should read "SO_2", not "SO2"

Figures 11 and 12: To which wavelengths do the aerosol optical depths and extinctions refer to? This is crucial information which is missing.