Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-332-RC2, 2018
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Atmospheric Chemistry and Physics Discussions
Open Access
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Interactive comment on "Long-range transport of volcanic aerosol from the 2010 Merapi tropical eruption to Antarctica" *by* Xue Wu et al.

Anonymous Referee #2

Received and published: 25 June 2018

General comments

This study investigates the transport of volcanic sulfur emissions from the 2010 eruption of Merapi. It uses both model simulations and satellite observations to investigate the poleward transport of sulfur to high latitudes. The topic is well suited to ACP.

Major comments

1. The study claims "good agreement" between the simulated trajectories and the MIPAS aerosol index measurements, and uses this "agreement" to underpin the conclusions of the work. Unfortunately, I just don't see a good agreement between the satellite observations and the model results. The model results (Fig 5) show a strong sulfur presence at and above the tropical tropopause, and strong poleward transport

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along the 380 K PT surface. From Fig. 7, the observations show almost no aerosol above the tropical tropopause, and slight aerosol increases in the high latitude upper troposphere. If this slight high latitude aerosol increase is from the Merapi eruption, the transport pathway does not appear to be via the 380 K PT surface, as in the model simulation. Plus, the observations show a highly elevated aerosol index in the SH high latitude stratosphere before the eruption, and some apparent descent of these aerosols, which could contribute to the elevated aerosol signal just below the tropopause in Jan and Feb 2010. I also worry about the choice of a very small reference period in the calculation of the anomalies in Fig 7, it seems likely that the anomalies shown might contain the influences of seasonal variations in aerosol and transport, for instance there are strong variations in UT aerosol in the NH, not associated with volcanic activity in Fig 6. The simulations presented here produce some clear results, but much more work would be required to be able to say that these results are supported or "validated" by the observations, this is simply not true.

2. The authors claim to have investigated the transport efficiency of volcanic aerosol, for example stating that "the most efficient pathway for the quasi-horizontal mixing was in between the isentropic surfaces of 360 and 430 K". They do not however define what they mean by "efficiency"–usually efficiencies correspond to some sort of ratio, e.g., the proportion of transported to injected material, but the analysis seems only to be based on the vertical levels where the largest numbers of trajectories existed, which is obviously strongly connected to the vertical level of the injections. It is also stated that transport from quasi-horizontal mixing is "more efficient" than the slow residual mass circulation of the Brewer-Dobson circulation, but this is a rather obvious result for an injection into the tropical UTLS especially when only short term simulations are performed. Is transport to Antarctica from a tropical UTLS injection more efficient than transport of background sulfate via the BDC? Such questions cannot be answered with this study.

3. The connection between the aspects of the simulated aerosol transport and different

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dynamical regimes–the phase of the QBO or the strength of the subtropical jet–are not tested in any way, and so cannot be linked in a causal way. For example, the study does not show in any way that the transport of aerosol "was facilitated by the weakening of the subtropical jet during the seasonal transition from austral spring to summer and linked to the westerly phase of the quasi-biennial oscillation (QBO)" as stated in the conclusions. This is an assumption based on prior work, not a conclusion of the experiment presented here.

Specific comments:

P1, l16: usually present tense ("estimate") is used to describe work done in the presented study. Check here and throughout.

P1, l19: See major comment 1: "good agreement" is never quantified, and there is little evidence that the observations suggest a significant amount of aerosol transported to the SH high latitudes.

P1, l26: Most efficient or strongest? Most efficient would imply calculation of some sort of efficiency index, which does not seem to have been done. These heights may have been where the strongest transport occurred just because of the height of the injection, not because the transport there is stronger than other heights.

P1, l28: I guess less than 4% of the SO2 was transported to high SH latitudes since SO2 is decaying with a 1-month timescale, you probably mean 4% of the injected sulfur.

P2, l8: The study by Aquila et al. (2014) presents model results of a geoengineering scenario: this is not strong proof for the kind of strong statement given here.

P2, l9: Similarly, the Pausata et al. (2015) study is also based on modeling results, this statement needs to be less conclusive to properly reflect current scientific understanding.

P2, l11: The impact of aerosol on polar ozone has been known about for a long time; a handful of references from 2008-2016 don't properly support the statement made here.

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P2, l17: “a new record of the size...” is awkwardly phrased.

P3, l11: Kravitz and Robock (2011) describe the impact of season on the impact of high latitude eruptions, and say little about the impact of the variation of quasi-horizontal mixing with season. Toohey et al. (2011) would be a more appropriate reference here.

Toohey, M., Krüger, K., Niemeier, U. and Timmreck, C.: The influence of eruption season on the global aerosol evolution and radiative impact of tropical volcanic eruptions, Atmos. Chem. Phys., 11(23), 12351–12367, doi:10.5194/acp-11-12351-2011, 2011.

P4, l12: “product is sensitive. . .”

P6, l12: Some indication of what kind of measurements have been used to build the “chronology of the Merapi eruption” would be useful for the reader here.

P6, l25: The reconstructed SO2 injection between Nov 1 and Nov 3, which shows injection above the tropopause but little anywhere else–seems unlikely given the reported eruption chronology. This portion of the injection timeseries seems very uncertain, and the implications of possible errors here should be discussed.

P7, l2: One should be clear that you are neglecting the impact of sedimentation completely, i.e., for all sulfate particles–it reads now like you are neglecting sedimentation for the small aerosol. Secondly, you need some words on the applicability of this assumption.

P7, l7: “parcels in the upper troposphere. . .”

P7, l21: The two sentences starting with “A more negative...” seem to be referring to conclusions from prior studies, which should be referenced. Also, the first sentence especially is confusing since the post-Merapi anomalies in Fig 4d imply a less negative eddy heat flux, best would be to adjust the description to be consistent with what is seen in Fig 4.

P8, l21: please be clear that you refer to “the simulated plume” here and elsewhere.

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P8, l25: Till->Until

P8, l30: I don't see the "secondary upward transport" commented on here, either the description needs to be clearer, or this statement cut.

P8, l31: Using the word "observable" when presenting simulation results is confusing.

P9, l9: I don't think you can conclude with all certainty that the transport was suppressed by the subtropical jets–it is likely, but you didn't test it in any way in your analysis.

P9, l17: Four days is a very small sample to base a reference state on. How sensitive are the results to the choice of a reference? Better might be to define a clean climatology from prior years and subtract a seasonally varying climatology.

P9, l22: Why does the zonal averaging "degrade" the ACI signal? Is this related to incomplete sampling and the plume being localized? What about the ∼1 month timescale of the conversion of SO2 to sulfate, does that play a role?

P9, l24: The strongest signal in the SH high latitudes is a large decrease in ACI, what is this?

P9, l25: How was the significance of this increase tested?

P10, l2: "comparable" is a rather weak adjective, and in fact there are many important differences between the model results and observations (see major comments).

P10, l4: "in the surf zone"

P10, l6: What is "our data"? The statement here seems based on the simulation results, but not strongly supported by the observations.

P10, l7: The conclusion regarding the main transport pathway stated here should apply only to the Merapi eruption–a large eruption like Pinatubo with a higher injection height will certainly have a different main transport pathway.

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P11, l5: “the simulated poleward...”

P11, l14: observed->simulated

P11, l25: The impact of the QBO was not shown in any way by this study, only assumed based on prior work.

P11, l28: No, Figure 4 shows there was weaker than usual wave activity after the eruption, and a colder, more stable vortex until the end of February.

P12, l1: This paragraph is irrelevant, unless the authors have some evidence that the aerosol from Merapi was enough to produce enough heating to perturb the QBO.

P12, l9: This sentence, with the use of the word “contributed” is a rather strong statement given the observations don’t really support the strong degree of transport in the model.

P12, l23: Again (see major comments), I don’t agree that the “MIPAS aerosol detections confirmed the MPTRAC simulations”.

P13, l12: “validated” is too strong.

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