Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1110-RC1, 2020

Atmospheric Chemistry and Physics
Discussions
Open Access

Interactive comment on "Co-emission of volcanic sulfur and halogens amplifies volcanic effective radiative forcing" *by* John Staunton-Sykes et al.

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Received and published: 18 November 2020

In this study the authors, using a CCM model, try to understand how co-emitted halogens may alter the climate impacts of stratospheric sulfur injection by volcanic eruptions. Their results show that the inclusion of halogens dramatically changes some of the simulated impacts, and that including such emissions is crucial to properly simulate large explosive volcanic eruptions. I found the paper to be excellent: the introduction does a great job at framing the problem and the methods are properly described. The result are also explained clearly, with pretty straightforward figures (which I suggest uploading in a higher quality format, they look a bit blurred when zoomed in). Overall, the manuscript is perfectly suitable to be published in ACP.

I have only a few minor comments:

L 69: highlight with no s L 93: The correct name of the model is CESM1(WACCM) (Community Earth-System Model and then WACCM) L 116: from the way the phrase is written, it looks like the different halogen emissions are applied to the same amount of SO2, but in that case, the ratio would also have to be 100 times. But Ming et al. 2020 compare also a low and high SO2 injection (10 vs. 100 Tg) and in there different HCl concentrations. Just try to clarify this point.

Fig. 1: please specify what is the quiescent period against which the anomalies are calculated in the caption. Fig. 1f: I'm a bit confused as to why in the SULF simulations, there is a small increase in OH that I don't think is properly explained in the text. In the SO2 plume, we expect a large OH depletion. I assume that can be balanced out by the influx of water vapor in the stratosphere from the lower stratospheric heating and produce globally a slight increase. But I'd suggest checking (or consider the tropical changes in stratospheric OH, where I'm sure the change is negative – albeit less than in the HAL experiments).

Line 255: it would be useful to show the changes in w* (maybe in the supplementary next to Fig. S1) to show the difference in the transport induced by the stratospheric heating.

Fig. 3: I'd suggest switching panels a and b, as logically one might expect the lower injection scenario before. Also, I find it interesting that the relationship doesn't hold as well for the lower injection case. I suspect this might be due to different QBO phases that affect the aerosols e-folding time (see Pitari et al., 2016), and that this effect is more evident for lower injection rates while for higher injection rates the increase heating rates modify the QBO too strongly independently on the phase it's in at the moment of injection (see for instance Aquila et al., 2014) resulting in similar lifetimes. The authors could just check if that's the case verifying the QBO phase, or just mention that's a possibility for the lower correlation in panel b (unless they have a better explanation).

Fig. 8: please specify if panels a-d are global changes.

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1110, 2020.