Review of “*Exploring accumulation-mode-H2SO4 versus SO2 stratospheric sulfate geoengineering in a sectional aerosol-chemistry-climate model*” by S. Vattoni et al.

This article is an interesting comparison between different sulfate geoengineering (SG) injection strategies, namely between SO_2 injection (the standard for most simulations up to now) and H_2SO_4 (that has been proposed but not as often studied). The study is innovative in its comparison of the two injection strategies (not in the coupling of CCM and sectional aerosol model, as mentioned by the authors at the end of section 2), and I believe that it deserves publication in ACP after the two major issues (and several minor suggestions), that I list below, have been addressed.

Major points:

1) The authors compare their results with previously obtained results (some of them at least), but they have one major difference with them that is not highlighted anywhere: the longitudinal distribution of the emitted sulfate.
Let’s take first Niemeier and Timmreck (2015) that the authors mention: they only have one simulation (Geo10-lon) where the injection is spread on all longitudes, and only for a very small latitudinal range (3N to Eq.). In all other scenarios, emissions are only between 120.9 to 123.75 E. Also Kleinschmitt et al. (2018): they mostly always inject at one single longitude (120E), and only once (BROAD) at 28 locations between 30N and 30S.
So, when the authors in the conclusion of their study claim that their RF efficiencies are smaller than those previous ones and they generally point at “lower stratospheric aerosol burdens in our model” they should explain this major difference.

I’m not suggesting the authors change their simulations to align closer to previous ones, but the difference between their study and previous ones should be further highlighted, and maybe compared with their sensitivity test (GEO_AERO_point_00 and GEO_SO2_point_00) if they want to make an apple to apple comparison. Overall, I think it would be good that the authors justified much better their assumption that injections over all longitudes are preferable to injections centered in one longitude (and so they chose to do pointed injections only as sensitivity case). Do the authors think this is model dependant? For instance, Tilmes et al (2017) note that, in their model, " Single grid point injections produce sulfate aerosols of smaller size that reflect sunlight more efficiently than injections over a longitude band”.

2) In Table 3 they show what they call “important impacts”, naming water vapor and ozone column. However, recent literature has shown that those are not the only (nor the more important) impacts, especially in terms of radiative forcing: what about methane (Visioni et al., 2017 and Tilmes et al., 2018) or ice clouds (Kuebbeler et al., 2012 and Visioni et al, 2018a)? I believe that both factors can’t be ignored, when talking about the possible range of impacts of SG. At the very least they should be mentioned, acknowledging previous results, but much better would be to show the changes produced in SOCOL regarding one or both of these aspects. Considering the authors focus also on chemical changes by showing OH and H_2O vertical profiles, I suggest they at least analyse their atmospheric methane changes, comparing them with previous studies.

As I said, before being suitable for pubblications, I believe a major revision considering these two points is necessary. Furthermore, I have some minor points that I list below:

Abstract: The abstract is way too long and confusing. You don’t really need to put all your results in the abstract, there’s plenty of space elsewhere. Also, the use of the term “surprising” twice is missleading, considering that the last phrase of the abstract is “this study corroborates previous studies". Shorten it by only pointing at some of the results and leave for the discussion all the rest. I agree with Alan Robock that the usage of parenthesis in that way needs to be dropped.

P. 1, lines 17-19: This phrase is very convoluted and confusing.

P. 2, lines 15-25: As I mentioned in my point 2), are the authors referring to only SG side-effects that reduce the efficiency of the RF produced by the sulfate aerosols (as stated in lines 17-18) or general drawback of SG? I believe the former would be more correct and would make more sense here. Because of this, a bit of clarification is important: (2) of all chemical side effects, ozone depletion is the one with the less significant RF effect (see Pitari et al., 2014). More important would be the effect on methane and other GHGs (see Visioni et al., 2017 and Tilmes et al., 2018) because of changes in photolysis rates and transport. Furthermore, in terms of RF, also the effects of a decrease in UT ice particles, as showed in Kuebbeler et al. (2012) and recently in Visioni et al (2018a), that would produce a cooling effect by trapping less planetary radiation, is important.

Point (4) has nothing to do with RF and possible effects of SG on ecosystem have been studied in more details after Kravitz et al. (2012) (such as in Xia et al., 2017), together with other changes (tropical storm ecc.). But this is not the point of this article.

P. 4, line 8: Visioni et al (2017, 2018a and 2018b) also used a sectional aerosol approach in their model and fully coupled microphysics and chemistry. It would be good to compare some of their results with yours since they also focus on RF changes, as I explain in point 2) and in some of these comments.

P. 4, line 19: Mills et al. (2017) is more of a description of WACCM-CESM new model set-up and the validation with Pinatubo data, so it is not about SG scenarios.

P. 6, lines 11-13: Here the authors should give the reader some more contest on this. The amount injected in this work is not really an issue here (because you are doing sensitivity studies and not looking at the long-term climatic response), but it's a bit of a stretch to say that that's how much Pinatubo injected. We don't know exactly how much SO_2 Pinatubo injected, so from a modeling prespective the amount of SO_2 for Pinatubo is very model dependant (from 10 to 20 Tg-SO_2 to get the best agreement with AOD observations), and it would be good to mention this (Timmreck et al. (2018) would be enough to reference, but just look at, for instance, Mills et al. (2016) where they inject 10 Tg-SO_2 and Pitari et al. (2016), where they inject 20 Tg-SO_2). Just give some contest to the reader. Personally, 1.83 Mt-S per year seems very arbitrary, and I would have preferred an amount more comparable to previous simulations.

P. 7, line 2: I would suggest explaining a bit what the authors mean by "surf zone" and offer some references (as you do, rightfully, for the BDC). It might not be such a common term as you think.

P. 12, line 1: As per my point 1): most other studies (see Tilmes et al., 2017, but also the entire GeoMIP G4 experiment) inject at only one longitude. In other simulations the single longitude was found either better for the scope (r_{eff} closer to the desired one, Tilmes et al., 2017) or unimportant (because of the fast mixing time). You should mention this here.

P. 12, line 5: The shared longwave surface anomaly doesn't really say anything, especially since you don't have a surface coupling. It would not be due to the aerosol absorption anyway, but it is the result of many more processes. I suggest removing this phrase or explain better what you think the surface LW anomaly tells you, if you think it's important.

P. 12, line 14: constant climatological what? Concentration? It's confusing.

P. 12, line 20: split this in two phrases, one for AS and one for CS. Don't use parenthesis this way.

P. 13, line 5: Either you write "Emission strategies like that investigated by …" or "Emitting at … as investigated by …"

P. 13, line 16-23: Again: ozone and water vapor are only two of the SG side effects when it comes to RF: you should at least mention the fact that methane lifetime would increase and ice cloud decrease.

P. 14, line 1: like in the case of SO_2 emissions

P. 15, lines 7-8: as I said in my major point 1), mention the big difference between the studies, that is the different longitudinal distribution of the injections.

P. 15, lines 13-14: Yes, but in the framework of CCMI, so considering reference scenarios. The response to the BDC to the stratospheric heating produced by SG is not something that is considered there. The reference is still useful but mention this, at least.

P. 15, line 24: As has been previously studied in Kuebbeler et al (2012) and Visioni et al (2018a).

P. 16, line 6: "with" current GCMs.

Figure 2: This figure has a very poor resolution (compared to Fig. 5 that is similar). Panels b) and c) have such large scales and the curves are minuscule.

Figure 4: This is an interesting figure but must be improved. The scale for the SW forcing could be reduced a lot to higlight the differences between the different simulations (panel b from -1 to .3 W/m2 and panel c from 0 to -2 W/m2). Furthermore, symbols are very hard to read. I suggest enlarging them (or using more color).
Very minor thing, the **i**s and **l**s in figure 4 are all "weird", they look bold while the rest of the letters don't. You should fix this. (same goes for figure 2, 6 and 7). It's probably just a problem of how you saved the figures.

References:

Kleinschmitt, C., Boucher, O., and Platt, U.: Sensitivity of the radiative forcing by stratospheric sulfur geoengineering to the amount and strategy of the SO_2injection studied with the LMDZ-S3A model, Atmos. Chem. Phys., 18, 2769-2786, https://doi.org/10.5194/acp-18-2769-2018, 2018.

Kuebbeler, M., Lohmann, U., and Feichter, J.: Effects of stratospheric sulfate aerosol geo-engineering on cirrus clouds, Geophys. Res. Lett., 39, L23803, https://doi.org/10.1029/2012GL053797, 2012.

Niemeier, U. and Timmreck, C.: What is the limit of climate engineering by stratospheric injection of SO_2?, Atmos. Chem. Phys., 15, 9129-9141, https://doi.org/10.5194/acp-15-9129-2015, 2015.

Pitari, G., Di Genova, G., Mancini, E., Visioni, D., Gandolfi, I., and Cionni, I.: Stratospheric Aerosols from Major Vol- canic Eruptions: A Composition-Climate Model Study of the Aerosol Cloud Dispersal and e-folding Time, Atmosphere, 7, 79, https://doi.org/10.3390/atmos7060075, 2016.

Tilmes, S., J. H. Richter, M. J. Mills, B. Kravitz, D.G. MacMartin, F. Vitt, J. J. Tribbia, and J.-F. Lamarque, 2017: Sensitivity of aerosol distribution and climate response to stratospheric SO2 injection locations, JGR-Atmospheres

Tilmes, S., Richter, J. H., Mills, M. J., Kravitz, B., MacMartin, D. G., Garcia, R. R., et al. 2018: Effects of different stratospheric SO_2 injection altitudes on stratospheric chemistry and dynamics. *Journal of Geophysical Research: Atmospheres*, 123, 4654–4673

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.

Visioni, D., Pitari, G., Aquila, V., Tilmes, S., Cionni, I., Di Genova, G., and Mancini, E.: Sulfate geoengineering impact on methane transport and lifetime: results from the Geoengineering Model Intercomparison Project (GeoMIP), Atmos. Chem. Phys., 17, 11209-11226, https://doi.org/10.5194/acp-17-11209-2017, 2017.

Visioni, D., Pitari, G., di Genova, G., Tilmes, S., and Cionni, I.: Upper tropospheric ice sensitivity to sulfate geoengineering, Atmos. Chem. Phys., 18, 14867-14887, https://doi.org/10.5194/acp-18-14867-2018, 2018a.

Visioni, D., Pitari, G., Tuccella, P., and Curci, G.: Sulfur deposition changes under sulfate geoengineering conditions: quasi-biennial oscillation effects on the transport and lifetime of stratospheric aerosols, Atmos. Chem. Phys., 18, 2787-2808, https://doi.org/10.5194/acp-18-2787-2018, 2018b.

Xia, L., Nowack, P. J., Tilmes, S., and Robock, A.: Impacts of stratospheric sulfate geoengineering on tropospheric ozone, Atmos. Chem. Phys., 17, 11913-11928, https://doi.org/10.5194/acp-17-11913-2017, 2017.