Review of the paper: *A statistical examination of the effects of stratospheric sulphate geoengineering on tropical storm genesis*, by Q. Wang et al., Atmos. Chem. Phys. Discuss., acp-2018-142, 2018.

In this numerical work, a statistical approach is described for analysing the effects of sulphate geoengineering on the genesis of tropical storms. The procedure is well designed on the general methodology of the GeoMIP project, with use of data that independent global models have provided in a common database with their G4 simulations. The manuscript is scientifically robust and deserves publication on ACP.

Some of the conclusions are important, mainly the fact that the thermodynamic role of SST changes induced by geoengineering aerosols dominates over the lower stratospheric aerosol heating. However, sometimes the authors compare the SST effects with changes in static stability, as if they were two independent things (see for example in the conclusions, lines 547-549). Actually, SST changes may affect the atmospheric static stability by themselves, even in the absence of a stratospheric warming. I would suggest rephrasing. The authors themselves clearly explain how static stability changes are controlled by both surface and upper tropospheric temperatures (page 18, lines 358-360).

This is the main *specific point* I suggest to better clarify all along the manuscript, before final publication on ACP. In addition, it is true that the aerosol heating is mostly located in the 16-25 km layer (see page 27, lines 540-542); however, due to the large size of the geoengineering aerosol particles (effective radius of the order of 0.6 μm or more), a significant fraction of the stratospheric particles would settle down below the tropical tropopause (Niemeier et al., 2010; English et al., 2012; Cirisan et al, 2013), thus producing some diabatic heating superimposed to the convectively-driven upper tropospheric cooling. This means that the surface cooling (with associated upper tropospheric tropical cooling, due to lesser efficient convective motions) may be expected as the dominant process controlling the geoengineering induced changes of atmospheric static stability. At the same time, the aerosol heating in a few kilometres layer immediately below the tropical tropopause (due to gravitational sedimentation of large geoengineering sulfate aerosols) should also be considered as a contributing smaller effect.

It would be worth to note that another indirect effect of sulfate geoengineering, related to the surface cooling and static stability changes, is discussed in Visioni et al. (2018). Here the sensitivity of upper tropospheric ice formation is studied with inclusion of the aerosol-induced surface cooling, with respect to a reference condition documented in Kuebbeler et al. (2016), where only the stratospheric warming due to the aerosols was taken into account. The conclusions presented in the manuscript of Wang et al. (2018) go in the same direction of what discussed in this other study.

Minor points

P. 3, line 66: the Kravitz reference has a wrong comma between the name and *et al*.
P. 3, line 72: some more recent articles can be cited here, for example Visioni et al. (2017).
P. 4, line 83: *are* used instead of *have* used.
P. 6, line 126-130: I would suggest rephrasing this concept, maybe splitting the long sentence in two. In its present form it is hard to follow.
P. 7, line 149: explain better the altitude at which the injection is simulated, since it has been shown how different injection heights may affect differently the climate response (Tilmes et al., 2017; Kleinschmitt et al., 2018).

P. 8, line 162-165: for a recent study analysing the connection between the stratospheric warming due to the sulfur injection and the tropospheric response in term of vertical motions, see Visioni et al. (2018) (now under review in ACPD).
P. 14-15: I suggest to the authors to move some of the longer equations derivations to the supplementary material for better readability of the manuscript.
P. 22, line 433: no comma between models and are.
P. 25, line 510: I would suggest using "variability" instead of "variations".
P. 27, line 539: analyze instead of analysis. Better rephrase "aerosol injection scheme" into a more appropriate description, such as "protocol".
P. 27, line 552: "in the response strength across the ocean basins" sounds probably better than "in strength of response across the ocean basins".
P.38, Fig. 1: adding a legend outside the figure, instead of having the names of the models close to the related lines, would make it easier to read.

References

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