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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone changes under solar geoengineering: implications for UV exposure and air quality" *by* P. J. Nowack et al.

Anonymous Referee #1

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General comments:

The present study investigates the impact of solar geoengineering on climate, stratospheric ozone and surface UV-B using a global atmosphere-ocean-chemistry-climate model. The analysis is based on three model simulations: a pre-industrial control experiment, a 4xCO2 experiment and an experiment with reduced solar irradiance to offset the CO2 induced global warming. The simulation set-up follows the GeoMIP G1 experiment.

In general the manuscript is well written, the argumentation is easy to follow, and the figures are well prepared. There are some sections where more detailed information would be helpful. I added a couple of remarks and suggestions below.

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My major concern is related to the experimental set-up, which is highly idealized and, in my view, not appropriate to address air quality issues under SRM. The applied scenario follows pre-industrial conditions. Although not directly mentioned in the manuscript, I assume this holds also for ozone depleting substances, tropospheric ozone precursors and aerosols. While such a scenario might be appropriate to investigate climate change under SRM, I have some doubts that this is also the case for tropospheric chemistry and air quality. I think we agree that SRM will never take place under clean air conditions. The authors state that the aim of the present study is not to provide a quantification of the effects, but to discuss principle changes of atmospheric chemistry and climate under SRM, but I think even a qualitative discussion is hardly possible, since the relative importance of the involved processes might change under a different atmospheric composition. I am aware that the authors simply follow the predefined set-up of the GeoMIP G1 experiment, but in my opinion this set-up is not appropriate to address tropospheric chemistry changes.

What should I recommend now? It is a solid study, and I am not at all against idealized model experiments. They can be very useful, but the set-up must be appropriate. I would either suggest extending the discussion towards more realistic atmospheric conditions (the authors might have some further sensitivity studies available that could be added) or focusing more on climate change than on air quality.

Specific comments:

- P2, L24ff: I would suggest to rewrite this paragraph or to split it into 2 sections. First, it describes the potential impact of SRM by particle injection on ozone depletion and, hence, increasing surface UV-B, and then it suddenly jumps to negative effects of decreasing surface UV-B on human health. This is not very intuitive and needs some more explanation.

- Section 2.1: In line 24-26 you mention a simple tropospheric chemistry scheme that has been implemented to your model. How 'good' is your tropospheric chemistry, es-

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pecially in terms of ozone? Since you discuss tropospheric ozone changes in Sect. 3.3 it would be very interesting to see how realistic your tropospheric chemistry is. Please provide some more details, e.g. references to previous model studies if available, or even a short evaluation of your tropospheric chemistry scheme. Otherwise it is hard to judge how reliable the simulated ozone changes are.

- Section 2.2: Which scenario has been chosen for ozone depleting substances, tropospheric ozone precursors, etc.? Also pre-industrial? Section 2.2 describes only the CO2 scenarios and the solar irradiance change. I would like to see some more details about the experimental set-up.

- P5, L20/21: Are 75 years enough with a coupled ocean? From Fig. 1 I got the impression that the 4xCO2 experiment is not yet in equilibrium after 75 years.

- P5, L15: The solar irradiance reduction of 49 W/m2 – where does this value come from? Specified by the G1 experimental set-up or calculated by the authors to compensate the surface temperature increase under 4xCO2, taking into account the model specific climate sensitivity? In case the 49 W/m2 are a model-dependent value, it would be interesting to see a short comment about the climate sensitivity of the applied model. How does it compare to other models?

- P6, L9-11: It seems that the authors performed some additional sensitivity runs that are not further discussed in the manuscript. For me this is a bit unsatisfying. How does the fixed ozone field differ from the interactive ozone? Which other chemical species were kept fixed at PI levels? How large is the RF of those species?

-P6/7, discussion of Fig. 3: I think this part needs some revision. The discussion of temperature and ozone changes in the 4xCO2 and G1 experiments is a bit unstructured. From what is written in L21-25 (P6) I got the impression that the ozone changes in G1 are also related to a colder stratosphere, although Fig. 3d shows a warming for large parts of the stratosphere. I would first show the temperature changes in 3a and b, and then the ozone changes in 3c and d.

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- P7, L2/3: Why does the stratospheric cooling shift the ratio between atomic oxygen and ozone towards ozone? Why does the atomic oxygen in R1.2 come from? Photolysis?

- P7, 16-20: Why is the decrease in atomic oxygen only visible on pressure levels and not on model levels? Please provide at least a short explanation. Referring to another paper is not very reader-friendly.

- P7, L32/22: What is the reason for the increased upper stratospheric NOx abundances under 4xCO2?

- Section 3.3: In my opinion the whole discussion on tropospheric ozone changes is purely abstract. Here air quality issues under pre-industrial, i.e. clean, conditions are discussed. I am aware that the authors stated in Sect. 2.2 that the G1 experiment is highly idealized and does not allow exact quantification of the atmospheric changes under SRM. I think we agree that SRM will never be applied under clean air conditions. There will be nitrogen oxides and NMVOCs, and, as also stated on page10, line 5-7, this will change the relative importance of the different mechanisms. In my opinion the experimental set-up is not suited to investigate the impact of SRM on tropospheric chemistry and air quality, so this section could be skipped or revised by discussing more realistic scenarios, e.g. by comparing the idealized G1 experiment with other model simulations using present-day conditions.

- P11, L23-25: Same as above, tropospheric aerosols and their impact on the UV-I are neglected although it's very likely that they will play an important role under SRM in a future climate. In my opinion the scenario is too much idealized.

- P12, L26-30: For such a general statement is it not necessary to run a fully coupled AOCCM. Some basic physical and chemical considerations would lead to the same conclusion. For a proper evaluation of risks and benefits of SRM one would need a quantification of these effects, which is not given here.

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- P14, L4-6: Now I am completely puzzled: Which scenario did you assume for ODS and ozone precursors?

- Table 2: Are the shown changes all statistically significant?

- Figure 2, right: Since the shown temperature differences range between -4 K and +4 K, I would adjust the color bar.

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