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Interactive comment on "The response of terrestrial ecosystem carbon cycling under different aerosol-based radiation management geoengineering" *by* Hanna Lee et al.

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The manuscript "The response of terrestrial ecosystem carbon cycling under different aerosol-based radiation management geoengineering" by Lee et al. investigated the impacts of 3 different aerosol-based radiation management geoengineering methods on land C cycle using the NorESM1-ME earth system model. By comparing the simulations under RCP 8.5, RCP 4.5 and 3 geoengineering scenarios, the authors suggested that different geoengineering methods can result in very different precipitation patterns in tropical forests and finally affect global C budget. Also, the authors suggested a significant impact from CO_2 fertilization.

Generally, this manuscript is well structured and written. Although, some analyses are

Printer-friendly version

Discussion paper

recommended to further improve the manuscript. Below are my suggestions:

General comments:

1. The authors investigated the spatial patterns of precipitation changes caused by RM applications, but gave only a little information on the spatial patterns of other variables affecting C cycle. As recently suggested by Zhang et al. (2019), vegetation at different latitudes show different sensitivities to aerosol-caused temperature changes. In Line 168-170, the authors indicated stronger cooling effect of RM in tropics than high latitudes. How is this pattern different from the RCP4.5 scenario? It would be interesting to compare and discuss. If possible [optional], to quantify this impact using sensitivity as in Zhang et al. (2019) or a few offline simulations on the land surface model will be able to distinguish the impacts of temperature (probably other factors) and CO2 fertilization and provide more insightful understanding of RM impacts.

2. Similar to the previous point, I would also check if the downward solar radiation at land surface has different patterns among the 3 RM methods, especially in tropical forests, because some studies indicate radiation-limiting vegetation in these regions (e.g. Nemani et al., 2003).

3. The authors suggested that the diffuse radiation is not so important regulating NPP in Line 160. To justify this, the authors need to clarify whether and how the model distinguish diffuse and direct radiation in the Model description section. Also, I recommend the authors to use total downward surface radiation and diffuse radiation fraction rather than direct and diffuse radiation because the previous way can more clearly distinguish the effect of aerosol-caused dimming and increase in diffuse light.

4. It is not clear whether SI1 is the spatial pattern of temporal correlation, or the pattern of spatial correlation calculated in groups of nearby grids? The later correlation might indicate mainly the response of PFT distribution to precipitation regime given the coarse resolution of the model. In the manuscript, to investigate the response of the vegetation to precipitation changes, the temporal correlation (partial correlation to

control temperature and radiation) is more reasonable.

Other comments:

Line 153 “large overall”. Also to be consistent in tense. For instance, Line 153 used “is” but Line 155 used “was”

Line 189-191: Not clear, need to be rephrased

Overall, this is a good study and relevant to ESD. I recommend to publish it with the above points addressed.

Mentioned References: Nemani, R. R., Keeling, C. D., Hashimoto, H., Jolly, W. M., Piper, S. C., Tucker, C. J., Myneni, R. B., and Running, S. W.: Climate-driven increases in global terrestrial net primary production from 1982 to 1999, Science, 300, 1560-1563, 2003. Zhang, Y., Goll, D., Bastos, A., Balkanski, Y., Boucher, O., Cescatti, A., Collier, M., Gasser, T., Ghattas, J., Li, L., Piao, S., Viovy, N., Zhu, D., and Ciais, P.: Increased Global Land Carbon Sink Due to Aerosol-Induced Cooling, Global Biogeochemical Cycles, 33, 439-457, 10.1029/2018gb006051, 2019.

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