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Earth System Dynamics Discussions

Interactive comment on "Climate engineering to mitigate the projected 21st-century terrestrial drying of the Americas: Carbon Capture vs. Sulfur Injection?" *by* Yangyang Xu et al.

Anonymous Referee #1

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The author studied the climate response to carbon capture and stratospheric sulfur injection geoengineering, using simulation results from two coupled global climate models. The author focused on the climate response, including temperature, precipitation, and aridity over major land area especially North and Southern America. While the scientific question examined here could further our understandings of the potential impact of different geoengineering strategies on the regional climate response, this paper fails to convince me to be useful in a number of aspects (Discussed below). Therefore, I don't think the current paper is suitable for publishing unless a substantially improved version is made.

First of all, the way the paper is written could be improved substantially. Many important details that are crucial for readers to understand their results are missing, which confuse me a lot during my reading of the paper. For example, in the Methods section, the model and experiment descriptions are just too simple. The author compares simulation results from two coupled global climate models, CESM-WACCM and CESM-CAM5, but how different these two models are, and how these differences could affect simulated climate response is rarely included. Also, the author mentioned that the land component differs from each other for these two models, but did not include any discussion about what the difference is. The description of the experiment design and post-processes is also not satisfied. For experiments used in this study, did you use the GLENS project results for the Sulfur Injection case? Where did you get the Carbon Capture simulation results and what is the trajectory of the CO2 concentration/emission in this simulation? For the bias correction, I am confused by how exactly the bias correction is applied to the future simulation cases to account for different climate responses for these two models? etc. Another example is that in section 2.2, the authors mentioned "CO2 capture and massive CO2 emissions cuts applied to start in 2015-2020", in the next section the author says "both schemes are designed to be deployed in 2020", I am confused which one is accurate? The author should at least state such basic information clearly in the paper.

I am also not convinced by the paper. In many places, the author compared the difference of climate responses to two geoengineering scenarios. However, these scenarios are simulated using two different global climate models, and the authors have mentioned that climate sensitivity differs substantially for these two models. How should I believe the difference in climate responses is due to different impacts on these geoengineering schemes, not just because of the difference in the structure of these models? Section 5 is helpful in some way, but I am still not sure how should I interpret most of these results? Many descriptions presented in the paper seem not accurate to me. For example, the authors claim that "spatial patterns show similar agreement between P/PET and soil moisture as well as P-ET" in figure 2. While the time series do show similarities between different indexes, the spatial patterns show opposite responses in many regions, especially Eastern North America and Southern South America. I am not sure if this could be seen as similar?

Line 7, the authors claim that "it is essential to apply the bias correction to model output before carrying out a meaningful comparison of future changes". If you are using simulation results from the CESM_RCP8.5 2010-2019 period to correct both WACCM_RCP8.5 and WACCM_SulfurInjection, and since WACCM_SulfurInjection is the same to WACCM_RCP8.5 before 2020, why should the difference between these two cases change compared to results without bias correction? Can you compare these simulations using the same model for all these simulations? Otherwise, I am not sure what is the best way to account for difference caused by using different models.

Line 10-12: First of all, I think the author should emphasize more that the analysis here is only for changes over land, especially when you are presenting the results. The author claims that the larger warming over land for WACCM is due to the larger climate sensitivity of the model. The two models differ not only in climate sensitivity but also the ratio of the land to ocean temperature responses. How much of this larger warming is due to larger climate sensitivity, and how much is due to different land/ocean temperature responses? The same concern applies to the next paragraph (lines 16 to 20). How much of the larger warming in WACCM compared to other models is due to climate sensitivity or land/ocean responses in these models? If the main reason is climate sensitivity, then the larger cooling in WACCM could just because WACCM produces more warming and the goal is to stabilize global mean temperature changes at the same level. I don't think the reason is simply that a larger injection amount or the injection strategy. If the main reason is the different land responses, then does the explanation in the previous paragraph using climate sensitivity still hold? Also, I don't know what you mean by saying "the longer lifetime of CO2 makes the impact of perturbation to the carbon cycle and atmospheric concentration slower to emerge". Factors that affect the cooling effect of CO2 capture include the inertia of the ocean,

the response of the global carbon cycle, etc. I don't know what does the longer Co2 lifetime means here. The last sentence point to Table 2, column 3. I don't know where to find these results? Also, the author saying "if only the land variables are considered". Do you have any explanation for this?

Line 54 to 56: From the time series plot, you showed a decrease in precipitation by comparing results between WACCM_SulfurInjection (2086-2095) - WACCM_SulfurInjection (2010-2019), but in table 2, the decrease in precipitation for end of century is 121.0 mm/day compared to WACCM_RCP8.5, smaller than the increase in WACCM_RCP8.5 compared to present day (123.2 mm/day) value. If I understanding this correctly, the present-day value should be the same for WACCM_RCP8.5 and WACCM_SulfurInjection because they branched from each other in 2020. So why is there a decrease instead of a slight increase in precipitation (am I reading this wrong here)?

Line 102-104: It is clear from Fig.4d that the P/PET index has different changing rates before and after 2050 and since changes in PET is quite consistent throughout the simulation, the difference is mainly caused by different responses in P (Maybe you should do the same calculation and see how much these two terms contribute to the difference in P/PET). However, the authors attribute these differences in P simply to "when the surface cooling starts to emerge, the precipitation also weakens in response to Sulfur Injection". What do you mean by saying "the surface cooling starts to emerge"? The changes in P/PET results seem interesting to me, but I don't think the authors explain these changes in an appropriate way here. I don't even understand what you mean by saying "when the surface cooling starts to emerge" here?

Section 5 investigated the contribution of P and PET to the change in P/PET for different scenarios. For the discussion of P, you compared the precipitation results of stratospheric sulfur injection with tropospheric SO2 increase, and then say "the larger slope of precipitation for Sulfur Injection is expected." Could you directly compare these two results under different forcing designs? I am not sure how could you then get the conclusion that “the model dependence is the largest uncertainty” when you are not comparing the same kind of forcings? Also, for the regression results, did you check the standard deviation for these regression results? How robust the regression results are? It is interesting that global major land PET scales almost linearly with global mean T. If the PET is such tightly related to the T change, why is there a difference in PET slope between these two forcings? I don’t think the authors explain it in a reasonable way. If you attribute this also to model difference, then maybe you should use the same model to do the comparison.

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