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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 #2

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Xu et al. compare the changes in aridity under a worst-case warming scenario (RCP8.5), an extreme mitigation and carbon dioxide removal scenario (that I'll refer to as CDR) and the GLENS sulphate aerosol injection scenario (that I'll refer to as GLENS). They combine results from 2 versions of the same base model (with importantly different climatologies) CESM1, which ran the CDR scenario, and WACCM, which ran the GLENS scenario, to assess the surface hydrological response to these different scenarios. To make the two ensembles comparable bias-correction is applied such that both models are adjusted to match the observed climatology. The authors evaluate a range of hydrological measures and settle on P/PET as their choice of aridity measure. Focusing on this metric they evaluate changes over the global land area (I'm

guessing this is what is referred to as "major land") and across the Americas, and evaluate the differences between the CDR and GLENS response. They also normalize the response by global temperature to address a mismatch between the CDR and GLENS experimental objectives. They find that RCP8.5 produces a general aridification over the land and over the sub-regions they focus upon, driven by a large increase in PET with regionally divergent P responses. They find that while CDR generally partially offsets these trends, GLENS goes too far producing a substantial net reduction in PET that more than offsets the reductions in precipitation. The Amazon is an exception for GLENS, where instead of producing a net increase in P/PET instead it only partially offsets the substantial reduction seen in RCP8.5.

General comments

My main reservation about this paper is that I don't believe comparing these scenarios is policy-relevant or that scientifically useful. The title sets this up as a competition (vs.) between two geoengineering options but these policies are complementary. The CDR scenario (carbon capture as it's often referred to) is also not primarily a CDR scenario but largely an emissions reduction scenario as compared to RCP 8.5 with net negative emissions only after 2050. The other issue is that the experiments were designed to achieve different objectives and produce quite different radiative forcing and temperature responses over the analyzed period and were run in model-versions that differ in quite important ways. Both of these factors undermine the utility of this comparison. That said, clearly a lot of careful analysis has been done and so I'll continue to my other comments.

Much of the paper is written as if the two experiments had the same aim but they didn't. The methodology doesn't make the different goals of the two experiments clear enough and their high-level difference ought to be described up front, i.e. section 2.2 should be elaborated upon to make clearer the experimental setups and their differences. There is often reference to "lagging effects" of the CDR experiment but given that it had different ends from those of the GLENS experiments it seems inappropriate to

describe the temporal evolution as if it were trying to achieve the same thing.

I was surprised that transpiration wasn't discussed as this is a major driver of terrestrial hydrology. I strongly recommend considering the direct CO2 physiological effect of CO2 in these simulations as this will be a big difference between CDR and RCP8.5 / GLENS. The authors report results showing PET and ET over land, finding that they both increase in lockstep. This was surprising to me, given the results of Swann et al. (2018) who found that PET was projected to increase in models while ET was not. Swann et al. found that this was due to reductions in transpiration due to CO2 fertilization offsetting the meteorological drivers that drove PET increases. This suggests that the CO2 physiological effect has not been included or is very weak. Please provide details and analysis to address this. Relatedly, it is not clear whether there were any differences in land cover between the CDR scenario and the RCP8.5 / GLENS scenarios. This could have a large effect on regional hydrology.

The temperature difference between the two experiments should be made clear, from table 2 I can back out that CESM-1.5C is 0.7C / 0.5C warmer than the baseline whereas GLENS is 0.1C warmer (is this right? I thought it was designed to perfectly offset warming from the baseline period). Given the lower sensitivity of CESM, this 0.5C figure would be larger if both were run in WACCM. This could be driving some of the difference between these two experiments and should be brought out and discussed. I'd like a clearer sense of the magnitude of the bias-correction overall and of the regional character of it in your study region. The fact that WACCM has around 25% more precipitation over "major land" (undefined as far as I can tell) ought to be highlighted! Beyond this the regional biases ought to be made clearer. Are the models far off in key regions such as amazon? How wrong are the arid and semi-arid areas calculated using the models uncorrected P/PET values? How different are the regional biases? An extra figure or two is needed to make this clear.

The temperature normalization section doesn't seem to add much to the paper and has some serious problems. I understand that it could be useful for comparing scenarios

with different levels of cooling if it were not for the difference in the climate sensitivity. WACCM's climate sensitivity looks to be $\sim$20% higher than CESM's which will mess with this normalization procedure. I'd suggest either cutting this section (what do we learn that isn't covered elsewhere?) or addressing this climate sensitivity issue by testing how different the RCP8.5 sensitivities are between these 2 models. I worry that model differences rather than scenario differences could be driving some of the response seen in this section. The section summaries seem unnecessary.

I think it would be more fair to describe the "carbon capture and storage" scenario as an extreme mitigation scenario. A reduction in positive emissions makes up the bulk of the difference between RCP8.5 and this scenario. If you wish to highlight the use of carbon dioxide removal (CDR) or the presence of negative emissions then I'd suggest using these terms instead of carbon capture and storage as this technology can be used without producing negative emissions, i.e. on coal power plants. I'd suggest coming up with some clear shorthand for the experiments and using it consistently in both text and figures, e.g. CESM-RCP8.5, CESM-CDR, WACCM-RCP8.5, WACCM-GLENS, Baseline. The figures have inconsistent labeling, line colours and styles and some of the captions are oddly formatted.

The manuscript text needs a careful proof-read by a native English speaker. There were too many grammar mistakes so I only addressed the worst. There were also many very short paragraphs that could be merged with their neighbors.

Specific Comments

N.B. Specific comments are given in the order that they appear in the manuscript with the line numbers as I saw them. It seems the pdf has cut off the hundreds part of the line number and I haven't converted the cycling line numbers into something more sensible.

Title – Given that you stress in the conclusion that you aren't trying to evaluate which is better and instead are focused on the mechanisms, I'd suggest: "climate engi-

neering and aridity in the Americas: a comparison of carbon dioxide removal and sulphate aerosol injection" 22 – I'd argue that this paper hasn't used a consistent framework: different models and different objectives for deployment 22-24 - Given that this is more a mitigation scenario than a pure-CDR scenario, it's not correct to describe this as the first paper to compare sulfate geoengineering against "carbon capture" as previous studies have made such comparisons: [Niemeier et al. 2013: DOI:10.1002/2013JD020445; Muri et al. 2018; DOI:10.1175/JCLI-D-17-0620.1; Jones et al. 2018, DOI: 10.1002/2017EF000720, etc.]. 32 – what does "mitigation potential" mean in this context? 39 – does it worsen the trend or is the trend worsening under RCP8.5? 67 – Vaughan and lenton 2011 don't provide evidence of investment, do they? 68 – I thought CDR installations would be effectively independent of emission sources. CCS is installed directly onto power plants, etc. but that's classed differently. 89 – worth comparing that to the "dry-gets-drier" pattern of global warming Sect. 2.1 This is a very short model description, and includes only one citation to one of the model versions used. Please elaborate. Sect 2.2 Both experiments need to be described in more detail. 30 – "to stabilize TEMPERATURE at 2020 levels. . ." would be more accurate. 35-41 – Is this based on another RCP, How large are the negative emissions, are there any differences in the land surface (more forest cover, etc.)? This description leaves out crucial details. Please outline them here even though they are explained in detail in the Sanderson paper. Sect 2.3 – This is too little material for a sub-section, so I'd suggest cutting it or else expand it to discuss more of the analysis approaches employed, e.g. the time-periods covered and the procedure to normalize by global temperature change that is employed later. 65-59 – This seems out of order, I'd suggest moving figures 1 and 2 out of the methods section and into the results section. Figure 1 – It would be much clearer to color one axis red and the other blue and use dashed versus normal to separate the experiments, then one could read the figure at a glance rather than having to get half-way through the description to know what is shown. Figure 1 – This shows ET and PET increasing in lock-step but Swann et al. 2016 (www.pnas.org/cgi/doi/10.1073/pnas.1604581113) showed all climate mod-

els they investigated diverging with PET rising rapidly and evaporation not rising at all. Swann et al. argued that this was due to the direct physiological effect of CO2 suppressing transpiration. That would suggest that there is no direct physiological effect in your simulations, is that correct? If not, can you explain the difference between your results and those of Swann? Figure 2 – "significant" – please elaborate here or in the methods. L80-84 – These descriptions are incomplete. I cannot tell whether this is a reasonable approach as the terms are not defined. 89-99 – This paragraph is hard to follow, I'd suggest revising it. 89-91 – Is the same bias correction applied to both models or are the separate historical bias corrections applied to the future in both models. 93-94 – I don't believe this is the same "present-day" as in the GLENS experiments which I believe aim to keep conditions fixed at their 2010-2030 levels. Section 3.1 – The sub-section title seems to be at odds with the section title, perhaps change the section title? Figure 3 – What is "major land"? Fgiure 3 – The change in axis range between b and d should be avoided. You should make clear visually or in the text that WACCM's precipitation over "major_land" is $\sim$25% too high. This is huge! 10-11 – What is WACCM's climate sensitivity? If it's not known then I'd reverse the order and highlight that it's higher than the already-high 4 C of CESM. 13-14 – This is the first time that this has been mentioned, I'd suggest highlighting this fact in the methods section. 16-20 – The off-tropics injection is irrelevant as if the sulphates were less effective more would have been injected. Of course there's also the afore-mentioned high climate sensitivity, which should be mentioned. 22 - "global major land" needs to be defined. 25-28 – Isn't it better to describe this as a fundamentally different experiment given their different aims? The carbon capture and GLENS experiments have different ends that end up producing a roughly similar temperature response, i.e. little change from 2020. Table 2 – Seems unnecessary, suggest cutting. You've already shown this in figure 3, and reported many (perhaps too many) of these figures in the text. 29-31 – These lines are unclear, larger than what? Also unclear what is being referred to. Is column3, mid-century P or is it all of the PET results? Do the variables count as a column? 35 – I'd avoid the term "mitigation" given its use to refer to emissions cuts in

the climate literature. I'd suggest renaming this "mitigation potential" to something else. 37 – explain that this is a reduction of a precipitation increase not a 100% reduction in precipitation. 38-63 – These paragraphs on the global-mean precipitation response ought to be revised, they are not well written. It is also strange to refer to an almost perfect reversal of RCP8.5 precipitation trends as being over-effective. L63 – Could you state what "this precipitation-centred argument" is or else reframe this. L67 – I'd avoid referring to sulfate injection as a mitigation scheme. L67 – why aspire to do it and not just get on with it? 70 – does it? I see a drying in all regions in P/PET and a wettening trend in most places except the amazon and central America. 73-75 – this sentence is mangled. 75-77 – is this in the RCP8.5 experiment? 77 – "bring forth the benefit of curbing aridity worsening" 89 – I'd leave this type of commentary until the discussion as its tangential to what is being described. 95-96 – Better to describe this as changing the sign of the trend as reversing is ambiguous in this context, i.e. it could mean simply offsetting. 98-99 – the shorter response time doesn't explain this. P/PET rises rapidly then stops rising, Why is this? Figure 4. Again, these experiments are so different in character it seems odd to compare their time evolution. They have a different temporal evolution of forcing (in ways not described or explored in this paper) so its unsurprising that they have different temporal evolutions of climate response. Section 3.4. I'd avoid the generic sub-section title "summary" and give something specific to this section. 10-14 – As I mentioned earlier, the results in figures 1 and 2 do not show the same pattern for the different variables so I don't think it fair to say P/PET stands in for all measures of aridity as this text implies. 18 – where is the CO2 captured results? 21 – This type of discussion should be saved for the discussion section. Section 4, how about "hydrological change in the Americas"? Figure 5. This figure has too much going on and should be split or else the mid-century results cut. The WACCM RCP8.5 results are missing which are crucial for interpreting the WACCM GLENS – RCP8.5 anomalies plotted. I'd suggest replacing the bias-corrected model results with the original model results. It is unsurprising that the bias-corrected results look very similar to the observations. The models simulated present day has a direct bearing on its projections for

the future whereas the bias-corrected present day doesn't. I'd also suggest making the CDR and GLENS anomalies relative to present-day rather than RCP8.5. Finally, The caption format shouldn't include paragraph breaks. 43-45 – which sees this to a lesser extent? Is the global value "major land"? 46-48 – this seems redundant Table 3. The formatting / caption could be clearer here. Is CESM historical the same as historical after bias-correction, are they similar? 51 – make clear that you are referring to the global arid area. 4.2. This whole South America section is poorly written, I'd suggest revisiting it. 61-62 – revise this sentence. 65 – I'd suggest using 2 significant figures here. What does the 15-30% refer to? 66-68 – Is the amazon really the only region in the Americas to see a decline in precipitation in RCP8.5? Figure 6 – Given the differences in the climates of the regions under investigation would it not be better to report results in percentage change terms? There's a similar $\sim$50 mm/day increase in PET in West US and Brazil but presumably they have very different absolute values. 73-74 – this isn't a good description of what is in the figures, P is up in all regions in the CDR experiment which is just different from what is going on under RCP8.5 and around 80-90% of the PET response is offset. 79 – why not open with the big picture for GLENS then address this exceptional response? 83-85 – it's not consistent with Simpson et al if it's the same data, it's just the same thing. 94 – continues? When did it start? 96 – There's a larger reduction in PET in GLENS, so the differences should be due to the differences in the precipitation response. Should probably note that the precipitation reduction in CESM-RCP8.5 is around twice as large as in WACCM-RCP8.5. 00-01 – Which regional trend is being referred to here? 11-13 – Again, what is being referred to here? Section 4.3. Again, I'd avoid using "summary" for a sub-section title. 37 – it's not a further decrease in precipitation if the decrease in precipitation is less than in RCP8.5. 50 – again, I'd suggest avoiding using mitigation in this way: "mitigation capacity" 52 – Presumably WACCM-CDR (if you had run it) would be about 20% less effective than CESM-CDR by this measure as WACCM has a higher climate sensitivity and so the response would be divided by $\sim$6C rather than $\sim$5C. Figure 7. I don't see any value in including this figure. How is the statistical test applied here? 56-59 – This

is a result of not changing the scales of the plot to reflect the fact that you've divided it by $\sim$5. 62 – Higher climate sensitivity is the obvious driver of this but isn't mentioned. Figure 8. It is not clear what is going on in this plot. Which model is plotted? Are these the bias-corrected results or not? This plot needs to labeled more clearly. I'd suggest producing a similar plot for the RCP8.5 results of the 2 model versions as I suspect that they could look quite different and this might be driving some of the difference attributed to the forcing differences here. 85-87 – missing citation to earlier work. 90-91 – missing again. 95-97 – it could also be that tropospheric aerosols are concentrated over the land. 06-07 – rather than reversing, I'd recommend "changing the sign of" or some other construction that's less ambiguous. Table 5 – The formatting is not great, I don't think the X / Y format is the best choice. The simulated PET value should also be reported as well as a total or mismatch column. 18-22 – This paragraph should be revisited. 26 – this is not a counter-argument, P/PET is a common measure of aridity P is not. 29-33 – The experiments are very different, this paragraph describes them as if they had the same goals. 35 – I'd suggest "less effective at offsetting the amazon drying"? 40-47 – This difference in climate sensitivity also undermines the normalization procedure in section 5.

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