Peer Review, Liu, et. al., 2022: PInc-PanTher estimates of Arctic permafrost soil carbon under the GeoMIP G6solar and G6sulfur experiments

The authors use the PInc-PanTher tool to analyze the simulated effects of solar geoengineering (SG) on permafrost carbon. The authors consider six different Earth system models (ESMs) and four different scenarios: moderate emissions (SSP2-4.5), high emissions (SSP5-8.5), high emissions with dimmed sunlight (G6solar) and high emissions with stratospheric aerosol injection, or SAI (G6sulfur). The authors quantify permafrost area, carbon stocks, and economic impacts in each case, and they find that all six ESMs show statistically significant impacts in both SG scenarios.

I thank the authors for the opportunity to review their work. The permafrost-carbon-climate feedback is a critical yet relatively ill-quantified consequence of global warming, and the possible impacts of SG on permafrost carbon are even less well understood. To my knowledge, there have been very few multi-model analyses of the potential effects of SG on permafrost, and this one is well-designed and well-written. My comments are relatively minor, and they largely address word choice, grammar, and clarity. I recommend the manuscript be accepted for publication with minor revisions, and I do not feel it necessary for me to review it again. Specific comments are included in the attached document.

Abstract:
- Line 10: I would be cautious of referring to circumpolar permafrost carbon as a "tipping point"; Schuur, et. al., (2015) argues that while such emissions would be a "significant source" of carbon over decades and centuries, there is little evidence that thawing permafrost would release a "catastrophic" or "large and rapid pulse" of carbon. As such, I would lean towards phrasing like "irreversibility," "positive feedback," and "significant" rather than "tipping point," which implies a sudden or catastrophic change.
- Line 15: There are a lot of numbers in this sentence, which makes it hard to read. Rather than just listing numbers, could you order and phrase them in a way that makes them easier to process? For example, "Under SSP2-4.5 and SSP5-8.5, respectively, permafrost area decreases by ___ and ___ by 2100, and carbon stocks decrease by ___ and ___. In comparison, under G6solar, permafrost area decreases by only ___ and carbon stocks decrease by only ___; under G6sulfur, they decrease by only ___ and ___." Alternatively, you could omit the raw numbers altogether, and write something like

"we find that G6solar mitigates X % of the permafrost area loss for SSP5-8.5 relative to SSP2-4.5".

- Line 18: This sentence is unclear because the subject of the first phrase lacks an object, and it took me several reads to understand the meaning. Either write out the object twice (e.g., "X raises soil C conservation, while Y lowers it, with the net effect of…") or write in the passive voice (e.g., "Soil C conservation is raised by X and lowered by Y, with the net effect of…")

- Line 21: Here and throughout the paper, "ESM" should be used when referring to only one model, and "ESMs" should be used when referring to more than one.

- Line 24: I talk about this more in my comments on the discussion/conclusions below, but I strongly advise cutting the last part of this sentence. This is a purely technical paper and not a social science one; while using the potential economic benefits of SG as income for Indigenous communities is an interesting idea, without any citations, such discussions are pretty far outside the scope of this paper. The abstract especially is much stronger if you stick to the results of the paper, and I'd recommend stopping after "...$20 trillion in economic losses by 2100," which is a very strong result to end on.

Introduction:

- Line 39: Again, I disagree with the characterization of the permafrost-carbon feedback as a "tipping point". I recommend either using different language, or finding other sources to support this characterization. When describing a tipping point, the author cited here (Linton, et. al., 2019) cites his own work (Linton, et. al., 2008), but in that earlier article, Dr. Linton defines a tipping point as "a critical threshold at which a tiny perturbation can qualitatively alter the state or development of a system"; Table 1 of that 2008 article lists the critical threshold of permafrost as "missing", and in Appendix 2, he writes "no studies to date convincingly demonstrate that [permafrost] is a tipping element by our definition". As cited above, more recent research (Schuur et. al., 2015) argues that, while significant and irreversible, the permafrost-carbon feedback will likely scale incrementally over time and with warming, rather than abruptly.

- Lines 51-54: I dislike that a political stance is used in these lines to justify this study's purpose. While the statement "the best option for mitigating climate change is to aggressively cut GHG emissions by switching to clean energy sources" is probably true, and I agree with it, the word "best" is highly unscientific without substantial support. Additionally, the number of other scientists studying SG does not make this study more

or less important. There is plenty of justification for this research based on scientific merit alone, and I recommend cutting these lines entirely and sticking to the argument begun in lines 48-50 and continued in lines 54-59: SG is potentially faster, cheaper, and technically easier than mitigation and CO_2 removal, past studies (e.g., Chen, et. al., 2020) find that it might work, and you improve on them here.

- Line 64: Comma between "injection, respectively."
- Line 68: Can you add more detail about the simulations you're analyzing from each ESM? Based on the rest of the study, each ESM appears to have one ensemble member for each of the four scenarios run from 2015-2100, but adding a sentence here (or earlier in this paragraph) would help clarify things, rather than having readers guess based on the plots or go and look up the G6 papers.

Materials and Methods:

- Section 2.1 as a whole: I would consider combining this with section 2.2, and possibly even moving Table 2 to supplementary, as discussed below. After reading the entire paper, it seems to me that the initial C stocks are listed and discussed to explain model differences and provide justification as to why bias correction is necessary, but they are not actually needed for the analysis, as in Lines 149-150, you say that PInc-PanTher only needs TSL, NPP, and RPE, and one of the models also doesn't report initial C stocks. If this is correct, this is good background information and still worth including, but consider consolidating and combining sections 2.1 and 2.2 and moving Table 2 to supplementary. (If this is incorrect, and initial C stocks are used directly by PInc-PanTher, please explain how you can conduct the analysis for the model whose initial C stocks are not reported!)
- Line 83: This sentence needs to be split up, e.g. "CMIP6 ESMs have many improvements, including X, Y, and Z. However, estimates still vary across models, which has been associated with deficiencies in the representation of Q and R."
- Lines 87-88: Not critical, but can you provide any more information on how the other models estimated initial carbon stocks if they didn't use observations? Nothing too detailed, but a few words here would be helpful if you can find them.
- Lines 91-94: "Can be" explained, or "is" explained? According to Table 2, the other models don't underestimate soil C loss; they show no losses at all. I don't understand the cause-and-effect you're trying to argue in the last two sentences in this paragraph; whatever you're trying to say, please try to explain it more clearly.

- Table 2: As mentioned above, consider moving this table to supplementary - the differences between initial carbon stocks are interesting, and mentioning them is important to explain why bias correction is necessary. However, since PInc-PanTher doesn't appear to use them (the numbers are missing for one of the models, and you're still able to conduct your analysis!), they're probably not necessary to include in the main body of the paper.

Results:
- Line 205: The comparison of your results in 2100 to those of McGuire, et. al. (2018) in 2300 raises an eyebrow. You mention that McGuire simulates faster rates of warming, but do the differences between RCP4.5/RCP8.5 and SSP2-4.5/SSP5-8.5 really justify comparing periods 200 years apart? If it does, you should include some more numbers to explain why (what are the rates of warming, exactly?), because otherwise there's a bit of comparing apples to oranges going on here. McGuire finds that, in their study, significant losses of carbon wouldn't occur until after 2100; perhaps digging into that would be a more apt comparison here or in the next subsection.
- Table 3 (and other tables, where applicable): Putting a description of the whole table - "Cooled TSL (℃)" - as the header of the first column is confusing. That information is already in the table description, so I recommend writing "scenario" or similar as the header of the first column.
- Figure 5: What defines the boundaries of the "permafrost region" in this figure? In the paper as a whole, the term appears to refer generally to the area at high latitudes where permafrost can be found, but in this figure, every panel seems to have exactly the same region outlined in color. More specifically, does the red color indicate regions that qualified as permafrost in 2015, or some other period, according to PInc-PanTher, but are no longer permafrost as of 2100?
- Line 237: It's a bit picky, but I'm not sure I would qualify 81 ± 8 vs. 92 ± 17 as "close"... maybe "similar to," "the same order of magnitude" or "within the bounds of uncertainty"?
- Line 249: "the spatial distribution is controlled by latitude and initial C stocks" - I thought all of the PInc-PanTher simulations used identical initial C stocks? This is implied by Lines 149-150, which says that only TSL, NPP, and RPE from each ESM are used; by Lines 154-155, which describes initial C stocks for PInc-PanTher; and by the fact that initial C stocks for one of the models are missing. If this is correct, then consider revising

this statement slightly, perhaps saying that distribution is related to initial C stocks and controlled by the initial distributions of the variables actually fed into PInc-PanTher.

- Section 3.3 in general: Lee, et. al. (2022, https://doi.org/10.1002/essoar.10512047.1) examined high-latitude stratospheric aerosol injection and its effect on permafrost in CESM2-WACCM. That study only used one climate model, but they also looked at the effects on soil carbon vs. vegetation carbon; it may be worth comparing your results here, or in the first or third paragraph of the conclusions.

Discussion and Conclusions:

- Lines 344-345: Can you qualify this statement from McGuire a bit more? Gains in vegetation C might outweigh losses in soil C in the near future, but is that true for any scenario, over any period of time?
- Line 359: Need an "and" after the comma
- Lines 360-end: I'm extremely nervous about telling historically marginalized communities what they "should" do, especially in a purely technical paper. You make some good points here, but as this is an earth science paper and not a social science one, I would strongly recommend leaving most of this implied, but unsaid - you've laid out the evidence for the economic benefits of SG; best leave it up to the social scientists to ponder what to do with them. I would cut this entire block, but if you do want to leave part of it in, I advise sticking to objective sentences that would be very difficult to disagree with.

References:

- Line 467: There's a typo in the DOI for McGuire, et. al. (2018): the DOI has a space before the last two numbers, which makes it a bit harder to follow the link.