Reviewer 1 (Jörg Schwinger) Comments:

The authors present an analysis of the CMIP6 ScenarioMIP SSP5-8.5 and SSP5-3.4-OS scenarios and their extension to 2300, which have been simulated by 4 ESMs and one EMIC. This pair of scenarios initially follows the same pathway, but diverges after 2040. SSP5-8.5 represents an extreme case of increasing fossil fuel use, leading to very high atmospheric CO_2 concentrations, while SSP5-3.4-OS assumes aggressive climate mitigation after 2040 including a large amount of net negative CO_2 emissions. The authors focus on the long term (beyond the 21st century) climate and carbon cycle response in these contrasting scenarios. Carbon fluxes between the land surface and the ocean, the transition of these fluxes between source and sink, the surface temperature response, and the proportionality between warming and cumulative carbon emissions are investigated. The main findings include a very large model uncertainty in the land components of the ESMs, which is particularly pronounced for the high emission scenario.

This manuscript is an important contribution to the analysis of the wealth of CMIP6 model data, and the first one (to my knowledge) describing results from the SSP-extensions. It fits well into the scope of ESD and will be of interest to a broad readership. The manuscript is generally well written and well structured. I did not find any serious problems with the manuscript, and I recommend it for publication in ESD after a few rather minor comments and suggestions have been considered by the authors.

We thank the reviewer for their positive assessment of the manuscript.

Main points:

1) The treatment of land-use/land-use change emission in this analysis could be explained better, particularly in the context of negative emissions and BECCS. The negative emissions in the SSP5-3.4-OS are generated (mainly?) through BECCS, I assume, at least there is a massive expansion of cropland in the overshoot scenario (more than a doubling between ~2050 to ~2070 at the expense of pasture, O'Neill et al. 2016, Fig. 4). On the other hand, the ESMs employed here will not represent anything like BECCS. At most, I guess, they will represent harvesting of crops (but release this carbon back to the atmosphere)? There is only limited information on this issue in the manuscript. The sentence in line 105-107 is unclear to me, particularly the part "thus can be separately inferred..." - but this inference is not done in this work? It would be useful to know how (if?) the massively increased crop area in the overshoot scenario is treated in the models, and what this implies for the analysis.

Related to this, in section 3.3 the authors diagnose fossil fuel and industrial emissions and show that these compare reasonably well with the IAM emissions. This must mean that the carbon uptake by bioenergy crops for BECCS in the IAM is counted as "fossil fuel and industrial"? Also, in Fig. 4b, the large and quite abrupt change in cropland area in the overshoot scenario seems to entail no land-use change emission flux (land-use change emissions are part of the depicted flux, are they?). Is this because a transformation of pasture to cropland has no large effect? This would probably be different if the cropland expansion would happen on the cost of forest?

It would be nice if some of these aspects of negative emissions and how ESMs represent or do not represent them could be covered in the methods section (in the existing subsections or maybe add an extra subsection).

This is a very good point and we will add further discussion of this in the manuscript. In particular, we will plan to include information in the description of each model for (a) whether they treat pasture and cropland as distinct land-use categories, (b) how they handle the transition between pasture and cropland, and (c) if and how carbon fluxes associated with BECCS are treated differently from carbon associated with non-BECCS crops.

2) The discussion of the role of non-CO_2 forcings in the TCRE relationship could be a bit more comprehensive. It would be interesting to know the long-term development of non-CO_2 forcings as specified in the scenarios (i.e., fixed at 2100 levels or linearly declining, etc.). Currently it is only mentioned that the fraction of non-CO_2 forcing is larger in the early parts of the scenarios (lines 310-311), maybe the authors can expand this a bit.

We will add further discussion of this point in the manuscript. The non-CO_2 timeseries for these long-term scenarios can be seen in figure 2 (emissions), figure 7 (radiative forcing), and figure 8 (concentrations) of Meinshausen et al. (2020, https://doi.org/10.5194/gmd-13-3571-2020)

3) The manuscript would benefit from moving the individual model descriptions to an Appendix. Instead, in the methods section, only a summary could be presented, but this should contain key features like the inclusion of nitrogen cycle, dynamic vegetation, and soil physics (which could be moved from the Discussion-section lines 459-464). Also, this would be a place to say something about how land use change is handled in the models. This is a personal preference, but I think this way it would be easier for the reader to grasp the most relevant differences between the models.

This is a good idea and we are happy to make such a change in the manuscript.

4) Finally, I find the "23rd Century surprises" in the title is pushing it a bit. The authors argue that the "lack of agreement among land models on the mechanisms and geographic patterns

of carbon cycle feedbacks, alongside the potential for lagged physical climate dynamics to cause warming long after CO_2 concentrations have stabilized, point to the possibility of surprises in the climate system beyond the 21st century time horizon, even under relatively mitigated global warming scenarios" (abstract lines 36-40). First, I wouldn't agree that the lack of agreement among land models is comparable between the very high and the mitigated scenario - Fig. 1c shows that on the global scale land models disagree much more for the strong forcing (also for Fig. 4b I would argue that the models do show some similarity for the mitigated scenario). Second, the fact that land models disagree widely does not mean that there will be or could be surprises - as long as the model results bracket the real-world behavior (which, of course, we cannot know, but this is a fundamental problem of all our science). It is a personal preference, but I would simply delete "23rd Century surprises" from the title and reword lines 36-40 in the abstract (and the other occurrences where the authors argue for "other surprises being in store" or similar).

As both reviewers agree that the current title is not working well for the manuscript, we will change it in revisions, most likely to something like “Multi-century dynamics of the climate and carbon cycle under both high and net negative emissions scenarios”.

Minor points:

lines 26-27: "...followed by stabilization of atmospheric CO_2 concentrations by means of large net-negative CO_2 emissions." This is not very precise: the large net-negative CO_2 emissions do not stabilize atmospheric CO_2 but decrease it.

We will reword this in revisions.

line 30: "climate-carbon feedbacks" - I suggest replacing this by the more general term "carbon-cycle feedbacks" (for the ocean a large part of the weakening comes from reduced buffer capacity of the upper ocean, which (in this terminology) is a "concentration-carbon feedback"

We will reword this in revisions.

line 232-234: "This implies a substantial slow component in the models which continue to warm past the period of CO_2 stabilization, beyond the effective transient values reported above..." The TCR values reported above apply at $2xCO_2$ in the 1% scenario, I do not really understand the connection here. Whether or not one would expect continued warming depends on remaining (implied) CO_2 emissions, non-CO_2 forcing and the ZEC of those models. Could the authors please clarify this?

We will add more detail on this point in revisions.

Technical:

We will make each of these editorial changes below in revisions.

line 25: I suggest deleting "second"

line 76-77: Please check and revise this sentence, it does not make sense to me.

line 102: "...global-mean timeseries, but do not feed back on atmospheric CO_2 (Fig. 1a)." -> "...global-mean timeseries (Fig. 1a), but do not feed back on atmospheric CO_2."

line 223: use the degree-sign instead of spelling out "degree"

line 259: "..., and consistent ..." -> "..., and which is consistent..." (but this is also a very long sentence, consider splitting in two)

line 347: "near-future time period" - please be more specific, up to 2040 or 2050?

line 435: "to emissions" -> "to cumulative emissions"

line 490: "unless behavior can also be constrained" - please check the grammar of this sentence (maybe consider splitting in two)