EGUsphere, referee comment RC3
https://doi.org/10.5194/egusphere-2022-82-RC3, 2022

Comment on egusphere-2022-82

Anonymous Referee #3

Referee comment on "Evidence of localised Amazon rainforest dieback in CMIP6 models" by Isobel M. Parry et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-82-RC3, 2022

General comments

Amazon forests play an important role on the Earth's carbon cycle. Previous modelling studies have suggested that a widespread forest dieback could occur as climate changes, but this remains highly uncertain. In this study, Parry et al. investigate the risks of such dieback in more recent models, by comparing the predictions of seven CMIP6 models under increasing CO2, and identified that about 7% of the Amazon could experience dieback once global temperature exceeds 1.5°C above the historical average.

Overall, I think the research question is highly relevant and can eventually provide important insights on the likelihood of significant and persistent changes of tropical forests under climate change. However, in its current form, the manuscript lacks important analyses and discussions on the mechanisms that result (or do not result) in significant forest losses in the models, and what are the main factors that cause the different outcomes across the models. To give one example, the authors mention that some of the diebacks are mediated by fire, but from reading the manuscript it is unclear which models predicted dieback due to fire, which models predicted dieback due to physiological/hydraulic failure, or even if the models that resulted in no dieback were also the ones that did not have fire. I list other instances in the specific points below. If the authors can strengthen the discussion and analyses in this direction, I think the manuscript could become a significant scientific contribution.

Specific points

Introduction. There are many studies that investigated the risk of critical transitions specifically in the Amazon, and it may be worth including them to provide a stronger motivation for this work. As a starting point, the recent Amazon assessment report (https://www.theamazonwewant.org/amazon-assessment-report-2021/, chapter 24) has an extensive review on this subject.

L49-51. This paragraph could be expanded to provide a stronger motivation for this study. The authors could justify why the current analysis is necessary, and how this study contributes to learning something new about the future of the Amazon to climate change.

Table 1. The authors could expand this table to provide a bit more information of the simulations and models used. For example, they could provide some information about the models (e.g., which ones had fires enabled, which other mechanisms cause mortality and biomass loss), and which variants were used from each model. If citations exist for these models, the authors could add references.

Section 2.3. I found the early warning section disconnected from the introduction. In the introduction the authors describe the theory of critical transitions, critical slowing down and increase in autocorrelation, yet none of these seem to be used in the actual analysis. Was there any reason for not using these established approaches?

Section 3.1. The authors use savannah as the alternate state for forests, and this can be misleading if fire is not the driver for abrupt changes. Also, this section describes the changes across tropical South America, but the authors do not provide any insight on what causes the variability across models. Presumably they also have broad range of predicted climate, presence/absence of fires, and different approaches to simulate drought mortality. Explaining these differences could help us understanding why there was such broad range in dieback responses.

L115–118. My previous comment applies here too. The remark that models largely disagree is correct, but not very informative. I would not expect the authors to provide details about every model configuration and formulation, but I think they could explore some potential causes by looking at other model output data (at the very least precipitation, some insight on model sensitivity to CO2, and some fire and mortality-related variables if available).

Section 3.3. I am unable to see the causal link between CO2 and the abrupt shift based solely on Figure 3. If anything, in GFDL most of the shift in the amplitude of the seasonal cycle seems to occur after the abrupt transition. Also, the difference in the seasonal cycle is very large across models, with NorCPM1 remaining below 4°C for the entire century, whereas the other models show much higher amplitudes. Is it fair to treat the shifts marked in Fig. 3 the same?

Discussion: I support keeping the discussion short, but maybe the current one is a bit too short and narrow in scope. For example, the authors mention that the dieback was present in previous generations of model but not in CMIP6. Why is this the case? Also, how does this result differ from the analysis by Cox et al. (2013) (https://doi.org/10.1038/nature11882), which had already indicated lower risk of a dieback. Also, the results implied that fire is an important mechanism for dieback, and I think the discussion could emphasise this further, considering that fire activity has significantly increased recently in the Amazon. The mechanistic links between increased CO2 and dieback (and the uncertainty in these links) could be discussed in more depth too. I think addressing some of these aspects would help placing these interesting results from the CMIP6 predictions in a broader context.

Minor points

L9. I am not sure I followed this sentence. Does this mean that an additional 7% of the NSA will experience dieback for every 1°C above 1.5°C (i.e., if the temperature change is 2.5°C, dieback will occur at 14% of the NSA, if the change is 3.5°C, 21% of the NSA will suffer dieback and so on?).

L49. Quantify "fairly short observational records"

L55. Include references that describe both CMIP6 and the 1pctCO2 runs.

L67. "Unforced control run" was not described up to this point. Consider describing it in section 2.1

L78–81. This seems to be out of place, it reads more like the caption of Figure 4 (which is referred to before Figures 1–3). Perhaps rewrite this to focus on how sensitivity and dieback risk were calculated.

L87–90. This text repeats what was described in the methods section. I suggest dropping it.

L105–111. "Jumps" seems a bit too colloquial, and it is unclear how it differs from "abrupt shifts", which is defined in the methods.

L175. “Study” is misspelt.

Figure 2. In panel (a), I suggest keeping only NSA, as this is the only specific region analysed in this study. Also, make the labels consistent with captions (e.g., use either abrupt shift or dieback shift), and define the acronyms (AS, cVeg) in the caption.

Figure 3. Why did the authors show different points for each model? Also add “W” after the last 60°.

Figure 4. I recommend adding a colour legend to the figure.