

Interactive comment on “Earth system feedbacks following large-scale tropical forest restoration” by Alexander Koch et al.

Anonymous Referee #1

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Authors compare the standard RCP2.6 emissions driven scenario to a scenario in which everything follows the RCP 2.6 scenario except that anthropogenic land use change is turned off in the tropical regions and in addition vegetation succession is allowed to take place which allows tree cover to expand. The biogeochemical and biogeophysical implications of this “restoration” are assessed. There is sufficient science in the results reported in the manuscript to constitute a reasonable publication. The manuscript is also written in a reasonably decent way. However, a number of points remain unclear, terminology needs to be improved, and the different points in the manuscript need to be tied properly for a coherent story telling.

The HadGEM2-ES is developed by the UK Met office. However, I do not see any authors from the Met office who contributed to this manuscript. This is okay as long

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as the authors themselves understand the structure and the primary processes in the HadGEM2-ES model that are required to explain the results reported in the manuscript. Unfortunately, however, this does not appear to be the case and this is one reason why the manuscript needs clarifications. I have provided hand written comments that the authors may find helpful in improving their manuscript. While, I will summarize most of my primary comments in this review may I please suggest that the authors also look at my handwritten comments in the attachment provided for other minor comments that should help address the overall readability of the manuscript. Answers to several of my hand written comments are in the manuscript but the reason I wrote them is because that information wasn't available when reading a given part of the text. This is an indication that either this information should be mentioned earlier, or a reference to it should be made that it is available later, or the manuscript should be reorganized so as to improve its flow.

Primary comments

1. In context of the terminology, I have two primary comments. First, I find the use of the term “control” to describe the RCP 2.6 scenario somewhat confusing. In the climate and Earth system modelling literature the term “control” is typically used for the preindustrial simulation. I suggest calling the standard RCP 2.6 simulation what it is - the RCP 2.6 scenario. Second, I am confused by the term “restoration”. I am familiar with the terms reforestation, deforestation, and afforestation but I have no idea what does the term “restoration” actually refers to. I understand the intent here but perhaps it would be helpful to clarify the intent more clearly and upfront in the manuscript.

2. Why the emissions driven RCP 2.6 scenario in this manuscript is driven with diagnosed emissions from the concentration driven RCP 2.6 scenario simulation of the HadGEM2-ES. Wouldn't it had been much easier to explain and to drive the emissions driven simulations of the RCP 2.6 scenario with the standard emissions provided by the integrated assessment models for the RCP 2.6 scenario.

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3. I am unclear of the protocol followed in the “restore” simulation. The reason for this is that the manuscript doesn’t show time series of crop [and pasture] area in the “control” and “restore” simulation so I can’t visualize how anthropogenic LUC is avoided in the “restore” simulation. This, I think, is the first aspect. The second aspect is related to the fact that the TRIFFID component of the MOSES land surface scheme is able to simulate the fractional cover of its plant functional types (PFTs) dynamically (note that I am not calling it “dynamic vegetation”). I am unclear how this second aspect works. Is the model allowed to dynamically simulate fractional cover of PFTs in areas already deforested and is this the reason for expansion of trees into an area of 1529 Mha?

4. I have no clue what is the purpose of section 1.3. I was not able to understand the context for this section and it appears to come of the blue.

5. At a number of places in the manuscript, it is mentioned that the carbon not released by avoided deforestation and carbon sequestered by the expansion of tree cover does not yield the expected reduction in atmospheric CO₂ burden because this carbon benefit is overwhelmed by negative feedbacks. These negative feedbacks include reduction in CO₂ uptake by the ocean and extra-tropical vegetation due to the reduction in atmospheric CO₂ concentration.

In the context of the earth system, positive and negative feedbacks amplify and reduce the initial perturbation, respectively. For example, the carbon uptake by land and ocean in response to increasing atmospheric CO₂ is a negative feedback since it reduces the amount of CO₂ in the atmosphere caused by fossil fuel emissions. If we use this standard definition/sign/interpretation of feedbacks then it becomes a little difficult to interpret that the negative feedbacks as you call them in your study reduce the climate benefit of “restoration” since in the normal context of climate warming negative feedbacks are the good feedbacks that reduce the rate of climate warming.

Perhaps it would be more clear if the phrase “negative feedbacks” is not used in this context but rather it is explicitly mentioned that “the carbon benefits of avoided defor-

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estation and the increase in forest cover, in the restore simulation, are not fully realized because the resulting reduction in atmospheric CO₂ also reduces carbon uptake by the ocean and extra-tropical forests”.

6. Equation (1) on page 4 is not 100% correct. The reason for this is the ambiguity in the term E_{DEFOR} which represents the deforested biomass according to lines 72 – 73 on page 3. Note that the deforested biomass is allocated to wood product pools with different turnover timescales. As a result, the land-use change related emissions seen by the atmosphere (and thus in equation 1) are not equal to deforested biomass but rather the sum of the fluxes from the wood product pools. Please note and correct this subtlety when revising your manuscript.

7. There are two aspects to vegetation acting as a dynamic component in an Earth system modelling framework. The first is related to changes in the structure of the vegetation including vegetation height, its leaf area index, rooting depth, prognostic leaf onset and offset times and its biomass. These dynamic changes in vegetation structure, in response to changes in climate and atmospheric CO₂ concentration, occur regardless of changes in the spatial extent of vegetation. The second aspect of vegetation dynamics is related to the changes in the fractional coverage of different PFTs. In the manuscript, the term “dynamic vegetation” is used to describe the second aspect. I would suggest to be explicit here (as I have done in point 3 above) and clearly mention “the changes in the spatial extent of PFTs” if that’s what you’re referring to.

8. Lines 356 – 357. You cannot call HadGEM2-ES the most sophisticated ESM. The diversity of ESMs in the climate community is considered a healthy aspect of the community. Several studies have shown that the model mean response to any perturbation is more robust than any individual model.

9. Finally, I think the manuscript can benefit from some reorganization to improve the flow of the manuscript. Perhaps starting with the big picture of changes in atmospheric CO₂ burden and temperature, followed by land C changes, and then finally by ocean

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C changes will be helpful.

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2020-432/bg-2020-432-RC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-432>, 2020.