

## ***Interactive comment on “Collateral transgression of planetary boundaries due to climate engineering by terrestrial carbon dioxide removal” by V. Heck et al.***

**Anonymous Referee #2**

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Review of “Collateral transgression of planetary boundaries due to climate engineering by terrestrial carbon dioxide removal” by Vera Heck, Jonathan F. Donges and Wolfgang Lucht.

General comment

I very much agree with the approach taken in this paper. We have long known that interactions among the 9 planetary boundaries (PBs) are important, but have only made qualitative assessment of these interactions so far. Applying a conceptual modelling approach to exploring a small set of PB interactions around a specific question is an excellent way to approach the interactions problem. And I fully agree that a conceptual modelling approach is an important step, as it allows one to better understand how

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the model is behaving – providing insights into how the system might be operating. The outcomes of this modelling study show how effective conceptual modelling can be in elucidating system-level constraints and trade-offs in a broad sense. The authors are to be congratulating for taking such an important and convincing step forward in developing the PB framework.

Specific comments:

1. Figure 1 is an excellent visual description of the model but it leaves one interesting carbon cycle-climate question a bit unanswered. In many countries, storage of carbon in land systems via reforestation and afforestation (and avoided deforestation) is being used to “offset” fossil fuel emissions. In Figure 1, these activities would be part of the loop “Land-human offtake-land use emissions-atmosphere”. These activities could be considered as “negative” human offtake, or human uptake. But the point – clearly made in Figure 1 – is that such activities clearly remain in the active carbon cycle and can in no way “offset” fossil fuel emissions. It is only when tCDR activities are undertaken, and the transfer of carbon is from Land to GE sink, can carbon originating in land truly offset emissions of carbon from the geological reservoir. Although this issue is not a part of the simulation, it might be worth including a paragraph that discusses this fundamental difference between carbon stored in above-ground vegetation (and thus in the active carbon cycle) and carbon stored in geological formations.

2. The PB for land system change is actually not based on the carbon storage on the three major forest biomes (boreal, temperate, tropical) but rather on the biogeophysical feedbacks of these three biomes to the physical climate system via changes in albedo and evapotranspiration. In the 2015 PB paper we noted that the land carbon issue, which in principle affects all terrestrial biomes (although the bulk of the above-ground biomass in land systems is in the major forest biomes), would be dealt with the climate PB, given that atmospheric CO<sub>2</sub>, a feature of the active carbon cycle, was the control variable for the climate boundary. An interesting off-line calculation might be to fix the land system boundary at 75% of the carbon storage for the three major forest biomes

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(based on potential areas), and then see what this means for carbon offtake for the rest of the terrestrial biosphere. This, of course, would only be interesting for those scenarios in which the land-system boundary is transgressed.

3. Just to follow on from point 2, there is an interesting further nuance to the tradeoff between the climate and land-system change PBs for very high tCDR rates – the scenarios that shrink the MCSOS due to transgression of the land-system PB in order to meet the climate PB. This may actually be counterproductive for the climate system, given that the land system PB is configured around biogeophysical feedbacks to the climate system. If these are disrupted due to transgression of the land-system PB, we may see significant changes in atmospheric circulation, monsoon systems, rainfall patterns more generally, even though the carbon aspect of the climate PB is respected via very high tCDR rates. So there is another interesting trade-off at play here!

4. The biosphere integrity PB (along with climate one of the two core PBs) was only mentioned once, I think, in the manuscript. This is OK, as it is beyond the scope of the study. However, the 2015 PB paper noted that this boundary was more likely to be a bigger constraint on the use of land systems for carbon management than the land-system PB itself (which is rather narrowly focused on biogeophysical feedbacks to climate). There isn't much that can be done yet in a modelling framework with the biosphere integrity boundary, but there are some promising approaches such as the Biodiversity Intactness Index (BII) or MSA (Mean Species Abundance) that are quantitative and could eventually be useful in conceptual modelling frameworks. So this is just a note to say "watch this space", with no action required on the present manuscript.

5. The issue of baseline emission trajectories was a bit confusing in the paper. This is especially important since, according to the conclusions section, managing an SOS depends, in addition to the anticipation of climate change and the potential maximum tCDR, on the baseline emissions pathway. For example, RCP8.5 was used early in the analysis as the emissions pathway (cf. Figure 5), but then Figure 6 switches to a low

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baseline emission pathway, while Figure 7 uses an emissions baseline of ~1600 Gt C cumulative emissions. It is only when we get to Figure 8 that we see the profound importance of the baseline emission pathway for the entire analysis! I think this problem could be rather easily fixed by putting a paragraph upfront in the paper foreshadowing that different baseline emission pathways are used in various points of the paper, and that there are good reasons for this. The para could also foreshadow the important of baseline emission pathway, but that this will be dealt with near the end of the paper.

6. I think the trade-off analyses in this paper are excellent, and are certainly a strong point of the paper. Even though this is a rather simple conceptual model, it yields some fascinating tradeoffs involving anticipation and timing of actions, as well as magnitudes of interventions. In particular, I really liked the statements in lines 394-398 and 427-429. These really show the value of this approach.

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