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

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In this paper, the authors extend a stylised carbon cycle model to include climate engineering by terrestrial carbon dioxide removal (tCDR). The modelling is technically rigorous and the paper is clearly written. There are clear advances in including climate-society feedbacks in a dynamical model and in the methods of analysis.

My main suggestion is that I would like to see more concrete conclusions, for example about the likely effectivness of tCDR and/or about what insights this modelling approach achieved (or even why this modelling approach was chosen). Most of the statements in the abstract and the conclusion are rather empty (e.g. the results of tCDR depends on its parameters; there are trade-offs) or at best could also be obtained by

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simple accounting of carbon stocks or emission rates.

To my mind the main advantages of a stylised dynamical model over simple carbon stock accounting are if the system under consideration has feedbacks or time lags or non-linearities that are crucial to understanding its dynamics. Perhaps this is true in the present case but I don't see it yet, at least not in your main conclusions. Can you see any consequences of dynamics on whether tCDR is likely to succeed (in keeping the earth system within, or moving it into, the SOS)? What is the time horizon on which tCDR has to start? How likely is it that tCDR will cause at least one PB to be transgressed?

Minor comments

- Line 132: It's the difference in partial pressures, not the pool size, that determines atmosphere-ocean flux

- Motivations of changes to Anderies et al.'s assumptions are clearly given, but what about discussing the validity of their assumptions and simplifications that haven't been changed? For example, the linear carbon-temperature relationship, and the single terrestrial and marine carbon stocks (which combine above and below ground and surface and deep ocean stocks, respectively).

- Lines 164-165: Why correct for carbon dioxide dynamics on long time scales? ("50% of the emitted carbon stays in the atmosphere")? Processes removing atmospheric carbon are already represented in the model. I would have thought temperature response to emissions on short time scales would have been more appropriate here. Long-time dynamics will emerge from the model.

- I realise you probably don't have control over this, but I would have preferred Table 2 at the section of section 2.2 rather than several pages later.

- Figure 4 is somewhat misleading. It suggests that the terrestrial biosphere will store carbon all the way to arbitrarily high atmospheric carbon concentrations. But in your

model, above a certain concentration the temperature will be high enough for respiration to exceed photosynthesis and you will have zero carbon storage.

- Line 205-6: Check grammar here.

- Line 215: The planetary boundary is 350ppm (Steffen, 2015). The range 350-450ppm is the 'zone of uncertainty' of the threshold at which dangerous consequences may start to happen. Therefore we have already exceeded the climate change planetary boundary, unlike what is written here and is presented in the figures.

- Line 222: Would appreciate being a little more explicit about how the number 0.31 is obtained.

- Line 225-7: I have no problem with this reasoning, but maybe be explicit about the assumptions on soil carbon. I guess the assumption is that soil carbon is unchanged by deforestation? Is this reasonable?

- Figure 8: Interesting that in (b) and (c) the parameter on the vertical axis needs to be within a narrow parameter range. Why?

- Line 423: The success of a climate intervention "nonlinearly depends" on tCDR effectiveness. This is not surprising; when the aim is to avoid a threshold (a planetary boundary), of course success will be very sensitive to parameters in the vicinity of the threshold. Or is there some other effect you're referring to?

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