

## ***Interactive comment on “Climate feedbacks in the Earth system and prospects for their evaluation” by Christoph Heinze et al.***

### **Anonymous Referee #3**

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This paper is a comprehensive review of the state of knowledge of climate feedbacks in the Earth System. I read the document from the perspective of someone who wasn't necessarily familiar with all the details of climate feedbacks, which is the intended audience. Frankly, since there are so many feedbacks, each with their own nuances and levels of understanding, this is the appropriate perspective to have since no individual scientist can fully understand them all. So, from that perspective, I felt that the level of presentation and discussion was appropriate. I have a few minor suggestions included below. Overall, I thought that the feedback discussion section was better than the feedback evaluation section, though this likely mainly reflects the big challenges that our community faces with respect to evaluating feedbacks with limited and short record observational data.

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The paper is well-written and is structured appropriately. I don't really have too many criticisms or suggestions as the paper was clearly put together in a thoughtful and careful way.

Minor points:

1. P. 3, line 27: replace "climatological timescales" with "climate timescales"
2. P. 5, line 18: I would add 'surface energy budget' into the list of things that LULCC affects. The changes in surface roughness between forests and grasslands/croplands is often one of the most important factors affecting the LULCC impact on climate through the impact on surface energy budget partitioning.
3. P.5, line 24: The comment about "Anthropogenic driving factors such as albedo changes from deforestation, agriculture . . ." seems to be mainly repetitive to the discussion of land use change higher up in the paragraph, but it is introduced as an additional 'factor'
4. P. 13: I found the beginning of Section 3 to be a little bit confusing. Section 3 is meant to be about fast feedbacks, but then there is some discussion in the introductory material of fast versus slow feedbacks and which overall feedbacks are considered. There is also a listing of the four basic feedback types, which are a mixture of fast and slow feedbacks. Maybe there needs to be a separate introductory section about what feedbacks will be considered and not considered. I would advise the authors to consider ways to clarify the text.
5. P. 13: Similarly, the list of the four basic types of feedbacks doesn't transition cleanly/clearly into the detailed descriptions in the following sections. For example, the detailed descriptions don't start with the first basic feedback type (thermodynamic shortwave radiation feedbacks). And, the detailed feedback descriptions tend to bounce around across basic feedback types as well, with even a transition to another section to describe some of the basic feedback types. I'm not sure if it would be

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straightforward to rework ordering or text to guide the reader along a little better (and also not clear how important it is), but thought I would highlight the possible issue to the authors.

6. P. 17: Line 17: “Clouds belong to the prime sources for uncertainty in”. This is strange wording and could likely be improved.

7. P. 24, line 24: I believe that an increase in LAI can lead to an increase or a decrease in albedo. The direction of change depends on the underlying soil albedo. Note that the amplitude of these feedbacks described in this paragraph are highly uncertain, which could be stated, though maybe that is true of many of the feedbacks and therefore would be repetitive to state that there is high uncertainty for many feedbacks.

8. P. 20, Section 4.3.1: From my perspective, there is too much emphasis in this paragraph on the impact of permafrost thaw in increasing methane emissions. Schuur et al. (2015) emphasize that the biggest feedback from permafrost thaw is expected to be from carbon dioxide release as organic material currently frozen or nearly frozen in permafrost soils thaws and decomposes. Increased methane emissions associated with warmer and potentially wetter soils is also a permafrost carbon feedback, but it is not expected to be as large as that associated with CO<sub>2</sub> emissions. Note also that all current estimates of the permafrost climate-carbon feedback have neglected the potentially significant emissions from abrupt permafrost thaw processes. The literature on this is essentially negligible, though, so hard to cite.

9. P. 30, line 32: CO<sub>2</sub> fertilization is not due only to improved water use efficiency of plants. Increased CO<sub>2</sub> uptake by plants under high CO<sub>2</sub> conditions is due to the impacts of CO<sub>2</sub> concentration on plant photosynthetic processes.

10. P. 31, line 5: True, but the models with CN representation in CMIP5 have been shown to have unrealistic behavior with respect to N-limitation impacts on the carbon-concentration feedback (e.g., Bonan and Levis, 2010).

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11. P. 47, Line 3: "Suggest an even larger RANGE of equilibrium"

12. P. 45, Section 5.1.3: It would be worth citing this recent paper (McDougall et al., 2019) that discusses the limitations of 1% experiments to assess feedbacks in ESMs.

13. P. 48, line 10: I would suggest citing the recently published ILAMB paper (Collier et al., 2018) in addition to Eyring et al. (2016c) to indicate the breadth of efforts in this arena of model assessment.

14. P. 49, line 10: The text as written implies at the beginning that the ToE has a 30-60 year timescale. Clearly, as the authors note further down in the text, the ToE depends strongly on which variable and on what spatial scale is being considered. And, another paper on ToE related to carbon is Lombardozzi et al. (2014).

15. P. 51, Section 5.2.5: Seems like this paper that highlights some of the potential limitations associated with emergent constraints should be cited (Caldwell et al., 2018).

16. P. 54, line 1: Perhaps should replace the term "individual modeler" with "modeling groups". Obviously, ESMs are not developed by individuals and decisions are not made about the quality of simulations by individual modelers either.

Lombardozzi, Danica, Gordon B. Bonan, and Douglas W. Nychka. "The emerging anthropogenic signal in land-atmosphere carbon-cycle coupling." *Nature Climate Change* 4.9 (2014): 796.

Bonan, Gordon B., and Samuel Levis. "Quantifying carbon-nitrogen feedbacks in the Community Land Model (CLM4)." *Geophysical Research Letters* 37.7 (2010).

MacDougall, Andrew Hugh. "Limitations of the 1% experiment as the benchmark idealized experiment for carbon cycle intercomparison in C 4 MIP." *Geoscientific Model Development* 12.2 (2019): 597-611.

Collier, Nathan, et al. "The International Land Model Benchmarking (ILAMB) system: design, theory, and implementation." *Journal of Advances in Modeling Earth Systems*

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10.11 (2018): 2731-2754.

Caldwell, Peter M., Mark D. Zelinka, and Stephen A. Klein. "Evaluating emergent constraints on equilibrium climate sensitivity." *Journal of Climate* 31.10 (2018): 3921-3942.

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Interactive comment on *Earth Syst. Dynam. Discuss.*, <https://doi.org/10.5194/esd-2018-84>, 2018.

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