Comment on bg-2022-208

Anonymous Referee #2

- Referee comment on "Effect of land use legacy on the future carbon sink for the conterminous U.S." by Benjamin Seth Felzer, Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-208-RC2, 2022
- This work examined how the initial state of the land surface affects simulated carbon cycles under an expected climate change. This is a kind of sensitivity analysis of a model because no systematic evaluation was conducted using observation-based data. Still, this work clarifies the importance of the initial state on simulations of transient changes in carbon pools and flux. I evaluate this model can be published in the BG if the author appropriately addresses the following issues.

Major issues

(1)This work evaluated results from only one model, "TEM-HYDRO2". Hence results are very model-specific. The author needs to discuss, at least, to what extent the finding in this work can be generalized.

This is an interesting point. After reviewing the full suite of TRENDY models, which serves as a nice intercomparison of leading edge DGVMs, I add that none of them actually include forest demography, though they do include changes in fractional land cover based on the LUC dataset, as well as conversion and product fluxes. I mention that Shiavlokova et al. (2009) does include forest demography using a tiling approach. In response to this comment, I have added a new experiment, HISTCOND, in which I condense all the cohorts in a transient historical run so that it is somewhat analogous to what many of the other models are doing. This (and another new run referenced above as HISTCONST) are now in Figure 2, the Methods, Results, and Discussion. The key paragraph addressing this point in the Discussion is (lines 535-547):

Most other terrestrial ecosystem models do not include the effect of forest demography. The Dynamic Global Vegetation Models (DGVM) included in Trends in Net Land-Atmosphere Exchange (TRENDY-v2) (Li et al., 2017) mostly include annual changes in PFTs to represent LULCC. They include the conversion and product fluxes resulting from these changes, and often include the effects of mortality and regrowth within existing grids, but do not incorporate the effects of forest regrowth due to LULCC. Two of the models (VISIT and JSBACH) (Kato et al., 2013; Reick et al., 2013) include elaborate

methods of applying the LULCC transition matrices to ensure the correct redistribution of PFTs and correct carbon fluxes. Shevliokova et al. (2009) does use a tiling approach to consider forest stand age and reduce the large number of cohorts used here. The HISTCOND run was designed specifically to explore the effects of forest demography by trying to emulate the effect of just redistributing annual land-use fractions. As seen in the results, it does substantially overestimate the carbon stocks and underestimate the NEP compared to the run that includes the full effects of forest demography.

(2)Before applying the future climatic conditions, the CONDENSED run was first equilibrated by repeatedly inputting projected climate during 2016-2045 (Line 210). Why were future climatic conditions employed here? To let the model equilibrate at the year 2015 (as is indicated in table 1), climatic data during the last few decades before 2015 would be simply employed here.

Agreed – I reran the CONDENSE run to equilibrate from 1986-2014. I agree with this point and so it warranted a model rerun of the CONDENSED experiment. The reviewer accurately points out that I should be using the prior 30 years to 2015, rather than the post 30 years, as the basis for equilibration. Therefore, I ran the dynamic equilibration from 1986-2015, and used that as the basis for the initial conditions of the CONDENSED experiment. This change is noted in the Experimental Design section (lines 249-250). The new results are now used in all the figures involving the CONDENSED run, and numerical values throughout the text changed, where necessary. This change did not significantly alter any of the results. Both old and new figures are present in the revised document, so the reviewer can confirm that the differences exist but are minor.

(3)The manuscript lacks descriptions of the nature of the LULC data and how the model implemented it.

I too would have preferred a more thorough explanation of our cohort approach, which I originally had in the manuscript, but was told I had to remove it because it was self-plagiarizing. So, instead I referred to a paper with the detailed description (see below, as in the manuscript). The first paragraph of the methods references the LUH2 dataset.

A cohort approach is developed to convert a dataset of land use transitions (Hurtt et al. (2011; 2020) to annual cohorts of land use and land cover change (Hayes et al., 2011; Lu et al., 2015), whose purpose is to retain the soil characteristics of the cohort from which disturbance occurred and maintain appropriate growth and stand age of newly developed cohorts (Fig. S2a). A complete description of this approach can be found in Felzer and Jiang (2018) (lines 127-131).

Minor issues

(4)Please unify model names. The present manuscript utilizes both "TEM-HYDRO2" and "TEM-Hydro" for the same model.

Ok, I removed the acronym TEM-HYDRO2.

(5)Words on some of the figures are too small. Please magnify them.

I have redone all the figures with Time New Roman 12 (and 14 for the titles). This was particularly a problem for the legends, which were in 10-point font. When I copy and paste them into the document, and then resize them, the text may have gotten smaller. For the final version, I will be providing high quality files that should not have this problem.

(6)Line 80

What is the "FIA" stands for?

Added "Forest Inventory Analysis" (lines 81-82).

(7) Line 201 "Future CO2 data are taken from Meinshausen et al.(2020)." Is this data correspond to the RCP8.5? The author needs to inform about it.(8)

Added "RCP8.5" but also fixed the reference to Meinshausen et al. (2011), so now reads: . Future RCP8.5 CO₂ data are taken from CMIP5 recommendations (Meinshausen et al., 2011) (lines 225-227).

(9)Line 212-214

Please clarify the difference between NCE and NEE in this work.

Both reviewers had same comment – added sentence: "NCE is the NEP plus carbon lost through land-use conversion or by decomposition of agricultural or timber harvest products" (lines 252-254).

(9) Line 342 "the amount available"

Does it mean "the amount of available inorganic nitrogen"?

Yes, added "inorganic" (line 431).

Review

(10) Line 385 "NE U.S." What it stands for?

(11) Lines 423-424

Values here correspond to NEP or NCE?

For NEP, so I clarified that, though the standard deviations are very close for NCE as well (line 509).