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Earth System Dynamics
Discussions

Interactive comment on "Diverging land-use projections cause large variability in their impacts on ecosystems and related indicators for ecosystem services" *by* Anita D. Bayer et al.

Anonymous Referee #2

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General comments

Bayer et al. presented a comparison of multiple land use and land cover (LULC) scenarios broadly used for understanding the future impacts of global change on carbon emission and ecosystem functioning. They highlight the large discrepancies amongst scenarios and what are the implications of this broad variety of scenarios on the trends of several ecosystem service variables. LULC is unquestionably an important driver of future changes and understanding and assessing the uncertainty and variability of such scenarios is critical for both science and policy makers. Therefore, the topic of this study is relevant and timely, and this work can be an important contribution, but

it still requires some clarifications in the methodological approach and the interpretation of results, and potentially one additional analysis that should be straightforward to implement and could make the manuscript more appealing.

My main concern with the current presentation is that the authors highlight the large discrepancies amongst LULC scenarios and even mention that some scenarios predict unrealistic regional patterns of LULC change. However, I understood that the authors used only one scenario (RCP2.6) from one model (IPSL-CM5A-LR), whereas many of the LCLU scenarios were aligned to a broad range of future scenarios, and potentially had very different initial states that may explain at least some of the discrepancies. Couldn't these differences explain some or most of the discrepancies amongst scenarios? Also, the discrepancy across LULC is not necessarily a bad thing, as many of the scenarios selected by the authors are for different socio-economic pathways and they should be different. The authors could clarify these points in a revised version.

Also, the authors presented and discussed the variability in the trajectory of ecosystem services (ES) across all scenarios. This is fine but I think an analysis comparing the emergent responses across all the scenarios analyzed could give much more insight on how LCLU change could affect the future of ecosystem functioning. For example, the authors could relate the changes in cropland/forest/pasture area in each continent with changes in runoff or evapotranspiration and use the slope of these relationships to understand the sensitivity of ecosystem services to LCLU changes.

Specific/Minor comments

Line 29. I don't think abrupt transitions necessarily indicate problems in this case. For example, a tropical forest may be minimally disturbed until new infrastructure (e.g., paved roads) is built. Likewise, policies change with governments and can result in significant reversals of trends, for example, the significant reduction in deforestation in Brazil in the 2000s and the current increase.

Line 35. I suggest to replace "edge" with boundaries or ecotones. Forest edge is

commonly use in landscape scale to define transitions from deforested and forest areas at landscape level (sensu Skole and Tucker 1993). Same comment for Line 75.

Line 80. Rephrase: large uncertainties in LCLU projections may affect the confidence in projected changes in ecosystem functioning, but not the ecosystem functioning per se.

Line 115. Some word is missing after “following”.

Section 2.2. Maybe I missed it, but what happens when the LCLU scenarios are inconsistent with the LPJ vegetation? For example, if the scenario indicates timber harvesting but LPJ does not predict any forest? In fact, it is unclear to be whether or not logging was considered in these simulations.

Line 195. What was the rationale for selecting RCP2.6 instead of other pathways? It seems to me that RCP2.6 is rather too optimistic.

Line 209. Related to my point in section 2.2. It is fine to group all the natural classes, but this still does not clarify what happens in the case of logging.

Line 231. I understand the rationale for minimizing the role of interannual variability, but it is unclear to me that 5 years is sufficient. Would the results change considerably if, for example, 10-year averages were used instead?

Section 3. The authors have a separate discussion section, but the text in the results section often read more like discussion (e.g., most of the paragraph starting in line 391). Also, I think a multi-panel figure that showed the average changes in cropland, pasture, and forest (both increase and decrease) would help to summarize the results.

Line 273. This sentence is confusing.

Line 288. Examples of some countries?

Line 372. “Central” instead of “Middle”?

Lines 391–422. Can changes in irrigation also contribute to changes in ET and runoff in LPJ-GUESS? Does LPJ-GUESS simulate irrigation?

Line 464. It was more than soy moratorium in the case of Brazil, law enforcement and policy changes were also important (Nepstad et al. 2014).

Line 638. I suggest “South American” Cerrado and Chaco instead to remove ambiguity, as the Chaco is not in Brazil.

Line 709. I agree with this paragraph and this is why I also suggested the analysis on the emergent responses. It seems that the authors already have the results ready for at least some initial analysis to qualify the changes in ES responses as functions of LULC changes.

Figures 1 and 4. Some of the colours are difficult to distinguish, at least for me (CLU and LUH2, for example in Fig. 1). For Figure 4, the authors could fix hues for different levels of relative change (rows), and fix brightness for the absolute values (columns), it would also make the figure more intuitive.

References

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Skole, D., and C. Tucker, 1993: Tropical deforestation and habitat fragmentation in the Amazon: Satellite data from 1978 to 1988. Science, 260 (5116), 1905–1910, doi:10.1126/science.260.5116.1905.

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