Reply to anonymous referee #2

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.

We thank the reviewer for these encouraging statements and in the revised version of the manuscript we will address the open points as described below.

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.

See our response to Reviewer 1, whose question points into a somewhat similar direction. We agree that the range in LULC scenarios isn't a bad thing at all - this wasn't the message we would like to get across (and we will make this point explicit in the revised version of the manuscript). Nevertheless, even given different initial states and different socio-economic futures, one would nevertheless assess simulated changes in land use from a "common sense" perspective, and critically reflect on some rates and/or directions of change that seem implausible. This would include, for instance, cropland expansion along the fringes of the Sahara or extremely large and rapid area conversions in some countries (e.g., Malawi, Tanzania, Zimbabwe, etc.). These seem quite extreme w.r.t. past trends. Likewise, a rapid reforestation of the Cerrado regions would be a complete reversal of past trends. While it is not completely impossible, of course, we argue that the speed and magnitude of changes in some scenarios would nonetheless seem implausible.

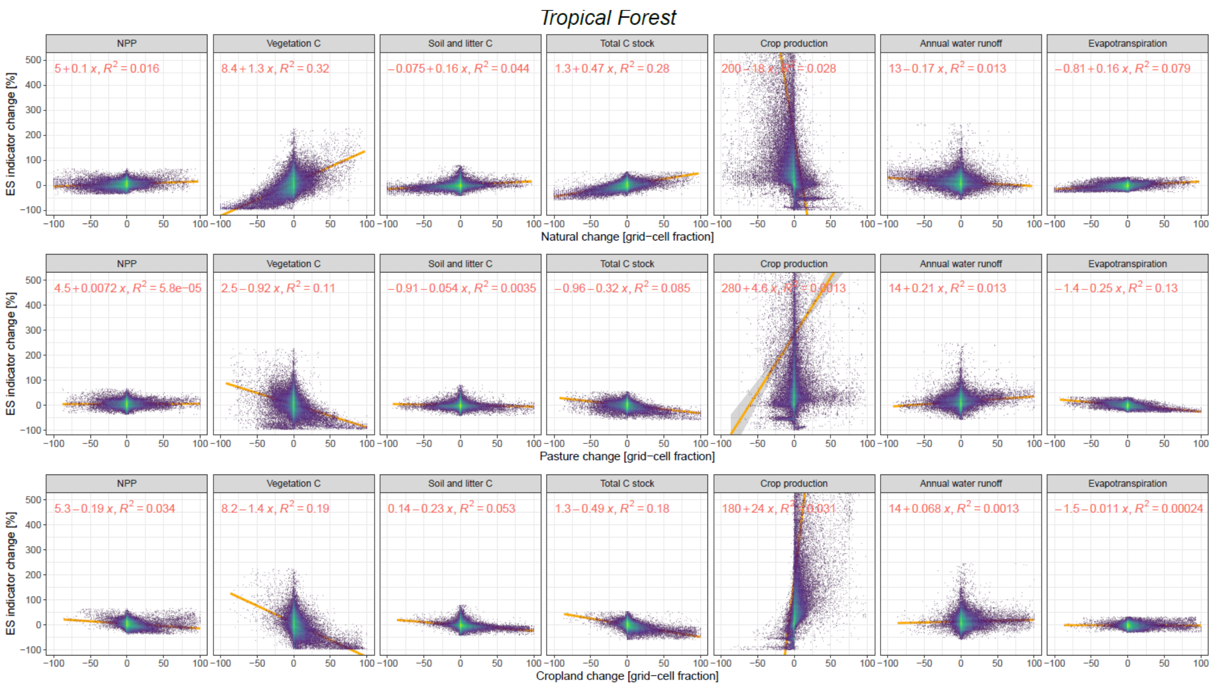
Regarding the selection of our climatic pathway and model: As stated in the methods section, IPSL-CM5A-LR projects future temperature and precipitation changes that lie 'in the middle' of an ensemble of a wider range of GCMs used in the ISI-MIP intercomparison project, which is the reason why we selected this GCM. The variability in some ES indicators when using climate forcing from multiple GCMs was explored in Ahlström et al. (2012) and Schaphoff et al. (2006), as highlighted in section 4.3. In addition, in section 4.2 we presented a summary of the results of a sensitivity test using climate inputs from five GCMs, instead of just IPSL-CM5A-LR, along with the four diverse scenarios from the LUH1 product in our simulation set-up.

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.

We thank the reviewer for this constructive suggestion. We had indeed tested a similar analysis already when writing the submitted version of the manuscript. In response to the reviewer request, we now performed a direct correlation of the changes ES with the changes in LULC classes, on the scale of biomes. The slopes of these relationships indicate some relatively pronounced responses such as for the increase in vegetation C upon the increase in the natural land fraction, and the increase in crop production upon the increase in cropland fraction – which in fact would be expected. Slopes for other ES indicator responses are mostly low, with little statistical power: R^2 values indicating the strength of the relationships are low to very low (majority below 0.1, best R^2 is 0.32). This reflects that direct correlations of ES indicator changes with changes in LULC are difficult to establish because they are significantly impaired by, e.g., overlying climate effects, different base levels in ES indicators and LULC configurations and different ecosystem responses below the level of biomes (including, e.g., legacy effects of past LULC changes). Nonetheless we believe it is worthwhile to include these findings in a revised version of the manuscript and address them in the following paragraph that will be added to section 4.2, along with supporting table and figure in the SI.

"A direct correlation of the per cell changes in ES indicators with the corresponding changes in cropland, pasture and natural land fraction could reveal the sensitivity of different ES indicators to changes in LULC. Across all scenarios and for the considered biomes, these relationships suggested for instance an about 1.5 % increase in Vegetation C per percent increase in natural land fraction and between 12 and 24 % increase in crop production per percent increase in the cropland fraction (see Table S8). Emergent responses of other ES indicators to changes in LULC fractions were mostly low (slope of regression lines close to 0) across the biomes (see Table S8). None of the identified relationships provided high reliability (highest R^2 was 0.32 for the change in vegetation C per change in natural land fraction in tropical forests, see Fig. S5). This reflects that direct correlations of ES indicator changes with changes in LULC are difficult to establish because they are significantly impaired by, e.g., overlying climate effects, different base levels in ES indicators and LULC configurations and different ecosystem responses below the level of biomes (including, e.g., legacy effects of past LULC changes)."

The following figure shows the direct correlation of the per cell changes in ES indicators with the corresponding changes in cropland, pasture and natural land fraction across all scenarios as an example for the tropical forest biome. It will be added to the SI of the new manuscript along with a table summarizing the slope values for all biomes.



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.

We believe this comment refers mainly to text in lines 464? The reviewer is correct of course that such abrupt transitions indeed do occur. However, past changes in a countries' land use policy are not included in any of the LUM model set up. Thus the modelled rapid transitions unfortunately do not represent a realistic policy process.

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.

Correct, boundaries is the better word.

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.

You are right, text will be adapted as suggested.

Line 115. Some word is missing after "following".

Thanks, sentence could be revised as "summarized in this section and …".

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.

These are important points - and indeed, wood harvest was not considered in the simulations because the underlying assumptions and technical implementations of harvest in scenarios vary a lot and could produce exactly the complications mentioned here. We will clarify this aspect in the methods section. Of each LULC scenario we only used the fraction of natural, pasture and cropland, as it is described in the methods section. Since harvest is not accounted for in the simulations, other inconsistencies are excluded: only the changes in the fractions of natural, pasture and cropland were considered as a change from the baseline LULC, i.e. they are consistent with the baseline (e.g. fraction of natural cell can only be reduced by the amount that is existent in e.g., year 2000 in the scenario period).

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.

We agree that given the still very high annual CO2 emissions, RCP2.6 seems a rather unrealistic pathway. Our chief reason to select RCP2.6 was to keep the impact of climate on projected changes and uncertainties in ES indicators relatively low, and to focus chiefly on the land-use change impacts. This will be included in the revised manuscript in section 2.3, for instance along the lines of "Climate projections followed the RCP 2.6 pathway. Large degrees of climate change and high atmospheric

CO_2 concentrations can have large impacts on ecosystem service indicators (see, e.g., Alexander et al., 2018). As our focus here is on the impact of land-use change, we chose a climate change projection, which over the simulation period would have relatively little additional impact".

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.

As stated above, the revised manuscript will specify that harvest and logging was not explicitly simulated. In addition, we added a table to the SI that explicitly shows the translation of LUMs LULC classes to the LPJ-GUESS LULC classes cropland, pasture and natural.

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?

In our experience this longer averaging period would not change the observed trends in ES indicators. We chose here a 5-year interval to "dissect" the relatively short simulation period into a meaningful number of sub-periods.

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.

This is a valid observation. In some sections of the results section we include reference to literature, which could be interpreted as a discussion. Specifically, when we refer to underlying processes that would explain a simulated change in ecosystems. We did this in order to focus the discussion specifically on the land-use change impacts and uncertainties. We acknowledge that this blurs to some degree the clean boundaries between results and discussion, but in our view concentrates the discussion on the main aspects of the paper - while still not omitting the links of our findings to the existing literature.

Figure: we discussed such a figure averaging across all scenarios for the original submission. However, since we are examining here scenarios that have very diverging underlying socio-economic assumption, we think it would be erroneous to produce an average across all these, which has the danger to leave the reader with the impression that such a figure would represent some form of an ensemble mean. Instead, Fig. 2 provides a summary of the dominant LULC changes for each scenario separately.

Line 273. This sentence is confusing.

Thanks, well spotted: We will revise the sentence along the lines of "In IMAGE, food production meeting underlying societal demand has large priority. "

Line 288. Examples of some countries?

This refers to the countries listed at the beginning of the paragraph and the areas marked in red in fig. 2 showing quite radical increases in the cropland fraction. We will adapt the sentence to clarify which countries are meant. "Here, some countries seem to provide substantially cheaper commodity *prices than others, explaining the radical changes seen in the regions as listed above (compare also Fig. 2)."*

290 changes seen in some countries or regions.

see previous comment

Line 372. "Central" instead of "Middle"?

Thanks, will be corrected.

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

As a stand-alone model as used in this study, LPJ-GUESS simulates irrigation in a simplified way, using the prescribed fractions of rainfed vs. irrigated management for each crop type, which are identical across all simulations (see methods section). Changes in ET and runoff are therefore consequences solely from the LULC change. Of course, e.g. an increase in cropland in a cell with high irrigated fraction and therefore increase crop growth will have higher effects on ET and runoff.

Only when LPJ-GUESS is bi-directionally coupled to a socio-economic land-use change model (such as in e.g., Alexander et al., Global Change Biology 2018, doi: 10.1111/gcb.14110), LPJ-GUESS adapts irrigation patterns to demands. We will clarify in the revised manuscript.

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).

We will clarify this in the revised manuscript.

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

Well spotted, thanks - this will be corrected.

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.

We appreciate your positive feedback. The suggested analysis was included as stated above.

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.

Thanks, we will ensure that the colors in the Figures are easier to read. We revised Fig. 4 and increased the brightness for the less important categories in order to make the figure more intuitive and also changed the color for CLU in Fig. 1.

References

Nepstad, D., D. McGrath, C. Stickler, A. Alencar, A. Azevedo, B. Swette, T. Bezerra, M. DiGiano, J. Shimada, R. Seroa da Motta, E. Armijo, L. Castello, P. Brando, M. C. Hansen, M. McGrath-Horn, O. Carvalho, and L. Hess, 2014: Slowing Amazon deforestation through public policy and interventions in beef and soy supply chains. Science, 344 (6188), 1118–1123, doi:10.1126/science.1248525.

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.