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# *Interactive comment on* "The impact of land-use change on the sensitivity of terrestrial productivity to precipitation variability: a modelling approach" *by* L. Batlle-Bayer et al.

**Anonymous Referee #2**

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—- General comments: —-

This study investigates whether and how the sensitivity of NPP towards precipitation anomalies is altered by the effect of land use change (LUC). To do so, the authors use a state-of-the-art DGVM and isolate the LUC effects by with- and without-LUC simulations of NPP. While the approach is in principal valid, and model and simulation setup are appropriate (apart from my concerns of using a single model, see below), the scientific question is not overly new, nor does the study add sufficiently novel aspects concerning methodology or detail that would lead me to say it is filling a crucial gap in literature. In particular in recent years, more and more observational evidence (e.g. Teuling et al 2010, Luyssaert et al 2014) as well as modeling studies (e.g. Brovkin et al

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2013, Christidis et al 2013) have emerged that clearly show LUC has the potential to alter biosphere-atmosphere fluxes significantly (including dependencies of vegetation activity to hydrological conditions). Considering this evidence, the authors' hypothesis "that LUC can modify this [NPP-drought] sensitivity" (abstract l., 5f.) is close to trivial. This is not to say that land use–climate feedbacks are not an important topic. I therefore outline below several points that, if accounted for in a substantially revised, if not new, manuscript, could add sufficient novelty to the present study to make it a very useful read to the land use/climate community.

—- Specific comments: —-

First paragraphs of the introduction: The review of earlier studies investigating correlations of climate anomalies and vegetation activity seems all very right and intuitive, but I am missing a discussion of in how far correlations between hydrological conditions and productivity are just a concurrent response to a third driver (such as global warming) and in how far previous literature – and the present study – prove causality. Similarly, in the results and discussion sections, I lack a discussion of the processes through which LUC influences the NPP-drought sensitivity – much of this is known from literature (see e.g. references cited under general comments and many of the studies cited by the authors), but the discussion of the results sadly stops at a description of what is obvious from the plots and a comparison against previous studies' results.

p. 588, l. 7: I agree that the study's setup is advantageous over many observational studies in that a model allows to isolate individual factors – but there are plenty of observational studies (such as paired sites) that have come at least close to this goal as well (e.g. Teuling et al, 2010, Lee et al, 2011) and should be better acknowledged.

p. 589, l. 12: Given the multitude of widely-used DGVMs, it reads presumptuous to label LPJmL as "prevailing".

p. 589, l. 23: "their" has no reference (since PFTs are spelt out only in parentheses).

p. 589, l. 26: * Spell out CFTs. * Tab. 1 has little to do with CFTs.

p. 589, l. 28: If managed and natural grasslands are models in the same way, then why does the GP transition later show such substantial responses (e.g. in India)?

p. 590, l. 9: * I would not call the LUC data underlying the two simulations "scenarios". For me, scenarios imply (a) a projection into the future (2) a transient time series. I think "land cover states" is the appropriate term. * I am missing information on how Monfreda's data (agricultural areas only) is used to define the extent of natural vegetation (I assume the ag data is simply overlaid over natural vegetation simulated by LPJmL, but then this leaves many choices on how to implement the data e.g. with respect to which natural vegetation type in a grid cell to use to fulfill ag area demand. And DeNoblet et al, 2012, show such choices make a big difference!) * The land cover states are discussed in section 2.4 already.

p. 591, l. 1 f.: Is preindustrial CO2 280 ppm? I couldn't find the description of the actual simulations beyond the spinup – I assume the CRU dataset's 1901-2009 time series is used as is for both land use state runs?

p. 591, l. 5f.: Given that LUC has started some 10 000 years ago labeling the 1901 state as "no-LUC" is very misleading. LUC by 1901 has been of similar size as the LUC that followed since. Why not be specific and label the simulations as "LUC of 1901" and "LUC of 2000"?

p. 591, l. 23: "land-cover type": at some point it should be explained that this refers to CFTs and PFTs (if this is correct).

p. 591, l. 23: You are looking at differences between two states in terms of fractional cover. This may hide that in fact the vegetation cover of grid cells may have been altered entirely, even if the diagnosed differences amount to only 20 or so percent (e.g. all forest area may have been transformed to cropland, but elsewhere in the gridcell pasture may have been transformed back to forest). Thus effects on drought-NPP

sensitivity may be different and potentially larger in reality than is modeled.

p. 592, l 22 f.: Is this your own definition or is there a reference?

p. 593, l. 12: Replace "avoid" by "reduce" – a linear fit is just an approximation to last century's climate evolution.

p. 593, l. 23: "significant correlation" – how do you calculate significance?

p. 594, l. 5: Can you show a map that shows the reader in which region which SPI has the best correlation?

p. 594, l. 9: "growing season": there are good datasets available that indicate true growing season (e.g. http://www.sage.wisc.edu/download/sacks/crop_calendar.html). Unless you base your choice of NPP month on such observational data I would rather be precise and call your variable "month of maximum NPP" than growing season.

Comments on the statistical approach:

The approach is not clearly described. I believe I understand the following: For each grid point, SPI is calculated covering 1- to 12-month timescales. Are calendar years bounds for the timescales or are they applied on the 109-year monthly timeseries? Then, for each grid point, for each year, for each month, SPI (all 1- to 12-month values) and NPP anomalies are calculated and correlated. E.g. NPP_July_2000 is correlated with the 12 SPIs covering the periods Aug_1999-July_2000, Sep_1999-July_2000, . . ., June_2000-July_2000, July_2000 (though obvious on second thought it wouldn't hurt to mention explicitly that only precip of preceding and concurrent months are correlated with NPP). It is also imaginable that the NPP anomaly of a month could be correlated not just with SPIs of different lengths up to that month, but that the NPP anomaly is correlated with SPIs (of fixed or various length) of any preceding month or multi-month time series; for example it can be expected for certain crops and regions that the month of maximum NPP correlates more strongly with precip during the planting season than with precip directly leading up to the month of maximum NPP. Besides the issue of

clarity, the latter comments also highlight that there are a lot of decisions done by the authors concerning the statistics that could also be done in some other way. Still, the implications of the specific decision are left unclear. For example, how do we know that a shift in month for SPI2 because of a new growing season month due to LUC doesn't simply lead to very different statistics that alter the correlation? p. 594: Now I am confused. How can the SPI2, referring to two months, be "annual"? What leads you to infer that SPI2 would be a good proxy for annual NPP anomalies, if the choice of SPI2 had been based on a single-month NPP? Again, large parts of the methods are insufficiently explained and/or hint to rather arbitrary choices, the influence of which on the resulting sensitivity is not explored.

Comments on the entire results and discussion sections:

One of my main concerns (admitted as being relevant in the very last sentences of the ms) is that simulated vegetation activity and simulated effects of LUC on biosphere fluxes have been found as to be enormously dependent on the type of DGVM used (see the LUCID and LUCID-CMIP5 studies, to which the authors partly contributed). Both inherent model differences as well as differences in the way LUC data is implemented have been identified as reasons for this model spread. The question then is what we learn from this single-model analysis. I suggest to follow at least two of the following paths: (1) Relate the findings to the model intercomparison projects. Evaluate NPP anomalies in LPJmL and the general mechanisms of drought-dependency against other model's descriptions, and include a thorough discussion of this. This request refers to a full step earlier than the comparison of end results against other studies on the regional level that the authors perform (and which is of course needed as well). (2) Repeat the analysis on MIP data. The authors mention TRENDY and LUCID, but LUCID-CMIP5 or possibly ISIMIP also provide useful data. Most other models will not cover the distinction between many different crop types, but apart from irrigated vs rainfed the wealth of LPJmL's CFTs isn't focus of the present study anyway; all other land use changes could well be assessed in MIP data. (3) Be more thorough on the

statistical analysis: Once my concerns on robustness of results towards the many statistical choices are addressed, the method should be evaluated in terms of sensitivity towards model choice (e.g., it is well possible that another index than SPI2, referring to another timescale, may be better suited for other models). The aim of this would then be to develop a method that can at a later stage be applied to all kinds of models, for which the current method has not been proven to be suited for. This would shift the focus of the paper from (the model-dependent) results to a useful method paper with exemplary application. Other choices have been made in this ms such as choice for a specific climate driver dataset (but see comment above on choice of a specific time period) or specific selection of LUC regions (based on an arbitrary threshold of 20% transitions). From the literature, however, it seems to me that potential biases introduced by the choice of a specific DGVM, with partly contradicting responses of different models, is the most crucial choice of all to be tested and justified.

Another aspect that the study could think about in order to gain novelty would be some good conclusions on what we conclude from the diagnosed change in sensitivity in terms of vulnerability and e.g. food security. Or further, how the diagnosed changes may feed back on climate.

p. 598, l. 2: "correlate with", not "to"

p. 598, l. 6 ff: Why does GP not lead to changes in Inner Asia, while it did in India? This hints to the ms's problem that the analysis is descriptive more than exlorative.

p. 598, l. 19 ff: This should go into the introduction.

Fig. 1: * See before comment on "land use scenarios". * What is white color?

Fig. 2: * The y-axis labels are oddly spaced and suggest non-linearity, while they are linear in fact. * What does the range around NPP and precip depict – is that sigma? * Why are there no negative values for the correlation panels? * "for three grid cells" should be "for one grid cell each"

Fig. 5 * is not readable. * Explain what pale vs dark colors mean in the third column.

Fig. 6: * What is the range, is it the spread over grid cells? * What are the boxes? Please keep in mind that a figure should be fully understandable just by itself including the caption!

The ms is generally well-written, but it would benefit from a native speaker's check (during copy-editing at the latest) for subtle language issues (e.g., it should be "model-determined" on p. 59, l. 16, or "3-month SPI" in l. 20)

Typos etc

p. 590, l. 1: "composed of", not "by". p. 591, l. 4: "applied to", not "for" p. 590, l. 20 (analogously p. 596, l. 21): "being water stress a key factor" should be "with water stress being a key factor". p. 592, l. 19: "of an SPI", not "on" p. 593, l. 4: "by the" duplicated

References

Brovkin et al, J. Clim, 2013: Effect of Anthropogenic Land-Use and Land-Cover Changes on Climate and Land Carbon Storage in CMIP5 Projections for the Twenty-First Century

Christidis et al, GRL, 2013: The role of land use change in the recent warming of daily extreme temperatures

DeNoblet et al, J. Clim, 2012: Determining Robust Impacts of Land-Use-Induced Land Cover Changes on Surface Climate over North America and Eurasia: Results from the First Set of LUCID Experiments

Lee et al, Nature, 2011: Observed increase in local cooling effect of deforestation at higher latitudes

Luyssaert et al, NatureCC, 2014: Land management and land-cover change have impacts of similar magnitude on surface temperature

Teuling et al, NatureGeo, 2010: Contrasting response of European forest and grassland energy exchange to heatwaves

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