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Interactive comment on “Climate-driven shifts in continental net primary production implicated as a driver of a recent abrupt increase in the land carbon sink” by W. Buermann et al.

Anonymous Referee #1

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Buermann et al. assess abrupt increases in NPP, their relations with shifts in global NBP, and the drivers of the shifts in NPP. The manuscript is very interesting and well written. Assessing potential abrupt changes in NPP is novel. While reading this manuscript, I was wondering if the reported shifts in NPP (especially in boreal Eurasia) might relate to the observed increase in the seasonal amplitude of atmospheric CO₂ (Graven et al., 2013). This increase in CO₂ amplitude originates from northern ecosystems and had the largest increase within the last years (Graven et al., 2013). I also was wondering how the results relate to previous findings of the same author (Buermann et al., 2014) which emphasize the role of drought on decreasing NDVI in boreal regions.

1 Major comments

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1.1 Uncertainty from FAPAR datasets

A thoughtful assessment of uncertainties from different temperature, precipitation and radiation datasets is done in this study. However, the major contribution to the temporal dynamic of CASA-modelled NPP originates from the FAPAR dataset. Several studies have shown large difference between different FAPAR (or NDVI) datasets (Fensholt and Proud, 2012; McCallum et al., 2010; Scheftic et al., 2014; Tian et al., 2015). Although the used GIMMS3g FAPAR dataset is the most reliable long-term dataset, the difference in more recent periods in comparison with datasets from modern sensors, highlights the need to account for FAPAR-related uncertainties. In order to convince the reader about the reliability of the reported NPP changes, it is necessary to #1 evaluated the reported changes to sensor changes in the underlying GIMMS NDVI3g record (similar as in (Tian et al., 2015)), and #2 to assess the uncertainty in NPP estimates also based on alternative FAPAR datasets at least for the overlapping period with newer sensors (e.g. MODIS).

1.2 Model evaluation

The results are praised by saying in the abstract “using (...) models constrained by observations”. However the only constrain is the use of GIMMS3g FAPAR within CASA. No further constraints are used for modelled NPP. Model results are not at all evaluated against independent data. In order to be more convincing, it is necessary to evaluate model results against independent data, e.g. NPP databases (Luyssaert et al., 2007), GPP site-level time series (FLUXNET), upscaled fields (Jung et al., 2011), C stock maps (Carvalhois et al., 2014), or long-term changes in the seasonality of atmospheric CO₂ that might be indicative of changes in northern terrestrial productivity (Graven et al., 2013).

1.3 Change point detection algorithm

The statistical analysis is very valuable. Especially, I very much appreciate that the authors evaluate several alternative statistical models by means of SIC and the uncer-

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tainty analysis for the change points is also a necessary step given the low robustness of such change detection methods (Forkel et al., 2013). However, many studies report changes in trends on NDVI datasets, such as greening to browning (de Jong et al., 2011, 2013; Piao et al., 2011). A good overview of potential changes is given in de Jong et al. (2013). Trend changes as further option in change detection was not assessed in this study. Trend changes might be here therefore either represented as changes in mean or as continuous long-term trend. I'm wondering if ignoring the trend change-option results in an overestimation of abrupt changes and thus affects the main conclusions of the study. In my opinion it is necessary to additionally account for the trend change option in the statistical analysis (Verbesselt et al., 2010a). The author's fear of overfitting time series with additional parameters as in trend change models (p. 13774, l. 1-3) can be easily handled by again using SIC on the trend change option. Further it was not clear to me how the seasonality of NPP time series was treated in the change point algorithm.

2. Specific comments

These are comments to specific parts of the manuscript. However some of these comments will be resolved by addressing the major comments.

p. 13770, l. 6: I don't understand why forest regrowth and fire suppression were used as examples for land-use patterns. Forest regrowth is a dynamic in land cover, the corresponding change in land use could be rather named reforestation or afforestation as the term land use usually implies human management.

p. 13771, l. 16-18: But this study applies only to arid grasslands. Are you aware of any references that try to quantify the CO₂ fertilization effect in FAPAR data for forest ecosystems?

p. 13771: How is the CO₂ fertilization effect on photosynthesis considered in CASA? Only through the FAPAR forcing dataset or is there an additional module that accounts for CO₂ fertilization? FAPAR might be not sensitive enough to the CO₂ fertilization

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effect especially in forest ecosystems that have upper FAPAR values. Based on modelling experiments it has been shown that the CO₂ fertilization effect contributes only minor to changes in FAPAR (Forkel et al., 2015). Therefore it might be possible to underestimate the CO₂ fertilization effect if the NPP model relies just on FAPAR.

p. 13771, l. 20: Is this really land surface (i.e. skin) temperature? I thought CRU provides air temperature at 2 m?

p. 13771, l. 25: I agree but at least it would be possible to assess FAPAR-dataset uncertainty for the overlapping period with MODIS or you could based on the FAPAR-NDVI relation you could try to use other long-term NDVI datasets (Marshall et al., 2015). An assessment of the findings in relation to potential uncertainty sources from different FAPAR datasets seems necessary given the striking differences in these datasets regarding trends and inter-annual variability (Fensholt and Proud, 2012; Tian et al., 2015).

p. 13773, l. 10-12: Is there a reason why the option “change in trend” (i.e. stable to positive, positive to stable, positive to negative etc.) was not considered? Several studies have shown that such trend changes exist in satellite-derived NDVI data (de Jong et al., 2011, 2013; Verbesselt et al., 2010a). Such changes were also detected in the GIMMS3g NDVI and thus are likely also present in the GIMMS3g FAPAR data. I assume by ignoring the “trend change” option, there is the risk of over-selecting option 2 (change in mean) as the preferred statistical model.

p. 13773, l. 26-28: ... and this might result in an over-estimation of abrupt shifts. I think it could be worth-while to check alternative change detection algorithms that also account for smooth changes by considering trends (e. g. (Verbesselt et al., 2010a, 2010b)). I think the risk of overfitting is low by adding two more slope parameters to the statistical model as you could use the SIC as well. Given the large use of trend change detection methods on NDVI time series it seems not plausible to my why this should not be done for NPP data. Furthermore, based on the large uncertainty of trend change

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detection methods (Forkel et al., 2013) it is necessary to consider several methods.

p. 13774, l. 6-26: The approach of the uncertainty assessment is very valuable. However in order to fully understand it but not to overload the average reader, I would suggest to extent the description of this approach (maybe incl. some illustrative figures or equations) and move it to the supplement.

p. 13775, l. 14-15: Do the numbers represent the magnitudes of the shifts? Please clarify.

Results section: I suggest to have some sub-chapters (e.g. 1. NPP shifts, 2. drivers)

p. 13776, l. 8-24: I think you cannot separate the driving factors on NPP with CASA. As you are admitting in the caption of Fig. 2a, the FAPAR dataset already integrates changes in temperature and precipitation and other drivers. Thus, the FAPAR dataset explains most of the dynamic in NPP. Even if you try to separate these factors, we still don't know about the temperature or precipitation effects. In my opinion, this separation of drivers cannot be insightful done with CASA but only with the TRENDY results.

p. 13776, l. 10-15: Recent studies suggest that changes in spring FAPAR and the begin of the growing season in boreal ecosystems are related to changes in water availability from changing snow cover (Barichivich et al., 2014) and to water supply from changes in permafrost dynamics (Forkel et al., 2015). I'm wondering if and how these processes are represented in CASA and if you are seeing similar relations on spring NPP.

p. 13777, l. 3: Trends in LAI from TRENDY models have been also evaluated against GIMM3g LAI showing diverging regional patterns of greening and browning trends, especially also in boreal Eurasia (Murray-Tortarolo et al., 2013). Therefore, I would expect similar diverging results for NPP changes from these models. I think it's worthwhile to provide results for individual TRENDY models, and assess their outputs against the observed FAPAR and your NPP estimate in order to draw conclusions only from those

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models with realistic changes.

p. 13777, l. 13-16: How does this sentence relate to recent findings about the importance of semi-arid ecosystems for the inter-annual variability of the net land uptake (Ahlström et al., 2015)?

p. 13778, l. 23-29: I was already wondering before how fire was treated in your models. Did the CASA version use the “GFED mode” to simulate fire dynamics? How is post-fire succession modelled? Given the large importance of fire activity on ecosystem dynamics in the two focus regions, it should be worth to assess the potential role of fire on NPP changes. The discussed relation between spring warming/greening and summer fire emissions targets in my opinion to the wrong effect. Although the fire season peaks usually in summer in boreal regions, the years with large fires often show different seasonalities. Moreover, it seems important that the increasing fire activity in boreal regions (Kasischke and Turetsky, 2006) resulted in a larger growth of deciduous trees (Beck et al., 2011) which might result in increasing NPP.

p. 13780, l. 1-6: Are these changes in NAO and AO detected and are also significant based on the change detection method?

Fig. 2 and Fig. 3: I suggest combining the CASA and TRENDY results in one figure for a better comparability of results.

Fig. S1: I’m wondering what is causing the abrupt decreases in NPP over moist tropical Africa and SE-Asia. Can you provide any explanation?

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