***Interactive comment on* "Abrupt seasonal transitions in land carbon uptake in 2015" *by* Chao Yue et al.**

Anonymous Referee #1

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The article "Abrupt seasonal transitions in land carbon uptake in 2015" by C. Yue and coauthors presents a detailed analysis of anomalies in carbon sinks and sources, climate and vegetation greenness during recent decades with an emphasis on the year 2015. Understanding the carbon cycle and its interaction with climate change is a highly relevant research topic, and the authors refer to state-of-the-art literature and datasets. The authors combine a number of observational datasets and model results, and my impression is that their methods and results are sound. The description of the work steps is clear and the data sources are well documented. In this regard, the article is good at what it does.

My major concern however is that it remains unclear what the authors are trying to achieve with this article. I would guess that the results might tell us something about how climate affects vegetation and the carbon cycle. What do the results imply about the relevant processes, about past climates and potential future developments, or about our potential to model these processes? The authors address such questions only briefly in the last paragraph of Sect. 4 and in the very short Sect. 5, stating that they go beyond the scope of the article.

[Response] We thank the general positive comments by the reviewer. We were originally aiming for two purposes in this article: (a) to diagnose the anomaly of large scale CO_2 fluxes for 2015 given the specific nature of that year, as a case study (high CO_2 growth rate, anomalously strong vegetation greenness and the historically highest annual temperature), using atmospheric inversion data, and (b) to diagnose whether abrupt transitions have occurred in terrestrial carbon uptake in 2015, and briefly infer the reasons for such transitions.

We agree with reviewer that the exploration of the general links among vegetation greenness, land carbon uptake dynamics and climate variations is necessary in order to put the 2015 case into a more general picture, to infer general patterns of land carbon dynamics that could be useful for future prediction of land carbon dynamics. We also add this point as one of the research aims of our paper. According changes are made in revised abstract, and the 3rd paragraph of the revised Introduction section.

We have extensively revised the manuscript to incorporate correlations of land carbon uptake anomalies with vegetation greenness anomalies and climate anomalies related with ENSO dynamics. Two new figures (Fig. 3, Fig. 4) are added in the main text, and three new figures (Fig. S4, S5, S7) are added in the Supplementary Material. Results and discussion sections are substantially expanded to include more discussions on the mechanisms underlying land carbon dynamics, and the relevance of this study.

I also wonder why the authors focus so much on the year 2015. What is so special about this year (apart from being relatively recent) that would justify this focus, and what can we learn from this case study that is valid in a greater context? If there is something I am overlooking, I suggest that the authors reframe their article to bring out their message more explicitly, and that they stress what the progress is compared to previous articles. I believe that this would improve the impact of their article. For example, the authors could systematically relate anomalies in climate, carbon fluxes and NDVI using the whole record, and not only focus on 2015. They should also consider to include the year 2016 (if possible) to capture the full recent El Nino event. It appears a bit arbitrary that they pick the year 2015 and one other previous El Nino event for their analysis, using the rest of their data only to calculate linear trends. A more comprehensive statistical analysis of the available data might allow more general conclusions without the need of running

climate models.

So far, the main selling points of the paper seem to be

(i) the (arguably) counterintuitive combination of high NDVI and negative carbon uptake anomaly (ii) large anomaly of the year 2015.

Regarding (i), I find it little surprising that greening and carbon loss (or a reduced carbon sink) can go together since both anomalies can be dominated by different locations and different seasons, and because they are not linearly related given the complex ecological processes involved. The authors point this out themselves, hence invoking a "paradox" seems a little exaggerated to my taste. (But I would be curious if this anti- correlation is a temporal feature or a robust trend that can be expected to continue in the future; something the authors might choose to give more attention to.) Regarding (ii), I find it misleading to speak of an "abrupt transition" (title, abstract and line 263+). This term gives the impression of a singular event with long-lasting consequences, like a forced non-reversible switch to another state or regime. However, the phenomenon discussed in the paper appears to be an anomaly that is the realisation of natural variability, hence an extreme but temporal event. This comes on top of a gradual trend to larger growth rates, so the year 2015 will most likely not be unique. In fact, the atmospheric growth rate of CO2 in 2016 was even higher than in 2015. I therefore wonder whether the term "abrupt transition" is useful here, and would suggest a more suitable term, e.g. the land carbon uptake anomaly in 2015. I therefore also strongly suggest a change in the paper's title.

[Response] (1) We now use the full 1981–2015 data and performed statistical analysis of vegetation greenness, land carbon uptake and climate anomalies for different regions and seasons. These results are incorporated in the revised manuscript in both result and discussion sections, with findings from previous studies being extensively referred to and discussed as well. (2) We maintain the "paradox" expression because we think it is adequate to describe the year 2015, which comes with extreme greenness and an only moderate land carbon sink. Higher greenness is sometimes simply assumed to be associated with higher sink, but this is not necessarily true, as is also pointed out by the reviewer. We now examine in more detail in the revised the relationship between land carbon uptake and vegetation greenness for different seasons and regions. (3) As explained in the response to the previous comment, now the manuscript is restructured around two research aims: to examine general relationships among vegetation greenness, land carbon uptake and climate variations, and to examine the 2015 as a special case on how land carbon dynamics have responded to a combination of extreme greenness and ENSO climate variations. This is made clear in the revised manuscript. (4) We mostly drop the word 'abrupt' given its potential confusion in an ecological context and, instead, the word "strong" is used. (5) We change the title to reflect the revision in the manuscript content to "Vegetation greenness and land carbon flux anomaly associated with climate variations with a special focus on 2015". (6) We did not include the year 2016 into the current analysis because the inversion data are not available yet. But we believe focusing on the year 2015 could already generate meaningful conclusions from our manuscript.

Minor comments

- line 85-86: "We used ... includes"

[Response] we changed 'includes' to 'including'.

- line 89: What is a validity period, and in what sense are the other years are not valid?

[Response] Site observations used in the Jena CarboScope inversion are coherent over time within the so-called "validity period", but are not outside the validity period. More specifically, the validity period is

defined by the one using a consistent number of sites, i.e., all sites that have observations over such a period. Outside the validity period, site numbers changed depending on their availability or operation time. It is optimal to examine the temporal trend within the validity period, but this does not mean the data outside this period are invalid and should not be used. In fact, the same situation also happens in CAMS inversion, which considers a variable number of sites during the full study period. Because our analysis has to reconcile the need of a large site number in 2015 and long historical period for a robust anomaly estimate, using the s04_v3.8 run outside its validity period is therefore a compromise. Besides the responses here, we made the according changes in Section 2.1.1.

- line 108: Why do the authors pick MAI to characterise the ENSO state?

[Response] We believe the reviewer means MEI rather than MAI. MEI (Multivariate ENSO Index) is the first unrotated principal component of six variables over the tropical Pacific that are closely linked with ENSO. Among the six variables sea-level pressure, sea surface temperature and surface air temperature are included. MEI has been widely used in literature as an indicator for the ENSO state, for instance, Nemani et al., 2003; Wang et al., 2013; van der Werf et al., 2008. The MEI should, therefore, summarize not only the ocean component of ENSO (El-Niño), but also the atmospheric component (the Southern Oscillation).

As a complement to MEI, we also used the Oceanic Niño Index (ONI, http://www.cpc.ncep.noaa.gov/products/analysis_monitoring/ensostuff/ONI_change.shtml) when comparing the evolution of El Niño events of 1997 and 2015 in Supplementary Figure S10. The ONI tracks the running 3-month average sea surface temperatures in the east-central tropical Pacific between 120°-170°W (the Niño 3.4 region). Supplementary Figure S10 shows very similar temporal patterns of MEI and ONI during El Niño evolution especially when El Niño reached its peak, indicating the suitability of MEI being used in ENSO-related analysis.

The pieces of information described above are also included in the revised manuscript in appropriate sections.

- line 141-148. At the first reading I did not understand the role of the "historical trend" for the growth rate in a given year. I understand now that specific anomalies in 2015 are later related to climate anomalies, with anomalies being defined as residuals after removing a linear trend. The reasoning behind this could be explained more explicitly here.

[Response] We added the following text in this paragraph to make it more explicit and hope it will help clarify better: "The record-breaking AGR in 2015 thus must be put into an historical perspective to reconcile evidence for extreme greening and the highest atmospheric CO2 growth rate. For example, if 2015 comes up with a large increase in carbon emissions accompanied by droughts (browning) in the northern hemisphere and the tropics, then the highest AGR might not be regarded as a big surprise. Therefore, to understand the contributing factors for the highest AGR in 2015, it must be separated into a long-term trend and interannual anomalies."

- line 201: both instead of bother

[Response] This has been corrected.

- line 136: It would help non-experts to briefly explain how the sources and sinks are quantified in the GCP. How independent is this dataset from the inversion calculations?

[Response] Estimates of land and ocean carbon uptakes are largely independent from the two inversions

used in this study. We have inserted the following text in this paragraph to clarify this: “Estimates of ocean carbon uptake in GCP are based on observation-based mean CO2 sink estimate for the 1990s and variability in the ocean CO2 sink for 1959–2015 from global ocean biogeochemistry models. Estimates of land carbon uptake in GCP are calculated as the difference between anthropogenic emissions, atmospheric CO2 growth and ocean sink. The estimates of land and ocean carbon uptake in GCP are largely independent from the two inversions used here, except that the CO2 records from atmospheric stations which are used in inversions are also used in GCP to derive global AGR.”

- Sect. 2.2.1: It would help me to already see time series and a map as a visualisation of the rank analysis. I understand the structure of the paper and find it reasonable, but it could make sense to merge the data analysis section 2.2 with Results Sect. 3. Otherwise, one has to read the methods section without visualisation, and later remember each methodological detail when the results are shown. This is a matter of taste and I leave it to the authors to reconsider the structure.

[Response] Relevant figures (Fig. 1, Supplementary Fig. S1, Supplementary Fig. S3) are now cited in this section in the revised manuscript to help readers understand better the methods. But we maintained the methods and result as two separate sections mainly for the clarity of the structure.

- line 226: “the seemingly paradox” is grammatically wrong.

[Response] “seemingly” is changed to “seeming”.

- line 350: data suggests (not suggest)

[Response] Corrected.

- Supplementary Material: I suggest to put captions underneath (not above) the figures and increase the space between the figures. There is too much space between the caption of Fig. 2 and Fig. 2. These things make it difficult to identify the right caption for each figure.

[Response] Figure captions are now put below figures.