

## **General comments**

The authors must be congratulated for improving the general state of the paper. It reads easier, some of the confusing results have been removed and the new analyses are interesting.

However, this reviewer still feels the manuscript lacks organization and clear objectives. The current version still feels like a collection of results, without a clear storyline and defined objectives.

The authors justification that future projections in LAI can be constraint based on current model performance is incomplete (See Section 5 comments).

## **Abstract**

Generally the abstract has improved and it is easier to follow the main results of the paper. However, this reviewer feels it fails to summarize the main points of the paper clearly.

## **Introduction**

Generally the introduction has improved and it reads better. However, this reviewer feels it lacks a clear definition of the objectives and the logic behind them.

Page 4, line 15. This should not be included here, but in the methodology.

Page 4 lines 14 – page 4 line 7. This reviewer feels the introduction fails to clearly state the objectives of the paper and to justify them.

## **Methods**

Generally this section is still poorly written. The section could greatly benefit from better sub-headers for each section (datasets, data analysis, etc.). In the current version the authors mixed the explanation of the datasets they used with the analysis.

Page 5 lines 12-20 Poorly written.

Page 6 lines 16-20 should be in the introduction

Page 7 line 20 I am failing to see why precipitation was not included.

Page 11 line 5. This should be shown at least as supplementary. If the analysis of future LAI is so dependent on precipitation I would expect to see similar patterns in the present-time.

Page 11 line 15: close to perfect is incorrect, should sate “equal to the observed data”.

## **Results**

Generally the section is clearer than before and has improved, however it still feels like a collection of different papers, lacking coherence across sections. Some of the results are not well summarized and the reader has to jump from one figure to another to understand them. Additionally the authors mix results with discussion, making it difficult for the reader to fully follow the story. There are several wrong generalization regarding the link between LAI, climate and food security that subtract value form the paper.

Section 3.1 I fail to understand why the authors choose 3 time periods across the century and 1 RCP8.5; instead of focusing in the changes by the end of the 21<sup>st</sup> century across all RCPs (or at least the two used here). It is hardly surprising to see the increasing change in LAI across time for the same RCP. I also found hardly surprising to find a correlation across RCPs, in spite of the different land use forcing.

Section 3.2. I do not agree with the usage of LAI as a proxy for food security, although this approach has been used to analyse present-day changes I believe it is not solid to use it for future projections. It is easier to justify this section saying LAI is a general indicator of vegetation health.

Spatial links between P and LAI could have been shown better, using simple linear regressions. Figure 6 is remarkably confusing and overly complicated.

Section 3.3. The analysis on C stocks appears out of the blue and is simply wrong. The analysis on the CO<sub>2</sub> fertilization is too simplistic and the interpretation is likely to be wrong. The authors are not separating the effects of CO<sub>2</sub>, climate and temperature on LAI; which leads to the false assumption that CO<sub>2</sub> is not the main driver of LAI. On the opposite, CO<sub>2</sub> is probably the main reason why LAI increased, but also lead to an increase in T and changes in P, which in turn drive LAI trends to decrease in the tropics (dryness) and increase on the NH (temperature). The correct way to perform this analysis is using simulation where CO<sub>2</sub> changes but not the radiative forcing and compare them with the full simulations.

Section 4.0 the authors still lack a proper discussion on weather current model performance is link to better future simulation. No discussion of the N-effect on limiting NPP (and therefore LAI) is included; also no discussion on how dynamic vs. prescribed vegetation will affect this.

Arguing that others have used a similar methodology is not correct; the cited papers (e.g. Cox et al.) are based on very robust scientific evidence of links between the vegetation NBP and climate (e.g. in Cox et al. is the C residence time in the soils over the tropics). The relationship between LAI and climate is not as straight forward (e.g. the growing season cannot be extended forever, the increase in T also leads to earlier snow-melt which can lead to drought latter on the growing season), hence one cannot assume that current model performance will be the same in the future.

Additionally, ranking model performance against 1 observation may be lead to errors due to the obs-dataset bias. See Forkel et al. 2014 biogeosciences and Forkel et al. 2015 Global Change Biology.

Section 5.0 again the author interpretation to some of the results is not correct (e.g. the LAI trends in RCP4.5 are smaller than in RCP8.5 due to CO<sub>2</sub> not climate forcing).

I am missing an analysis linking P to low-LAI over the historical period.

**Tables**

Table 4 seems unnecessary.

**Figures**

Figure quality and control has improved and the new figures are clearer and better. However they are hardly self-explanatory, lacking information such as titles, legend names, etc.