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Earth System Dynamics
Discussions

Interactive comment on "Impacts of land-use change and elevated CO_2 on the interannual variations and seasonal cycles of gross primary productivity in China" *by* Binghao Jia et al.

Anonymous Referee #2

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The paper has discussed total GPP and its regional distribution in China from 1981 to 2010 using results from 12 terrestrial biosphere models. Effect of LULCC and atmospheric CO2 levels on GPP in China has also been studied by analysing results from different experiments that were well-described in the text. Overall, the paper is comprehensive in terms of understanding the effect of LULCC and CO2 on GPP for China for recent years. Validation of the results, the use of ensemble mean for the purpose of this study and representation of the figures is appropriate. Congratulations to the authors for coming up with a detailed study. The manuscript is well-written overall. However, I have some issues as described in detail below:

Major comments:

1. Identification of gaps in literature has not been done adequately in the Introduction section. Page 3, line 16 "However, few studies have adequately explored the impacts of climate change, atmospheric CO2 concentration, and LULCC to interannual and seasonal variations of GPP in China". If there are already studies that have studied these impacts, they should be cited here and effects of LULCC and CO2 on GPP as estimated in this study should be compared with these studies in the later sections.

2. Page 4, line 19, explanation of the term MTE is not very clear. This should be made clear before MTE is used to represent the dataset in the rest of the paper from this point on.

3. Figure 1 (on page 23) shows 16 different kinds of vegetation types in the legend but only the major ones are visible in the plot. To make the plot readable, similar vegetation types like MIXSB, MIXSC, MIXSG, SHRUB should be merged since they are anyway not much distinguishable in the plot.

4. Page 5, Section: 3.1. Since this section starts with the discussion of results presented in Fig. S1 and has an entire paragraph on this figure, the figure should be moved to the main text.

5. There is a lot of mismatch between the region references in terms of region names and regions numbers in the Results section. For instance: a. Page 7, line 13, "central China and northern China" should rather be "northern China (R4) and northwestern China (R5)", as per the numbers represented in figure 4. b. Page 7, line 32, "in summer over southeastern China (Fig. 5j)". 5j corresponds to R9 and as per fig. 1, R9 is southwestern China, not southeastern China. To avoid this confusion in region names and region numbers, I would strongly recommend the authors to double check the text in the sections of Results and Discussions, and to use region numbers along with region names in these sections to that the text explanation can be verified easily with the figures.

6. Page 11, line 12 and Page 1, line 34: A strong concluding statement has been made about how climate is the dominant control factor of annual trends, IAV and seasonality of China's GPP, without much analysis of results in this context in the Results section. Some analysis of trends coming from SG1 case should be included in the results section before making this statement, specifically since the paper has focussed mostly on LULCC and CO2 effects, and there are not many remarks on impact of climate in the paper.

7. The implications of this study and application of the results are not adequately emphasised. The authors are suggested to add some information on how this work his valuable, specifically considering how understanding of the effects of LULCC and CO2 on GPP can help in comprehensive scenario of things and decision making.

Other issues to be considered:

1. There is no mention of the study period in the abstract so it is not clear for which years are the results mentioned in this section applicable for.

2. The phrase "independent upscaling GPP estimate" in the abstract does not give any idea of the dataset being talked about and hence should be either modified or eliminated from this section.

3. The usage of a few words and sentence formation in the text is questionable in some places, for instance: a. Page 2, lines 25 and 27: "60% of the uptake by terrestrial ecosystem was due to raising(?) atmospheric CO2" and "It suggests that the impact of raising(?) CO2 on land carbon sink may be a negative feedback to future climate". b. Page 4, line 14: "The simulated monthly GPP from these 12 models was conducted(?) for the period of 1981–2010." The authors are suggested to re-check these typos and small errors.

4. Table S1 (mentioned on Page 4, line 10) only has all "O" under columns SG1, SG2 and SG3 for all models, check attached file. I am not sure what purpose the table is

serving apart from citing references for each model description. This table can either be improved or deleted.

5. Page 5, line 23, Fig. 1a.(?). This seems to be a typo and Fig. 2a. should be mentioned here.

6. Figure 7 has comparison of LUH1 data with CLUD for major vegetation types. Clearly, there is a mismatch in the recent trends of both datasets, more specifically from year 2000 to 2010. This difference is intriguing but since the figure does not represent 100% land cover of China, there is missing information here. For instance, the sum of major vegetation types shown in fig. 7a represents $\sim$ 60% of area for CLUD for 2010 and $\sim$ 80% of land cover for LUH1 for 2010. I would suggest this figure to be modified to account for 100% area of China so that the entire land cover distribution and the transitions can be accounted for.

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