

## ***Interactive comment on “Three decades of simulated global terrestrial carbon fluxes from a data assimilation system confronted to different periods of observations” by Karel Castro-Morales et al.***

**Anonymous Referee #1**

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This paper develops an analysis of the global land C fluxes for three decades. The aim of the paper is to analyse the performance of a data assimilation system, over a historical period. The paper misses clear science questions that would broaden the interest of the study. I lack a clear understanding of the novelty of this work. The conclusions focus on the result that a single decade of data leads to similar results to 3 decades of assimilation, but I am not clear how robust this result is, and how significant it is.

I suggest the authors focus more clearly on the novelty of their experimental results. It

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would be helpful to focus on science questions that are more broadly relevant to the C cycle science community. It might be helpful to add experiments assimilating just FAPAR or just CO<sub>2</sub>, and test the outputs against the other (withheld) observational dataset. This experiment might indicate whether the model effectively couples canopy processes with atmospheric concentrations.

The abstract is too long, it needs to be reduced to 300 words and focused on the key outcome.

Writing style is clunky, missing words – for instance the opening sentence is poorly comprehensible: “The observed contemporary in atmospheric CO<sub>2</sub> is driven by anthropogenic emissions from fossil fuels and land-use change”. I suggest the authors work together to improve the English, reduce sentence lengths, and shorten the more bloated paragraphs.

Methods: The global grid is very coarse (8x10°) – what are the implications? Why mix such a large spatial grid with such fine temporal scales? Is this valid or required? Why not have a grid that matches TM3? Explain what is meant by ‘each iteration’ (L. 139). It is unclear how FAPAR data are generated from equation 2, which seems to generate NDVI estimates. We require details on the FAPAR observation operator – I could not find any. Why are there no FAPAR data after 2006? (l. 278) What are the implications of not including fire emissions? Why were fire products such as GFED not used to provide this input? I would like to know more about the process to determine which parameters were selected for optimisation? These seems to mostly phenological variables which will link to FAPAR. I would expect to see other parameters related to C turnover, e.g. mortality rates, decomposition rates.

Results: What are the substantial changes in tropical LAI (l. 306) – how do these match in situ measurements? What is the increase in R<sup>2</sup> (l. 314) – please report in the text. The key result seems to be shifts in the timing of CO<sub>2</sub> exchanges – it would be interesting to focus more on the shifts in model process representation (parameters)

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required to allow these changes. I do not find Figure A2 very helpful in this regard. The large drop in GPP in the posteriors is significant – what process is this traced to in the parameter adjustment? I did not find fig 4 helpful in regard to identifying improvements in IAV modelling – it would help to have some statistics to support the statements here (l. 461).

#### Discussion

“the mismatch between observations and model output is small, and thus of little concern”. This statement needs to be more rigorous – how is ‘small’ determined? What is the threshold for concern? The lack of tropical IAV suggests too weak an ENSO response – were any relevant parameters included in the CCDAS that would have allowed identification of drought response? Some of the discussion seems circular – that using FAPAR data in the assimilation improves modelling of FAPAR seasonality. It would be good to clarify the text to the explicit calibration and validation results to strengthen this section. It seems this result has already been identified in an earlier CCDAS publication, so it is not clear what is novel here.

The authors identify problems “results from the structural dependence of the MPI-CCDAS on few, globally applicable PFT-level parameters, and challenges in using the spatial mixed signal at the model resolution to infer PFT-specific parameters.” It would help to develop these ideas some more – do we expect these issues to specifically affect the current analysis and in what ways?

Table 3. The posteriors suggest a Ra:GPP ratio of ~65% - it would be useful to discuss this value which seems high for a global estimate.

The very large reduction in soil C stocks from the prior needs further discussion – JS-BACH was spun up to steady state for the prior, so I am not clear how the experiments generated 50% drops in this value. How far is the model from steady state with such a reduction in soil C?

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We hear again that a result here repeats an earlier CCDAs result (l. 660), which reduces the novelty of the analysis.

Other comments:

Figures – there are too many figures, some of which are of low value, and this distracts from the key message of the paper. Some of the figures in the appendix are referred to several times, so why are they not in the main text (replacing those referenced less).

L. 59 . Citation needed for this 5.6% value

L. 297. The zone between 20-60° is not well described as “sub-tropical”

Table 1: Add row numbers

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