We appreciate the comments from two anonymous reviews. We answer below in blue text to each comment listed in black adding the explicit action taken in a revised manuscript.

**Reviewer #2**

This is an interesting and useful paper, albeit of more technical than scientific interest. There are a number of factors that reduce the scientific impact of the paper, while focusing more on the interaction of observations with a model of this type when used in assimilation mode.

The reanalysis is limited by a number of factors, the very low spatial resolution dictated (I suppose) by the resolution of the atmospheric inverse model, the limited data fields assimilated (just carbon fluxes and FAPAR) and the lack of potentially important processes, such as fire.

Reviewer #2 rightly points out the limitations to our study. However, we think that it still offers relevant insights despite its limitations, which have also been pointed out by reviewer #2. We will revise the manuscript to discuss the limitations of the current study more in detail, while at the same time, focusing on the main story of the manuscript: the use and effect of long-term data sets for assimilation and the question on how long the improved model/data agreement can last. We hope that this study will inspire the future use of CCDAS systems to integrate further data streams (such as SIF or VOD), for which a CCDAS is uniquely suited given it's ability to use data at different resolutions and for different time-periods. We will discuss this more clearly in the revised manuscript.

As in our response to reviewer #1, the spatial resolution is indeed dictated by the computational setup. Increasing resolution would of course allow for a better integration of remote sensing data as well as the current sub-grid scale variability in climate. However, as has been pointed out before (Müller and Lucht, 2007; Peylin et al., 2016), have suggested that increased resolution would not necessarily have a strong effect on the overall performance of the model against global carbon cycle observations.

As in any model study, MPI-CCDAS does not include all processes. As noted in our response to reviewer #1, using data sets to account for this flux is not possible given the lack of data before 1997. While there is a fire module in MPI-CCDAS (Lasslop et al., 2014), it has been identified a number of issues with that module that would need to be addressed, and the effect of these issues on the spatio-temporal dynamics of the land carbon balance would need to be clarified before it is possible to include it into these long-term and computationally intensive MPI-CCDAS runs. We agree that the addition of disturbance processes due to fires is an interesting aspect for future MPI-CCDAS developments and may contribute to an improved representation of the interannual variability. However, we note that some of the major fluxes (deforestation, peatland fires) are not considered by this, and many other, fire models. In the revised manuscript, we will include a paragraph containing the potential implications to our results of not having explicitly included fire emissions in our experiments.

The author's assessment of model skill is ambivalent, they point to low errors in some places, while noting that the El Nino cycle is not well-captured, a time scale that others have argued provides a critical clue to climate sensitivity (eg Cox et al).
In the revised manuscript we will clarify this issue, which is probably at the core of many model-data inter-comparison studies: the fact that cost functions measure the absolute misfit of a model versus data, and are not necessarily sensitive to important aggregate system properties such as the response of the tropical carbon cycle to ENSO. Our assimilation study clearly exposes this problem, as the assimilation stops without such known variability considered and this is because in the cost-function the benefit of matching this interannual variability is not strongly weighted. In the revised manuscript, we will discuss the implications and potential ways around such problems.

By contrast, the advanced methods used and the useful assessment of the impact of the duration of the assimilation experiment, as well as other technical innovations provides a useful update to their prior paper, as the scientific conclusions are overlapping. As assimilation becomes more prevalent, and as data records lengthen (for this study, of a 30-year time scale these really are the most relevant global fields) with SIF, radar-constrained biomass, and water variables such as vegetation optical depth becoming available for > 10 years, this paper provides encouraging news about the utility and impact of records of decadal length.
We appreciate the comments and support from the reviewer to this manuscript and for valuing the scientific contribution of our work.

I'd suggest rewriting the paper modestly to emphasize the lessons learned about the impact of assimilation, and the time horizons, and placing less emphasis on the carbon cycle results, especially as the authors note (and correctly) the conclusions broadly overlap their earlier paper. I note that papets of the paper are awkwardly written and could use a careful edit, and there are a lot of figures I found them helpful in reviewing the paper but several of the figures could clearly be moved to supplemental material.
We believe that it is important to demonstrate that the carbon cycle results of a 30 years and a 5 years experiment (as in Schürmann et al. 2016) are broadly comparable to set the stage for the impact of the different time horizons. However, we agree with both reviewers that in the previous version this obscured the key innovation of the study and we will therefore revise the manuscript to make this clearer. More concretely, we will place much of the evaluation material and associated text to the supplementary material, and instead give more space to the results and discussions of the time horizons. Specifically: Fig. 2, on the experimental design and mentioned only once in the main text, it will be moved to appendix. Figures 3 and 4 will be moved to the supplement: Fig. 3, showing the spatial distribution of LAI before and after the assimilation, but the manuscript does not focus on the specific changes on LAI after the assimilation, instead $R^2$ values are given in Table 2. Fig. 4 shows the interannual variability of FAPAR for the different sub-regions is only mentioned briefly in the results and will be moved to the supplement. As from figures from the appendix Figures A3 and A4 were mentioned more frequently hence they would be moved to the main text.
With the aim of delivering a clearer message in our manuscript, the revised version will undergo a thorough English revision before re-submission.

**References listed in this response.**

Lasslop, G., Thonicke, K., and Kloster, S.: SPITFIRE within the MPI Earth system model: model development and evaluation, Journal of Advances in Modeling Earth Systems, 6, 740-755, 2014, 10.1002/2013MS000284.

Müller, C. and Lucht, W.: Robustness of terrestrial carbon and water cycle simulations against variations in spatial resolution, Journal of Geophysical Research, 112, D06105, 2007, 10.1029/2006JD007875.

Peylin, P., Bacour, C., MacBean, N., Leonard, S., Rayner, P., Kuppel, S., Koffi, E., Kane, A., Maignan, F., Chevallier, F., Ciais, P., and Prunet, P.: A new stepwise carbon cycle data assimilation system using multiple data streams to constrain the simulated land surface carbon cycle, Geoscientific Model Development, 9, 3321-3346, 2016, 10.5194/gmd-9-3321-2016.