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Interactive comment on "Lagged effects dominate the inter-annual variability of the 2010–2015 tropical carbon balance" *by* A. Anthony Bloom et al.

Anonymous Referee #2

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Bloom et al rely on a data assimilation approach to constrain parameters of a carbon cycle model (DALEC2a) based on which the analyses separates the contribution of meteorological (CON) versus lagged (LAG) effects on the inter-annual variability (IAV) of net ecosystem carbon fluxes (net biosphere exchange, NBE). The study is focused on tropical regions, where lagged effects on IAV of NBE are comparable to instantaneous effects (CON) but dominate the inter-annual change of NBE. The topic is timely and relevant for the carbon cycle and climate research communities and the methods and datasets represent the state-of-the-art in the field.

This is a study with a clear split between a methodologically elegant approach using

model-data assimilation based results to interpret dynamics of the land surface, and a dataset that seems too short to effectively evaluate the approach which may undermine the robustness of the results. Hence, there should be a bit more stringent evaluation of the model output focused on the IAV component. Of course is hard to do this with only 6 years of NBE, but there is a wealth of data in the study that could be explored e.g. the IAV per month across all months; or the IAV in LAI (which spans ∼20 years); also the IAV of SIF; or global observation based products of evapotranspiration (which also span at least a couple decades). The evaluation that is done seems to aggregate across large regions, though the temporal domain per gridcell is not reported: how does the model compare to observations at gridcell level too? In Figures 4 and 5, was there a gridcell comparison within region to ensure that the apparent good behavior is not only a result of the aggregation (e.g. by plotting the distribution of the gridcell differences between model and observations)?

As reading the model-data fusion component there are details missing, many of which are omitted "for the sake of brevity". But there should be a bit more details on the cost function determination, structural uncertainties and dimension of the data streams. How is determined the model structural error (mentioned in line 246)? It is an important component of the assimilation exercise itself but is only mentioned that was determined via a "trial and error" approach (L281). Are the values reported in L284-291? But, does it mean sigma_i in equation 4 is always constant? One other aspect to report is how many observations per data stream in the likelihood function are used? I would suggest the use of the appendix or supplements section to be a bit more descriptive about these approaches and results obtained.

Also for understanding, why not assimilating all the years that have observations? Predicting a point in the future is mostly about evaluating the models' temporal predictive capacity, and not really the model's ability to diagnose the LAG/CON causes of NBE IAV based on available observations. From the results presented (Figure 5) this could lead to a better IAV description? This is probably an experiment computationally too large to compare, but perhaps there is another motivation behind leaving years out.

As I was going through the LAG versus CON concept and the interpretation of the results, I was wondering: if there is a trend in the carbon pools, whether realistic or not, then the \DeltaNBE will be mostly attributed to LAG effects. But the trend is a model output with few constraints and one is left wondering if this has a strong effect in the attribution scheme. This could be demonstrated contrasting the trends in stocks (soil and vegetation stocks) against \DeltaNBEˆ{LAG}. But, would it be possible to assess if the trends in the stocks themselves are robust (or too high or too low)?

On the contribution of \DeltaNPP to \DeltaNBE: it seems that it should relate to the contribution of \DeltaGPP to \DeltaNBE as well once NPP is a fixed fraction of GPP. Was there a reason not to evaluate the IAV of GPP or NPP against independent observation-based datasets?

Other points:

* Section 2.4.: is fair to say then that the maximum lagged effect determined by this approach is of one year.

* Table 1: the numbering of the footnotes is wrong.

* L299: missing verb “we a subset” → “we use a subset” ?

* Eq.12 define \deltaM_a

* L396: “in principle” is repeated and unnecessary

* L399: “NBEˆ{DIR}_{a}” should be “NBEˆ{CON}_{a}”?

* L511: is very interesting that plant-available H2O is seen to contribute to \DeltaNBEˆ{LAG}. Would this mean that controls on NPP via soil moisture would dominate instantaneous controls of precipitation or radiation on photosynthesis in tropical ecosystems?

* L536-538: Is unclear how Figure 7 supports this observation...

* Eq. B4: very interesting for this version of DALEC to include v_e. Given its importance on the simulation of water controls on GPP, and the role of plant-available water, is there a spatial gradient from wet to dry tropics?

* L275-276: interesting approach, very similar to Desai 2010 (https://dx.doi.org/10.1029/2010JG001423).

Ultimately, I would like to acknowledge that this manuscript shows a great endeavor in model-data fusion approaches wall-to-wall. Is a follow up on previous developments in the CARDAMOM framework that is clearly of value for the scientific community. But the confidence and interpretation of results is dependent on the evaluation efforts, which would lend robustness to the analysis by focusing more on IAV. I hope that the Authors find these comments useful and I look forward for the feedback.

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