Reviewer #1 comments

Major comments

1. Introduction: I have some trouble understanding what kind of data the authors want to use for data assimilation in the future. After rereading the motivation several times it seems to me that they aim at incorporating remote sensing or earth observation data for variables that have not or barely been processed (e.g. FAPAR) but not for variables that are derived by a model (e.g. GPP) because the second are not well constrained (L30f). If this is the case this is at least to me not easy to comprehend. If this is not the case and the authors plan on also using heavily processed data such as GPP, how will using such badly constrained data improve the process-based model? Can you please clarify?

We'd like to sincerely thank Referee #2 for his or her thorough re-reading of our manuscript, for which we are very grateful. It is very important to us that the core aim of not only the manuscript, but also our work is understood. In order to address the issue raised by the referee, we have ammended ammended the manuscript as follows:

1) The last sentence of the 3rd paragraph of Section 1 (Introduction) now reads:

"Another issue is that ***these only refer to CO2 sinks, while*** *we lack spatially distributed data sets of terrestrial biosphere \coz sources.*

2) The following paragraph has been split in two and the beginning has been changed to:

"However, in order to identify the drivers of terrestrial carbon sources and sinks, such as vegetation state, soil carbon content of different qualities, temperature, soil moisture, atmospheric humidity, or light availability, we need models that are ***both internally consistent -- i.e. can be run without remotely sensed input –- and at the time*** *thoroughly evaluated against reliable observations.* ***Those observations should be as independent of specific model assumptions as possible, so that it is possible to clearly distinguish between model predictions by themselves (when we run without using those observations), and predictions resulting from the combination of observations and model assumptions.***

Furthermore, *if we also want to …"*

3) The (formerly) 5th paragraph (starting L41) has also been split before "Data assimilation is a valuable tool" (L47), and a clarifying sentence has been added. This newly formed paragraph now reads:

"Data assimilation is a valuable method for automatically finding the optimal combination of model initial values, parameters and even input quantities given the observations assimilated, pertinent to certain assumptions about prior values and uncertainties of models and data within a Bayesian framework (Tarantola 2005). While not providing a ready made answer -- it always needs to be

assured that the thus optimised model simulations "make sense" -- data assimilation can be used to find the most reliable model and data based estimates of quantities of interest, e.g. carbon fluxes, and serve as a tool for evaluating assumptions about the inherent processes driving changes in those fluxes. ***What we want to avoid, however, is to assimilate data streams that themselves rely substantially on model assumptions, such as the global GPP products mentioned above, because this would make the results depend on model assumptions outside of the core model used for assimilation.*** *Thus, we expect significant added value…"*

4) As a minor modification, we have added a further example for this method where the data assimilated is locally measured, not remotely sensed (after Knorr and Kattge 2005, L61):

"… eddy-flux measurements (Knorr and Kattge, 2005; ***Kato et al., 2013****)"*

Kato, T., Knorr, W., Scholze, M., Veenendaal, E., Kaminski, T., Kattge, J., and Gobron, N.: Simultaneous assimilation of satellite and eddy covariance data for improving terrestrial water and carbon simulations at a semi-arid woodland site in Botswana, Biogeosciences, 10, 789–802, https://doi.org/10.5194/bg-10-789-2013, 2013.

2. L496-499: Why do the authors think SIF is a useful metric for model evaluation if at the same they state that the observations a high uncertainty and they therefore have to increase their scaling parameter S_SIF – which mainly seems to be a tuning parameter (see L602f) - by an entire order of magnitude? Considering the large uncertainty I would imagine that using a Bayesian approach, the posterior parameter distribution of a model after data assimilation is not well defined. Are there alternatives to SIF observations that have a lower uncertainty?

The uncertainty in the SIF measurements themselves is not particularly high. But there is a high an uncertainty concerning the absolute magnitude of the SIF observations, reflected by a high ucncertainty of S_SIF of the SIF observation operator (Equ. 1). This is due to large uncertainties pertaining in particular the spectral conversion factor (Equ. 2). Both can be addressed through calibration of the scaling factor in Equ. 1, which will be performed within the assimilation procedure. For such a case, we expect the SIF measurements to provide an important constraint on the core model by virtue of the information contained in the temporal fluctuations of SIF and the relative rather than absolute changes in the measured data. For clarification, we have ammended the text at L500ff. (3rd paragraph of Section 5.2) as follows:

"The difference in magnitude between the modelled and observed SIF is likely due to the choice of prior parameters for the SIF model, taken from Gu et al. (2019), and the specic spectral conversion used (Equ. 2). Although it has not been done here, there is scope within D&B to adjust these parameters in the assimilation. ***This is achieved through calibration of the scaling factor s_SIF in the observation operator for SIF (Equ. 1). Given that the model with its prior parameter set can already track*** *the seasonal and diurnal cycle of the observations, and this appears to work reasonably well,* ***SIF measurements can provide constraints on processes that affect the temporal evolution of***

photosynthetic rates, such as leaf phenology, or timing of stomatal closure.*"*

Minor comments:

1) Check correct usage of "than" and "then".

Thank you. Found two instances and corrected them.

2) L74: I suggest to add one or two example variables that directly link to remote sensing information to make the connection to the previous paragraph.

Added "(e.g. FAPAR, VOD)"

3) L218f: "[...] wood directly feeds SOM." What is the reason for this and why is this an acceptable simplification?

In DALEC we have two dead organic matter pools – one rapid turnover (litter) and one slow turnover (SOM). For simplicity we transfer dead wood to the SOM pool as wood is relatively slow decomposed. It is an assumption that is explicitly stated in various publications describing DALEC.

4) L227: What is meant with "the footprint of the observations?"
Added ", i.e. the source area of the signal measured by the respective instrument."

5) Eq. 11

Sorry, and thank you! This is correct. We've changed the manuscript accordingly.

6) L375:

Thank you! "form" changed to "from"

7) L409-412: The sentence in brackets is very long, hard to comprehend and potentially wrong. It states "[...] model-measurement correlation for NEE to be zero, but substantial [...]", but how can something be zero and substantial at the same time. I suggest to split this into at least two sentences.

The text in brackets has been modified and now reads:

"Let us assume that the true temporal average of NEE is zero, while each, the model and the measurements, reproduce temporal average of GPP equal to temporal average of TER, but with some temporal "noise" added. This noise might be due to measurement or model error, but it is uncorrelated between model and measurements. In this case, we would expect the model-measurement correlation for NEE to be zero. However, correlation between modelled and measured GPP or TER could still be substantial due to coinciding temporal variations shorter than the

averaging period."

8) Sec 5.1: This section only refers to the boreal site, which could be stated again at the beginning of the section.

Thank you for pointing this out. The section now starts:

"The simulations at the Sodankylä site …"

9) Sec 5.3.: This again does not include a statement regarding the reported site.

Done. Thank you.

Reviewer #2 comments

None

Other changes:

Added thanks to anonymous revierws to the end of the acknowlegements.

Notification to the authors:
With the next revision, please consider renaming the supplement materials according to our standards: https://www.geoscientific-model-development.net/submission.html#assets > Supplements

We checked this section and found that the following applied to the supplementary information included with the present manuscript:

3. Equations, figures and tables in supplements should be numbered as (S1), Fig. S5 or Table S6. Sections are numbered as S3, S3.1, and S3.1.1.

This has been implemented in both the manuscript and the SI.