Reviewer #1 comments

1. Introduction: The introduction does not provide an overview of currently available comparable models and whether such models already fulfill some of the requirements identified from BETHY studies (L54-59). I believe this is needed to fully evaluate the novelty of the D&B model.

We have added three paragraphs of discussion of three further possible candidate models that all have a history of data assimilation, and all can be run as part of general circulation models (weather forecasting, or earth system models), namely C-TESSEL, JULES and ORCHIDEE, as well as introducing DALEC in this context. The discussion follows the list of requirements, and precedes the introduction of the D&B model:

"There are a number of models that could potentially fulfill those requirements. They range from carbon models incorporated into routine weather forecasting, such as C-TESSEL (Boussetta et al 2013), to highly complex land surface and ecosystem models that can be operated both within earth system models or independently, such as JULES (Best et al 2011, Harper et al 2016) or ORCHIDEE (Traoren et al 2014). Of these, C-TESSEL has probably the strongest track-record for assimilation of satellite data, mainly for the purpose of constraining soil moisture (Scipal et al 2008). However, it does not simulate the mass balance of carbon, despite simulating photosynthesis and respiration, nor can it predict leaf area index, which it requires as input data. C-TESSEL is therefore of limited use when assimilating FAPAR, or variables related to biomass.

By contrast, JULES includes a full set of carbon fluxes and pools (Clark et al 2011). An adjoint version of JULES has been developed to optimise parameters at site level using eddy flux data (Raoult et al 2016). ORCHIDEE includes not only carbon but also nitrogen cycling (Vuichard et al 2019). A data-assimilation framework for ORCHIDEE also exists, which has been successfully employed at site level for the step-wise optimisation of model parameters using remote sensing data (e.g. FAPAR), as well as water and carbon flux observations from the eddy covariance flux networks (Peylin et al 2016).

We note that less complex models such as C-TESSEL are often much better suited for data assimilation than highly complex models, because a simpler structure with fewer parameters, omitting processes not relevant at the time scales of interest, makes optimisation both computationally and mathematically much more more feasible. For example, C-TESSEL and BETHY lack representation of carbon pools (except for leaf area in the case of BETHY) due to a focus on short time scales of up to a few years. This is contrasted by another model, DALEC (Data Assimilation Linked Ecosystem Carbon), which focuses on carbon pools and longer-term processes, but is structurally also simple. DALEC has been developed specifically for assimilating information on C fluxes and pools from satellite observations (Bloom et al 2015), eddy covariance systems (Bloom et al 2015, Famiglietti et al 2021), and biometric data including biomass (Smallman et al 2017, Quegan et al 2019). "

2. L22-24: While I agree with the authors' statement on the reliable characterization of carbon and water fluxes, I think that their justification "we currently lack a robust, spatially and temporally

explicit knowledge of sources and sinks of CO_2, or of the drivers of those variations" could be more concise. It is not clear if the authors refer to sources and sinks of CO_2 in general or which is what I would expect considering the scope of the article, only those related to vegetation dynamics.

We have amended the statement as follows:

"as we currently lack a robust, spatially and temporally explicit knowledge of the sources and sinks of CO_2 ***within the terrestrial biosphere****"*

Additionally, the statement should be underpinned by an overview of the current knowledge. Specifically, it would strengthen their statement to provide information on the following:
- The spatial and temporal resolution and coverage of state of the art models and data products.
- An overview of known and suspected drivers.

We have have attempted to clarify by adding the following discussion:

"The lack of knowledge exists despite of the availability of products of net or gross carbon uptake by terrestrial vegetation, such as those from MODIS with daily and up to 250~m resolution (Zhao eta 2005), or from the Copernicus Global Land Service with a 300~m spatial and 10-day temporal resolution (Swinnen et al 2021). One issue is that those products are no direct observations of carbon fluxes, but rather a combination of remotely sensed information and a set of assumptions. They thus do not necessarily agree with each other or with the results of ecosystem models (Turner et al 2006, Sun et al 2021). Another issue is that we lack spatially distributed data sets of terrestrial biosphere CO_2 sources.

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 thoroughly evaluated against reliable observations. If we also want to identify existing carbon sources and sinks and attribute those to certain drivers and processes, we also need to be able to run and evaluate those models at the spatial and temporal resolution of interest. Running models at high spatial and temporal resolution is not an issue in principle. The problem lies in finding suitable observations at high temporal and spatial resolution for terrestrial ecosystem model evaluation and in finding out which model formulations, initial conditions and paramerisations can reproduce those observations. "

3. L28-35: This seems to be a crucial section motivating the development of D&B. However, I do not understand how the need to include primary earth observation data in data assimilation frameworks provides added value for indirect or secondary earth observation data products (L30-35).

As stated already, the issue is that few remote sensing products refer directly to the quantity of interest, i.e. CO_2 sources or sinks. Using remote sensing products only indirectly related has then two subsequent purposes: First, it gives us the opportunity to evaluate models and to ensure the produce the observations as reliably as possible. Second, we can use those model simulations that best *reproduce the available observational data to derive what the reviewer calls "secondary earth observation data product".*

The first is explained at the end of the added discussion responding to the previous comment. To explain the second, we have added the following text to the paragraph:

"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 tool for evaluating assumptions about the inherent processes driving changes in those fluxes."

4. Section 2.3: I am missing how DALEC and therefore D&B constrain the ratios between the biomass of different plant organs (e.g. leaf to root ratio) and also if allometric relationships are considered somehow. Is this handled implicitly via the regional-scale calibration? This should be included in the model description and potentially the discussion of model limitations."

We explain in the text that "NPP is allocated to each of the four live biomass pools based on fixed site or PFT-specific fractions". Yes, the regional-scale calibration handles the estimation of both allocation ratios and organ lifespans that determines biomass dynamics. However we have not explained how in CARDAMOM we use 'ecological and dynamical constraints' to ensure that allometric relationships (like root:shoot ratio) are kept within ecological realistic bounds.

We have now added the following text to Section 2.3:

"Parameters for the C cycle in D&B use PFT calibrations for DALEC derived using the CARDAMOM model-data fusion approach (Bloom and Williams 2015). CARDAMOM uses ecological and dynamical constraints to ensure that allometric relationships arising from parameter selection (like emergent root:shoot ratios) are kept within ecologically realistic bounds. By calibrating DALEC using both LAI and woody biomass data, a constraint is placed on relevant model parameters to match the measured biomass of these plant organs."

5. Section 4.2: The section is missing the description of the initial conditions for vegetation biomass and soil carbon. If these do not need to be initialized how are initial values determined? Generally, an overview table in the main text or SI containing all variables for which initial conditions need to be prescribed and the respective initial values would in my opinion improve the clarity of the model setup description.

Additionally, the description jumps between sites which reduces reading flow. I would kindly ask the authors to describe the sites after each other.

The initial conditions of the biomass pools are shown in Tables 6 and 7 of the SI, Secton 1.4 "Model setup". We agree that we should have mentioned this in here in the main text.

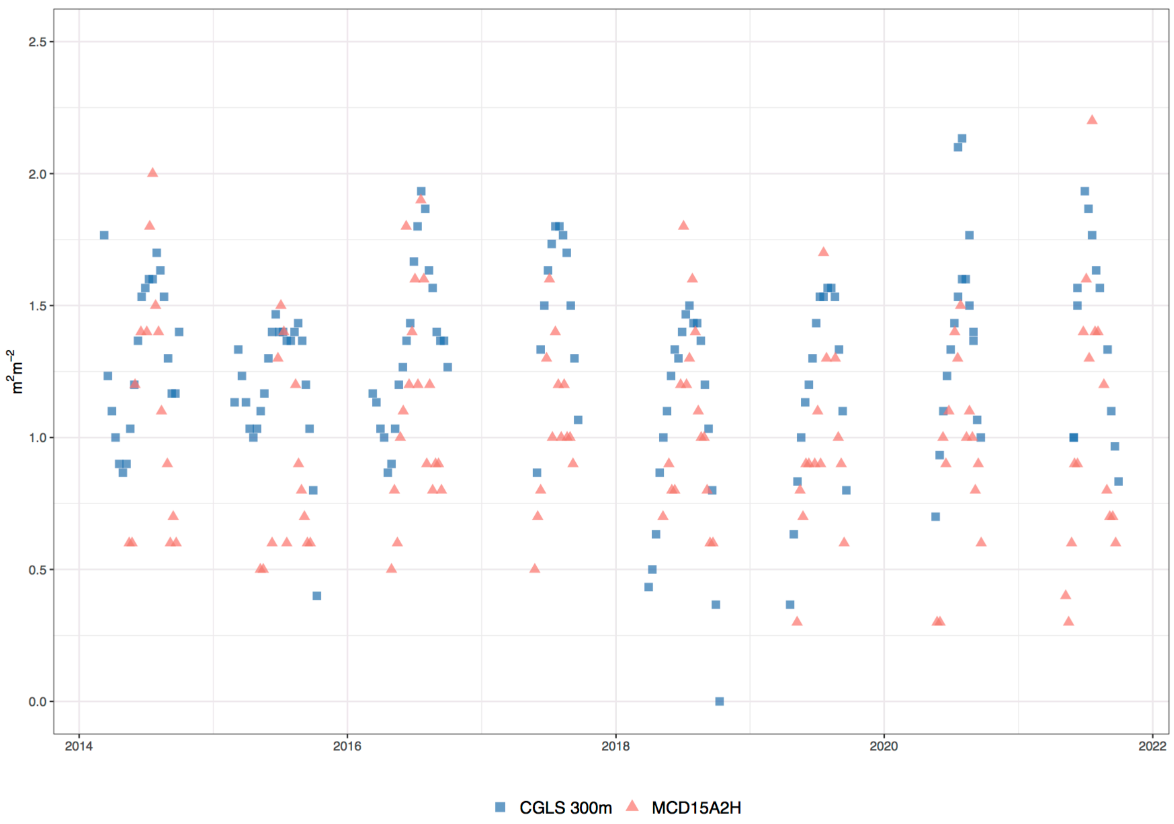
We have restructured Section 4.2 such that in the first paragraph, settings applicable to both sites are described, and then dedicate one paragraph each for each site's particular setup. We also now refer to the tables in the SI that contain the initial conditions.

6. Section 4.3: The description of the evaluation metrics is not detailed enough. For example it is not clear what is meant by multi-year averages of the annual cycle and the multi-year mean and how these differ. From following sections it becomes clear the second is the average of the annual sum but the explanation is missing. The authors should also provide equations for all evaluation metrics.

We have added equations for all metrics. We also now clarify the different time periods used for the metrics (2016-2021) and the total simulation period (2009-2021) for the Sodankylä site (start of last paragraph Section 4.3)

7. Section 5.1 and 6.3: The authors explain the intra-annual fluctuations of FAPAR by the LAI seasonality (L414f). However, I would expect that the two PFTs evergreen coniferous forest and evergreen shrub should not have a strong LAI seasonality which is confirmed by the observations (L423f). The authors briefly discuss this in the limitations section and relate it to phenology but do not provide a detailed explanation of model behavior (L523-528). I understand that the authors cannot provide a calibrated version of D&B at this point but would like to see how their results relate to eq. 147-149 to fully explain this behavior.

We agree that these two PFTs are not expected to have a strong LAI seasonality. The evergreen coniferous forest and evergreen shrub PFT calibrations for the DALEC component were constrained with seasonal cycles of series of leaf area index (Copernicus Service Information, 2020) from the study domains. The Copernicus product includes a strong seasonal cycle of LAI across northern latitudes, which is unexpected and at odds with in situ knowledge of the ecosystems. Therefore the calibrations of foliage dynamics in the model reproduce the observed cycle from Copernicus data. The outcome is a model calibration which is at odds with the FAPAR product used in the evaluation.



We have included a figure showing the MODIS MCD15A2H LAI product (https://lpdaac.usgs.gov/products/mcd15a2hv061/), which uses observations from both the Terra and Aqua satellites as well as the Copernicus LAI product "Copernicus Global Land Service (CGLS) Collection 300m LAI" (product page: https://land.copernicus.eu/en/products/vegetation/leaf-area-index-300m-v1.0).

In SI Table 6 the relationship with model eq. 147-149 is clarified by the parameter estimates. Particularly see parameter 'clf', which is the reciprocal of the annual leaf loss fraction (and so is proportional to leaf lifespan). Our calibration generated 'clf' values between 1.0-1.5, which represents deciduous ecosystems. Evergreen systems with multi-year needles would have clf>2. We are currently developing processes to correct the bias introduced by the highly seasonal LAI products used in model calibration.

In section 5.1 we now state:

"The level values of the observed FAPAR match the expected behaviour of the largely aseasonal evergreen canopies of the PFTs for the boreal region. The pronounced seasonal cycle of FAPAR in the model runs corresponds to a seasonal cycle in the LAI of the model. The modelled LAI behaviour results from calibration using Copernicus LAI time series which have a strong (and unexpected)

seasonality."

Section 6.3 now states

"While the initial task to match and compare modelled and observed data streams was successfully demonstrated, the results of this study also point at the need to further investigate the representation of the seasonal cycle of LAI in northern evergreen conifer forests and shrubs. Earth observation products for the boreal region show seasonality in LAI that is not consistent with ecological expectation and FAPAR data. The phenology scheme of D&B has the flexibility to simulate vegetation with a low seasonal variation in LAI, if corresponding information is provided for the prior calibration of the parameters in the phenology scheme. Such information could come from field observations of LAI time series in boreal regions or improved satellite products."

In the supplement, the following corrections were made:

"Losses from the foliar pool are linked to specific periods in the annual cycle through parameters for the day of leaf fall (d_fall) and ***period of leaf fall",*** *instead of "labile release"*

"c_lf is the ***reciprocal of*** *annual leaf loss fraction,* ***and so is proportional to*** *leaf life span" instead of "related to"*

8. L541-548: This paragraph is quiet generic and in my opinion applies to process-based models in general. It could in my opinion be extended to highlight how this is different for D&B.

We have added the following sentence:

"The potential advantage of D&B coupled to multiple observation operators is that it allows model testing via multiple data streams, thus providing are more comprehensive model evaluation which makes it less likely the model matches observations while misrepresenting important processes."

In addition, we added that stastical and machine learning models are used as black boxes so the question of being right for the wrong reasons does not even occur.

Minor comments:

1. L25f: The sentence contains some small language issues:

- I suggest to change "terrestrial carbon stores" to "terrestrial carbon storage" because stores has multiple meanings.

- Unclear what "those variations" are. I assume variations in C fluxes and storage but it should be clarified.

-Unclear what is meant by "forcing factors". I assume changing climatic conditions (e.g. temperature change and so on) but it should be defined.

We have replaced "stores" by "pools", and "those variations work and interact" with "variations in carbon fluxes interact".

We have added "(such as climate, land use, CO2 fertilisation)".

2. - 7. *These minor comments have been followed as suggested*

8. L150-157: I have several minor issues with understanding:

- Is root water supply capped at field capacity? I assume yes but it is not stated.

- "Actual stomatal conductance are then set such that transpiration is capped at the root supply rate": First, it should be "root water supply rate" not "root supply rate". Second, I would assume that it is capped at the minimum of root water supply rate and demand for transpiration. Can you splease confirm and elaborate this.

- Could you add a reference to the equations of the supply-demand calculation to make it easier to find.

We added ", reaching a maximum with soil water at field capacity"

The sentence has been modified to "Actual photosynthesis and stomatal conductance are then set such that transpiration is downregulated from its potential rate to the rate of maximum root water supply." We have also added a reference to the SI, where the equations and the reference can be found.

9. Section 2.2.: Variable names are introduced in some parts of the sections (e.g. L135-145) but not in the entire section. I think this should either be consistent or the authors should explain why they introduce certain variables in the main text and others only in the SI.

We agree, this paragraph sticks out as listing many variable names that are not later used in the main text. As suggested, we have removed non-esssential introduction of variable names here for consistency.

10. - 12. *These minor comments have been followed as suggested*

13. L193 "ensure" instead of "insure". Also I do not understand where the "separate calculation of FAPAR at the correct solar zenith angle" has to be performed. Is it within the model or is it a correction of data from observations. If the first, what is the difference to using FAPAR calculations from the model run?

We have simplified the description here and now only refer to how it is done in the model – which is a reasonable approximation. Section 3.1 now states:

"Therefore, the observation operator for FAPAR utilizes FAPAR calculations performed within the model's photosynthesis component at the times and dates where model and observations solar zenith

angles match.

14. L261: Delete “bptj” and please elaborate how the parameters were chosen. Was this part of the calibration or an expert assessment?

We have replaced “chosen” by “calibrated such that the model reproduces”.

15. *Done as suggested.*

16. L303, 442 and other occurrences: Here the authors refer to Sodankylä as the boreal site deviating from their so far consistent terminology. Similarly they sometimes refer to Majadas de Tietar as the savannah site.

We have kept switching between both, as this is explained in Section 4.1 (“Study Sites”)

17.-22. *Done as suggested.*

23. L373ff: This sentence is quiet long and I do not fully understand its meaning. E.g. “[...] measured NEE [...] that are not reproduced by the measurements”. I believe one of these should refer to simulations and not measurements.

Split into two sentences and dhanged to “… are not reproduced by the model. Such fluctuations are also found in the observation-derived TER flux.”

24. L383f: What is the reason for the overestimation under favorable conditions?

We added “, while overestimating photosynthetic capacity.”

25. *Done as suggested.*

26. L391: This is true for GPP (2.11 vs 2.25 modelled and 3.39 observed) but not for NEE (1.88 vs -0.09 modelled and -0.05 observed). Neither the order of magnitude is similar nor is the difference to the model mean smaller. I also do not understand the significance of this results. Please elaborate.

We are sorry, this should have read “RMSE of GPP and TER …”. We have also modified the last sentence of this section to: “While the high r^2 suggests that the model reproduces the interannual variability of the net carbon fluxes well for this site, the combination of rather high RMSE and similar observed means suggests that day-to-day variations are less well captured.”

27. Section 6: When referring to the results references to the respective sections are missing.

We did so in order to interrupt the flow of the text. We believe it is easy enough to identify the relevant sections.

28. *Done as suggested.*

29. L565f: I am not sure that this can be generalized. The process model may also not be able to match observations for a specific variable within reasonable bounds of the parameter space if the process is implemented but its formulation is not universal and therefore not applicable to the context of the experiment. So you cannot say that a process is missing but only that either a process is missing or the formulation of processes used is not suitable.

We have changed "by missing process" to "by missing or unsuitable process representation".

30., 31. *Done as suggested.*

32. Fig. 3, 4, 6 and 7: You could consider adding the 5th to 95th percentile values to illustrate inter-annual fluctuations. This would in my opinion also underpin your results where you compare inter- and intra-annual match between observed and simulated data.

As there are only six years, we have instead plotted the highest and lowest value found for each specific day of year within the time series.

After careful analysis of the revised figures, we have re-written the 2nd paragraph of Section 4.4. This now reads:

"For the energy flux evaluation (Figure 4), what stands out is the good agreement between modelled and simulated SHF, except for the spring (ca. DOY 50 to 100), where observations exceed simulations. LHF is also well matched during the summer, (ca. DOY 120--260). Since in the model, energy balance is exactly fulfilled, we would expect an equally good match for the net energy input (i.e. net radiation minus ground heat flux, cf. SI Equ. 124. 127) if the energy balance is also fulfilled for the observations. However, observations during the summer period are systematically lower than simulations. Therefore, we attribute the mismatch in net radiation during the summer to a lack of energy closure of the eddy covariance measurements. However, for the winter months, SHF is in good agreement, but both net energy input and LHF show systematically higher values for the model, and hence there is no evidence of lack of energy closure for the measurements. The difference might thus be mostly due to an overestimate by the model."

33. Fig. 4, 5, 7, 12, 13, 14 and 15: Captions are missing the color scale.

We are not sure we understand, but the meaning of the colors are explained by the legends shown in the figures.

34. *Done as suggested.*

35. SI Title of section 1.2.4 only refers to evaporation but section describes also transpiration.

Title changed to "Evapotranspiration from vegetation". Thank you for spotting this.

Reviewer #2 comments

General comments:

1. This paper sounds like a well written technical report and needs major changes before it can be published in GMD as a model description paper. A number of models, not all very complex, are mature enough to do the same job (and more) as the one proposed by the authors."

We appreciate the comment, and agree that there are many combined land surface water, energy and carbon cycle models in existence, and there are certainly models out there that are also mature enough. However, we are not aware of and could not ascertain that there a such models with a proven history of land surface data assimilation. A possible reason for referee #2's opinion here is that he or she refers to a much broader range of models and modelling frameworks, including weather forecasting and climate modelling.

For clarification, we have added two paragraphs discussing the suitability of other models for the task (see reply to Reviewer #1 Major Comment 1)

2. Why is this new model needed? In terms of process representation, I could not see any innovation in the proposed modeling framework, except for the simulation of SIF and VOD. I assume that the innovation is in the data assimilation part, but no example is shown.

The observation of referee #2 is correct insofar as the novelty of the described modelling systems is related to its data assimilation capabilities. These capabilities depend on two things: 1) a set of observation operators that translates model output into observables, and (2) the data assimilation framework. We have deliberately refrained from including (2) as this would have unnecessarily made an already long manuscript unwieldy. The innovation can thus be found in the observation operators for FAPAR, VOD, SIF and surface soil moisture. We would also like to point out that there is currently no VOD observation operator or even approach available in the literature that is able to exploit short-term (i.e. sub-annual) variations in VOD.

We believe that the text added comparing D&B to previously published models provides the necessary clarification (Reply to Reviewer #1 Major Comment 1).

3. Since data assimilation is not demonstrated here, "data assimilation" should be removed from the title. Similarly, data assimilation is mentioned frequently throughout the paper, but no example is provided. Data assimilation could be mentioned briefly in the Introduction and as a perspective in the Discussion section. No more. A clear definition of data assimilation is also lacking. Data assimilation can be done in many different ways. As far as I could understand, in this paper data assimilation is equivalent to "model parameter tuning". This is quite different from the variational or sequential Kalman filtering methods used in meteorology and in some land modeling frameworks to initialize initial conditions (e.g. of root-zone soil moisture) at a given time. This should be clearly explained.

As mentioned in the previous comment, the title includes the purpose of the model to be used within *data assimilation frameworks. If we drop a reference to data assimilation in the title, we run the risk of losing the main target group of this manuscript, which is the community interested in using various data assimilation methods to infer important parameters of land surface states. This was also the purpose of the European Space Agency project the work was funded by.*

As far as a the specific method of data assimilation is concerned, we would like to repeat that this is specifically and intentionally not part of the manuscript, since the model could be used for various data assimilation setups. However, we can see that this was not clear from how we referenced data assimilation.

4. It should also be explained why the authors do not trust their default model parameter values. I assume that these default model parameter values come from the scientific literature. Why not trust them?

This is a valid question. Our data assimilation work rests on Bayesian statistical principals which specifically assume that no measurement, parameter value of model formulation should ever be fully trusted. We would like to reserve a full discussion of this for a later manuscript that demonstrates the data assimilation system built around D&B.

To clarify this, we have added the following text to Section 1:

"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)."

5. Finally, a critical risk of parameter tuning is that the tuned model might be good for bad reasons, which is unacceptable for a model that aims to explicitly represent the main biophysical processes ("process-based modeling system"). This is acknowledged by the authors on L. 546. But how do the authors ensure that this does not happen? This is not clear.

We can see what Reviewer #2 alludes to here, and, as mentioned, we discuss this point already. However, there is very clearly no set recipe to prevent that a model is "right for the wrong reasons". It is a matter of expert judgment and requires both knowledge of the model, experience with its behaviour, and an intimate knowledge of the processes represented, ideally underlain by field experience.

We have added the following statement to the end of Section 6.3:

"The potential advantage of D&B coupled to multiple observation operators is that it allows model testing via multiple data streams, thus providing are more comprehensive model evaluation which makes it less likely the model matches observations while misrepresenting important processes." (Also in reply to Major Comment 8 of Reviewer #1.)

6. Finally, the rationale for "parameter tuning" is that improved static model parameter values are needed for the surface component of climate models and for climate change impact models. I do not believe that the current model is designed for such applications. What is the real purpose of the model? For monitoring and reanalysis, sequential assimilation would be preferable to model tuning.

This is very good point. We agree with the argument or Reviewer #2 that, when it comes to the purpose of data assimilation, deriving static parameters lends itself for the improvement of process representation (e.g. in climate or climate impact models, as mentioned), and adjusting initial conditions lends itself naturally when it comes to monitoring or short-term forecasting. However, there are many more possible configurations, including adjusting input data, or a combination of adjusting all those simultaneously, i.e. input data including land type classifications, initial conditions. In fact, in a probabilistic Bayesian methodological framework, not adjusting any part of the model is always a compromise between mathematical accuracy and computation feasibility.

See reply to Comment 4 of Reviewer #2 for text we have added to Section 1. The text should clarify that the D&B framework is open to all of such application.

7. This work combines the BETHY and DALEC models. A comparison of the model simulations is presented over two contrasting sites (Spain and Finland). Why these two sites in particular? Results from the comparison are not good, which tends to show that this new model is not a good model. Or maybe these sites are particularly difficult to represent? Could you indicate score values from other models over these sites?"

The reason for choosing those two sites are that, while both are located in Europe, they cover a large variation in ecosystem properties. The parameters chosen were not site-specific and therefore we do not expect a perfect match.

At the start of Section 4.1, we have added "The D&B model is run for two study sites ***with widely varying climate****."*

Particular comments:

8. L. 54-59 (list of requirements): Is it something that other models could not do?

See the added discussion of alternative models (reply to Reviewer #1 Major Comment 1).

9. L. 97 (daily time step): I believe that the daily time step is not sufficient to represent snow processes. Especially when snow melt occurs.

We have included a comparison between modelled and observed snow height and find a rather good agreement.

10. L. 154 (potential photosynthesis): It is not clear whether potential photosynthesis varies from one day to another according to solar radiation and leaf temperature. Could you clarify?

The answer is that it does. We have added: "This rate of demand is determined by the potential rate of photosynthesis without water stress ***computed previously at each time step****."*

11. L. 275: The ICOS data portal contains a large number of sites. Why have you selected these two sites in particular?

See answer to Comment 7 of Reviewer #2.

12. L. 324 (overestimate of GPP): Over the Boreal site, GPP is much more than "overestimated". There is nearly a factor of two at summertime. Why is the model that bad?

A factor of two can easily obtained within the uncertainty of model parameters, such as Vmax, or state variables, such as LAI or fractional cover.

13. L. 433: On L. 214, Ssif = 1 and on L. 434 Ssif = 10. Could you explain why?

We have added the following sentence to the second paragraph of Section 5.2:

"While the prior value of s_SIF was 1, this change reflects the high uncertainty regarding the absolute magnitude of the measured SIF."

We have also added the following clarification:

"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. al (2019), ***and the specific spectral conversion used (Equ. 2)****. "*

14. L. 481: "in the simulations, soil moisture decreases to near zero": why? Could this be caused by the overestimation of soil evaporation (a classical modeling problem)?

Thank you for pointing this out. In D&B, soil evaporation when approaching zero soil moisture decreases linearly with soil water content (see Eq. 83 of the SI). Judging from the observations, it may be possible that the relationship is much more non-linear than expected. Unfortunately we lack measurements of soil evaporation and are therefore unable to systemtically explore alternative formulations.

We added the sentence

"We also find that the model may overestimate soil evaporation for very dry soils."

to Section 6.3 (3rd paragraph).

- L. 486-487 (carbon fluxes [...] are simulated reasonably well): You cannot say that for the Boreal site.

Agreed. We have added the following qualifier: "... reasonable job at representing energy and carbon *fluxes between the atmosphere and terrestrial vegetation**, albeit with the seasonal amplitude of the net carbon exchange overestimated at the boreal site**".*

We do not refer to either GPP or TER, because these are derived quantities and not direct observations.

15. L. 529: The large uncertainty on Ssif shows that the biophysical basis for SIF is very weak in the proposed model. Why not using machine learning to build an observation operator for SIF?

The link between electron transport and SIF is well established in the literature. D&B explicitly models the electron transport, and so it makes sense to couple the Gu et al. (2019) leaf level SIF model in this manner. However, all SIF models rely on some form of internal parameterisation and it is clear that some work is required on how we couple D&B and the Gu model. We see this as an opportunity. The more we are able to strengthen that link between D&B and the leaf level SIF model, the better we are able to interrogate the model performance at a process level.

In principle, we have no objection to building machine learning emulators to serve as observation operators, but we argue that this is best done once the biophysical mechanisms are well understood, otherwise the emulator becomes a black box where we may struggle to interpret the results. Understanding of the biophysical mechanisms is best served, in our opinion, by building process level models.

We have also added the following paragraph to Section 5.2:

"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). Although it has not been done here, there is scope within D&B to adjust these parameters in the assimilation. We believe, however, that it is more important, in the first instance, that we have a model that can track the seasonal and diurnal cycle of the observations, and this appears to work reasonably well."

Further edits:

1. In the introduction (beginning of last paragraph of Section 1), we have further clarified the role of the LCC study:

*"In this contribution we present the newly developed D&B (DALEC & BETHY) model together with original measurements from two study sites of widely varying climate and vegetation**. Both, the model development and the field campaign, were carried out** within the ESA-funded Land surface Carbon Cycle (LCC) study.*

2. We have added a further reference to a study for eddy covariance data assimilation with BETHY, Section 1, 6th paragraph: Kato et al. (2013).

3. In Section 3.3, an explanation of the wavelength dependence of parameters was added, in addition to a few minor edits.

4. In Section 3.4, we tried to better clarify the dual purpose of the surface soil moisture layer.

5. Small clarifications at the beginning of Section 4, and the end of Section 4.3.

6. We have added a clarification to the 4th (last but one) paragraph of Section 4.5: "For instance, at the start of the years until ca. DOY 130, net radiation and LHF agreement suggests an imbalance ***in the observations*** *starting close to zero at the start of the year* ***and*** *increasing to around 40 Wm-2."*

7. In the 1st paragraph of Section 6.2, we have made the following clarification:

" The anticipated default setup in data assimilation mode is for combined calibration ***and initialisation****, i.e. adjustment of parameters of the process model, its observation operators and of the initial state of the carbon pools."*

8. We changed the statement on code availability from future to present tense.

9. There has been an error in the author contribution, which has now been corrected:

"WK and ***MW*** *provided original model design and description of D&B."*