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Interactive comment on "Estimating global gross primary productivity using chlorophyll fluorescence and a data assimilation system with the BETHY-SCOPE model" *by* Alexander J. Norton et al.

Anonymous Referee #2

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This paper aims at improving the BETHY-SCOPE estimated GPP through assimilating SIF data into the model. The results did show a substantial improvement of simulating SIF over different periods. However, the improvements in GPP are very limited. The presented method of assimilating SIF product in constraining model parameters in estimating SIF&GPP is very interesting and could be potentially used for many other models and also for other RS products. The authors have put extensive focus on describing this data-model assimilation method and related results, but there is generally lacking of information about how the model works, like how these sensitive parameters

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influence SIF and GPP and how the SIF-based optimization could potentially improve the GPP estimations. Also, there is a lack of discussion about if the changes of parameters values after optimization make sense.

General comments: In general, there is no clear explanation about how SIF is linked to GPP in the model, which is very central for readers to understand the work. How the parameter Vcmax and Cab is used in the model and how these key parameters regulating the information translated from SIF to GPP are missing. What do these under-estimated high SIF value mean in terms of GPP modelling? I would strongly suggest the authors to add more information in the model description part and a deep discussion about potential linkage of uncertainties from SIF evaluations to GPP estimations.

The authors argue there is an improvement of GPP in global distribution relative to independent estimates after assimilate SIF into the model (from Abstract). However, from the result (Figs. 11 and 12, the absolute value of GPP in Table B1), we can see very limited improvements (sometimes worse estimations), relative to the FLUXCOM and TRENDY products. The authors refer the improvements to the closer value of GPP ratio between tropical and subtropical regions, but this ratio is mainly influenced by the increase of GPP in northern extratropical regions. This increase of GPP in the extratropical region after data-model assimilation is however not closer to the FLUXCOM and TRENDY products.

There are certain/large limitations in terms of satellite-derived LAI for some regions (like tropics). This work use prescribed LAI from MODIS as inputs, so how the limitation in MODIS-derived LAI could potentially contribute to the discrepancies we see between the modelled GPP (both before and fater the data-model assimilation) and the other two products? The authors could potentially test use other ecosystem model-based LAIs to drive the model and figure out the impacts of prescribed LAI on the GPP estimations.

About structure: There are large parts of text describing methods (e.g., in Section 3.1.3

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and 3.1.4) were placed in the result section. The authors should re-arrange the text a bit. Also a clear description of different sensitivity tests (like Vcmax, seasonality, etc.) and associated reasons for these tests to fit the aim of this study should be added in the method section.

A lot of discussion text with references has been placed in the result section. The authors would consider merging result and discussion sections.

More detailed comments are listed here:

1. Any special reason to choose 2015 as calibration period, and use a few month data from 2014 as validation data, not opposite?

2. P3, L15: what is observation operator?

3. P4, L1-2: Does the model simulate the fractional coverage? Or the fractional coverage from some data?

4. P5, l14, "... we assign relatively large prior uncertainties..." what are the methods used for defining prior uncertainties?

5. Section 2.2, Strongly suggest to add a column with short explanation of each parameter. It is difficult to read this table alone.

6 Both SIF and LAI data are gridded to 2 by 2 degree resolution, which interpolation method was used? Please mention it.

7. P13, L9-10, "... these large SIF values typically occur over tropical forest, grassland and cropland regions..." can the authors explain it why?

8. P20, Line 2-5, if the FLUXCOM GPP is not a validation data and the values are too low, as mentioned, could the authors elaborate a bit more why they think the FLUXCOME GPP is too low?

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