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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.

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We thank the referee for their comments. Here we provided a response to each comment. We hope that this new version is much improved. Note that original referee comments are enclosed in < > symbols.

While we have made the necessary changes following the referee comments, we have also rearranged and clarified parts of the discussion. We also noticed that we include one figure both in the manuscript and in the supplementary material. This has been

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removed from the supplementary material.

<This paper is a revised version of previous submission in 2018 with the same title. As a reviewer for both papers, I found the authors made helpful improvements but with new problems. In the earlier comments, I raised two main questions: First, the GPP is not effectively improved with SIF assimilation. Second, the GPP-SIF relations are not well explained. Compared to the earlier version, this revision improves the second aspect but still fails to show a reasonable improvement in GPP. >

<The authors include more details about how GPP and SIF are connected in the model. In general, these two variables have some offsetting phases, because they share the same radiation energy. Such relationship explains why the posterior parameters reduce the high biases in SIF (Fig. 4) and consequently promote GPP (Fig. 10). However, compared to the 2018 paper, SIF is higher in the 2019 paper and is closer to observations (Fig. 4). Then why the GPP is much higher in this paper (167 Pg C yr-1) compared to earlier version (137 Pg C yr-1), instead of lower value? It shows that the SIF-GPP assimilation system may be arbitrary or casual about parameter adjustment. >

Given the concerns from RC1, it is perhaps helpful to point out the specific differences between our last assimilation setup and the present one. While it is not a focus of the paper to distinguish between these two, it is useful to present this in more detail here than is feasible in the manuscript. Here are the major differences in the assimilation setup between our current manuscript and the previous version to which the referee refers to: (i) The prior chlorophyll (Cab) parameters are set to be higher. The prior Cab parameters presented in the earlier version were too low (with a PFT average of 13 μg cm-2). This is not considered realistic as anything below $\sim$20 μg cm-2 suggests light interception is very low and will strongly limit photosynthesis (see Fig. 3.4 in Bjorkman, 1988); this is not expected under most natural conditions. This change in prior Cab values means our sensitivities (the slope of SIF with respect to Cab) were too large, as the expected change in SIF from Cab would be very large given it is strongly light-

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limited. The effect of more realistic Cab values would be an increase in APAR and increase in GPP. We also set the 1-sigma prior uncertainty of all Cab parameters to be a consistent 10% of the mean value. (ii) The APAR provided to the photosynthesis module was total APAR in the previous version. In the SCOPE model, the total APAR represents the absorbed radiation by all canopy leaf elements. However, only green chlorophyll directs absorbed radiation to photochemistry. The model was therefore altered so that the green APAR was provided to the photosynthesis module, a change that has also been made to a more recent version of the SCOPE model. The effect of this more realistic model setup would be a decrease in APAR provided to GPP, hence a decrease in GPP. (iii) The version of Fluspect used in our last submission was not actually that of SCOPE v1.53. Therefore, we had to update the version of Fluspect in our model. Fluspect simulates the leaf level fluorescence and calculates the leaf level reflectance, transmittance and absorbances. The main issue was that the fluorescence quantum efficiency used in Fluspect (termed “fqe”) was the same for photosystem I and II, but actually the values should be such that photosystem I is one fifth the value of of photosystem II. Therefore, the simulated SIF would have too high a contribution from PSI, which is not affected by physiological changes e.g. Vcmax.

These changes therefore include a change in model formulation and the prior parameters. We are confident that these changes make the model more realistic. The first two changes, i and ii, also have opposing effects on GPP, hence the prior GPP is similar between the current manuscript and previous version. The major difference is therefore in the sensitivities between SIF and the parameters, particularly Cab. This means our Jacobian matrix (H) is also different. The third change, iii, changes SIF but not GPP, but it will change the Jacobian matrix (H) as the contribution photosystem II to canopy SIF is larger and it is this photosystem that is regulated by physiological feedbacks. In layman’s terms, the assimilation has different knobs and dials that it can use to minimise the cost function. If the model formulation or the Jacobian changes, these knobs and dials will change in size and strength, thus the posterior will also change.

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There are a couple of notable differences in the posterior parameter set that probably cause the higher global GPP is in this version: (i) posterior Cab and leaf angle distribution (LIDF) parameters are different, with Cab being higher on average than our previous version (average of 13 PFTs is 22 μg cm-2 compared to 5 μg cm-2 in the last version), hence the new parameter set has higher APAR; (ii) the posterior Vcmax is slightly higher than the last version, with a PFT average of 61 μmol m-2 s-1 in the current version and 57 μmol m-2 s-1 in the previous version. This results in a higher APAR and higher LUE, hence higher GPP.

To address this, we have added a model version (now BETHY-SCOPE v1.1) to the methods to distinguish this version of BETHY-SCOPE from the previously submitted manuscript as well as the one used Norton et al. (2018) GMD paper (BETHY-SCOPE v1.0). We have also added a comment in the model description section of the Methods to highlight the changes: "In BETHY-SCOPE v1.1, the key changes are (i) the correction of an error in the Fluspect module where the fluorescence quantum efficiency (fqe) for PSI and PSII were set to be equal, while SCOPE v1.53 sets fqe for PSI to be one fifth that of PSII, and (ii) the leaf biochemistry module is now driven by green APAR (as mentioned above), rather than total APAR that is used in SCOPE v.153."

<The simulated GPP is much higher than present-day estimates from other studies/models. Results in Fig. 11 show that the 'improved' GPP is way too higher than the values from FLUXCOM and TRENDY. The authors claimed that GPP from FLUXCOM and TRENDY may be biased in tropics due to the limits in observations (Page 22). However, for mid-high latitudes (35-60EÌĹZÌŇN) in Northern Hemisphere where most of FLUXNET sites locate, the SIF-derived GPP values almost double the FLUXCOM. As a result, I think the assimilation system may have systematic biases, either from parameters (e.g., Fm',IÌĹT Ìğp) or physical processes (e.g., the Equations 1-3), that degrade the values of this framework. In a word, the improvement of SIF does not effectively improve GPP. >

A recent review showed that global GPP is far from well-constrained as credible estimates range from 112-169 Pg C yr-1 (Anav et al., 2015). More recently, another SIF assimilation study by Macbean et al. (2018) produced a posterior global GPP of 166 Pg C yr-1, almost identical to our posterior estimate (although both have uncertainty ranges). Joiner et al. (2018) used GOME-2 satellite SIF and flux tower data to quantify global GPP and produced a global GPP of 140 Pg C yr-1 (in 2007). Other recent studies using different data suggest other estimates of 147 Pg C yr-1 (Badgley et al., 2018), 150-175 Pg C yr-1 (Welp et al., 2011), and 147 $\pm$ 19 Pg C yr-1 (Koffi et al., 2012). This large range of estimates reflects the lack of good direct observations and the inherent difficulty in quantifying global GPP. It also demonstrates that our posterior GPP estimate is not outside of other credible estimates, despite being at the higher end.

Overall, we do not state that our posterior global GPP wholly and completely "improved". In fact, what we state in our findings reflect our results quite directly e.g. in the abstract: "The SIF assimilation increases global GPP by 31% to 167 $\pm$ 5 Pg C yr-1 and shows an improvement in the global distribution of productivity relative to independent estimates, but a large difference in magnitude."

We respect the critique of the referee, but we do not find evidence that our GPP is beyond a credible range or that our parameters have systematic biases. Does the referee have some suggestion as to how they conclude our GPP is too high? Alternatively, is there are recommendation of what we should be changing?

We do not expect to produce a perfect or accurate global GPP estimate. In fact, we cannot even test whether our global GPP estimate is correct as there is no strict validation data for GPP at this scale. The FLUXCOM and TRENDY GPP products are used as a guide for the general patterns of GPP. As such, we assume the patterns of the FLUXCOM GPP are a useful check on our GPP patterns, but only in regions with plenty of flux sites, which is why we do the analysis in Figs. B5 and B6. We have made some changes to the discussion to make our interpretation clearer and to highlight the point that GPP estimates vary widely.

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