

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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GPP is the largest carbon flux and constraining it is very important for understanding the terrestrial carbon sources and sinks. This paper presents a method to estimate the GPP in a data assimilation system based on the OCO2 SIF products. Compared to previous studies mainly based on the linear relationship between SIF and GPP, this paper adopts the process-based manner in which terrestrial biosphere model explicitly simulates the GPP and SIF. It is a new pathway to constrain GPP using the satellite SIF products. Also, there are some concerns about the results. I list them as follows: Several major concerns: (1) Actually I also noticed your previous online version

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about this paper (Norton et al., 2018, Biogeosciences Discuss). I find that GPP can be largely increased by 31% in this new manuscript, while it was only increased by 7% in the previous one. So what's the difference in the background assimilation process? I carefully compare the prior parameter values in the Table A1, and find the only differences in the chlorophyll ab content (Cab). Is this the only difference? You mentioned that the Cab is set more in line with physiological understanding here (P6 Line 33-34). So what's the reference? If only tuning the prior Cab values makes the large difference, how to explain? The Cab value is only related to SIF not to GPP. (2) As you mentioned that the calculation of observation uncertainties is an important aspect of the data assimilation study. You calculate the observation uncertainty with a scale of 1/2 (Equation 4, P8 Line 11-20). How do you determine this scale? Sensitivity experiments? (3) P19 Line 9-10. You say that the changes in the posterior GPP can differ in sign and magnitude from the changes in posterior SIF. You explain it as a result of the non-linear effect in the process-based approach. First, you use the same SIF module as your previous manuscript (Norton et al., 2018, Biogeosciences Discuss)? It seems that the non-linear effect is not obvious in the previous one, but significant in this one, why? Second, If the nonlinear effect is obvious, how can we determine the GPP can be truly optimized? (4) P15 Line 1-3. You say that the SIFprior does not show this systematic underestimation, but has a poorer global fit (Fig. S3). If we look at the Figure S3, we can find that modeled SIF in lots of grids keep near constant (below 0.5). Or actually the scattering turns out the linear relationship is not statistical significant. But you show the $p < 0.001$, I think it is because you do not calculate the effective degrees of freedom, instead that you use the number of the points to calculate the p values. So the SIF module itself has large model errors. (5) In Section3.2, you show the results about the posterior parameters. You say “we can be more confident in parameters that see large reductions in uncertainty. Conversely, parameters with little reduction in uncertainty following optimization should be accepted cautiously.” Actually, in the data assimilation, the uncertainty should be more or less reduced owing to the mathematics. But the reduction of the uncertainty does not mean the optimized

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parameters are more accurate, because parameter optimization accounts for the LAI uncertainty, model structural uncertainty etc. In fact, posterior parameters can only partially improve the SIF simulation. In Figure 4, we can see the posterior SIF is more in line with the observed SIF. Therefore, the posterior parameters may be over-tuned. So is it possible to validate the posterior parameters based on the other datasets.

Specifics: (1) P1 Line 19. “(see Anav et al., 2015)/P2 Line 7 (see Baccour et al., 2015; ...)” ->remove the ‘see’ (2) P 6 Line 11 “Overall, the modelled link between SIF and GPP occurs via the above equations” ->actually it is not clear according to the above three equations. (3) P9 Line 11. Miss an “and”. Should be “by the uncertainties in the observations Cd and model parameters Cx, respectively” (4) P11 Line 7-8. I can not clearly understand the sentence “... but forced by the respective monthly mean diurnal cycle such that a single diurnal cycle is simulated for each month” (5) Figure2 and Figure 3 can be presented in the same color bar. (6) P13 Line 14 “underestimate large observed SIF values $> 0.5 \text{ W/m}^2/\text{sr}/\text{um}$ ” (7) P15 Line 5 “This is largely because of observed SIF values that are slightly negative”. Can it be shown in Figure 1 with the negative color bar. (8) Section 3.1.3 “A case with seasonally Varying Parameters” can be regarded as a discussion in the Section discussion. (9) P16 Line8-12 Is it possible add an equation here. (10) P 16 Line 17-18 “..with R2 increasing from 0.74 to 0.77 and the slope increasing from 0.67 to 0.71. This indicates that the systematic under-estimation of large observed SIF values may be improved.” This conclusion is vague without the figures. (11) P20 the comparison between FLUXCOM and posterior GPP over the North America. The spatial correlation has an improvement with increasing correlation coefficient from 0.89 to 0.95. However, the amplitude is much larger than the FLUXCOM GPP. So it is improved or turns out poorer because you also mentioned the FLUXCOM GPP over north American and Europe may represent the actual GPP? (12) Maybe can adjust the orders of the Appendix figures. You first describe the Figs. B5 B6, then describe the Figs. B2 B3.. (13) P22 Line6. You say “In both of these studies an increase in tropical GPP was found”, In Macbean et al., 2018, the posterior GPP seems a reduction?

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