Biogeosciences
Discussions

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Interactive comment on "Sun-induced Chlorophyll fluorescence and PRI improve remote sensing GPP estimates under varying nutrient availability in a typical Mediterranean savanna ecosystem" *by* O. Perez-Priego et al.

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Please note the following distinctions in types: -> comment from the Reviewer <- response from the authors

-> This paper by Perez-Priego et al. evaluates the performance of sun-induced chlorophyll fluorescence and the PRI to estimate GPP variation in response to nutrient availability in a Mediterranean savanna grassland. In addition to being a very well planned and executed study, the authors conduct a detailed analysis comparing the predictive

power of SIF and PRI to that of baseline alternative approaches using greenness and other meteorological data. They show that both SIF and PRI correlated well with GPP in response to fertilization, while greenness indices (i.e. NDVI and MTCI) failed to do so. Despite that some sentences are written in a bit complex stile, the work is well presented and the text flows well. Overall, I believe this is a significant and original contribution that adds to the increasing number of studies evidencing the potential of SIF and PRI to improve our capacity to estimate GPP dynamics, in this case, in response to plant nutrient status. I think this study certainly deserves to be published in Biogeosciences after a few issues are addressed.

<- We greatly appreciate the Referees' in-depth review and constructive comments. The following pages contain our responses to each of the comments from the Referee and detail the changes we have made as a result of remarks from the Referee.

->1) The use of the term GPP is a bit confusing. The authors use the terms GPP2000, maximum daily GPP, daily average GPP, and then "GPP" alone. Accordingly, it is not clear what exactly it is denoted when "GPP" is used alone (instantaneous?, mean?, max?, noon?...). The authors should clarify the terms throughout the paper.

<- We thank the Referee for this comment and apologize for the confusion. We agree that GPP is used in a confusing way throughout the manuscript without specifying neither its temporal resolution or the timing on which it was measured (or modeled like in the case of the GPP2000). We have considered the Referee's comment, and have included indices in the abbreviations list with the different definition of GPP described as follows: - GPP: gross primary productivity or instantaneous gross photosynthetic rate. - GPPnoon: instantaneous gross photosynthetic rate taken at solar noon (between 11:00 and 15:00 pm solar time). -GPP2000: gross photosynthetic rate estimated at 2000 of PAR derived from the fitting of the light response curve on GPP and PAR (Ruimy et al., 1995) -GPPdaily: mean value of the diurnal time course of gross photosynthetic rate. These abbreviations have been referred throughout the manuscript as suggested.

-> 2) From the MM it is understood that chamber measurements were conducted with three rotating chambers and that each measurement lasted for approximately 3 minutes. But there is no information on what was the temporal range of these measurements: Where they conducted from sunrise until sunset? This seems to be the case otherwise they could not have constructed their light response data in Figure 2. Once this is clarified they could also mention briefly how was maximum daily GPP and daily average GPP calculated. In the same lines, it would be good to mention explicitly how the PLRC curve was obtained (e.g. by pooling together all diurnal measurements for each treatment and sampling date).

<- The Referee can read in the text (p11902-L11:13): "Specifically, the Michaelis–Menten function was fitted to GPP and PAR data taken throughout the day for each field campaign as follows...". However, to better clarify the temporal range of these measurements, this reads now: "Specifically, the Michaelis–Menten function was fitted to GPP and PAR data taken throughout course of the day (from sunrise until sunset) for each field campaign as follows..." As we mentioned above, a better description regarding the use of GPP has been adopted throughout the manuscript.

-> 3) Apparently, the authors use measured VPD and soil moisture as inputs in the MM model. Although it is stated that soil moisture was measured with a Theta Probe, there is no information as to how VPD was estimated. Perhaps they could add a clarification in Page 11899, lines 16-18: e.g. "Chamber humidity data was used to estimate VPD".

<- We have clarified this as suggested.

-> 4) It is stated that chamber measurements lasted for 3 minutes, but could the authors provide a bit more of information as to how long it took to reach equilibration before NEE was measured, and similarly, how long it took to reach the steady state for Reco after placing the dark cloth?

<- Following both Referee's recommendations, we have further described this in the manuscript. The chamber is open and ventilated during 1 min after NEE measurement

C5668

and the opaque blanket covers the chamber just after we place the chamber back on the collar. In addition of this 1 min, we must consider the starting time (15:25 s) – the period that defines the initial slope of the fit after chamber deployment on which the "undisturbed" flux is estimated. Obviously, this "starting time" time is not fixed since we must also consider the need of both to stabilize the chamber atmosphere following deployment and for transport of sample air from chamber to gas analyzer. This starting time is automatically estimated using a change point detection algorithm (Killick and Eckley, 2010), and implemented in the bootstrap resampling-based algorithm, (Perez-Priego et al., 2015).

-> 5) Page 11911, Lines 22-29. I think the analysis presented in Figure 9 and its implications are very interesting and the authors could expand a bit on it in the discussion. Their analysis in Figure 9 nicely shows the complementarity between NDVI and PRI/SIF. At low GPP levels, NDVI and not Fy760 or PRI respond to GPP, whereas at high GPP levels it is Fy760 and PRI but less NDVI that respond to GPP. Could we build on this complementarity to better track GPP dynamics?

<- We have considered the Reviewer's comment and have further discussed the implications of this analysis around these lines. "Figure 9 suggests that the relationship between NDVI and sPRI/Fy760 is not unique and NDVI may play an important role in driving GPP in ecosystem characterized by marked seasonal variations. Our results highlight the complementarity between NDVI and Fy760/sPRI. Particularly, NDVI assisted Fy760/sPRI in predicting GPP under conditions with low biomass (i.e. low LAI), when confounding factors may affect Fy760 and sPRI."

-> 6) As far as I understood the authors were feeding the MM models with field data (both VPD and soil water content). But, what would happen to MM performance if they would have used modelled/estimated VPD and SWC? Could it be that RSM would have been then far superior than MM? The authors might wish to briefly discuss (or even assess) how uncertainties in VPD and SWC estimates would propagate and affect the performance of MM in a real case scenario where no field data is available. As it

C5669

stands, the comparison between MM and RSM might favor MM.

<-We agree with the Reviewer that this is a relevant and interesting point that requires a further evaluation of both MM and RSM approaches for up-scaling purposes. Such would involve explicitly addressing uncertainties in the different forcing fields of the MM and RSM models, which goes beyond the objectives of this current work and would justify a unique study per se. However, we have acknowledged this point in the discussion section of the manuscript around P112-L26: "From a practical point of view, the forcing variables of RSM approaches may show a better observational coverage. In effect, the satellite-based retrievals of RSM forcing variables could additionally overcome representativeness limitations and potential regional or seasonal biases in meteorological fields (Dee et al., 2011) which could propagate to the predictive uncertainties of GPP."

-> 7) Page 11907. Line 5. There is no mention or data on GPP2000 in Fig. 2. (see also Page 11907, Line 16). The authors seem to refer to the differences in GPP2000 and GPP between treatments several times in the results and discussion but that analysis is not explicitly shown. How about adding GPP (daily mean, or max, or noon) and GPP2000 into the analysis presented in Fig 3 with two additional panels?

<- The methods describes widely how GPP2000 was estimated and results from the analysis were explicitly mentioned (P11907-L6:12; "GPP2000 was higher in +N and +NP treatments (18.6 and 20.1 μmolCO2m-2s-1, respectively) compared to C and +P treatments (14.9 and 15.4 μmolCO2m-2 s-1, respectively)...". We again apologize if the Referee mixed up among GPP acronyms and improvements have been made to address this issue. Regarding adding GPP in Figure 3, the results were structured over the manuscript in separated sections, where we try to walk the reader through nutrient-induced changes in separate variables i) photosynthesis (Fig 2) and ii) vegetation optical properties (Fig 3). For this reason, we would rather prefer to keep this structure.

-> Minor corrections: -> Page 11906. Lines 20-22. Is this correct? If I checked it right I

C5670

am getting 79.7 +-16.5 and 75.9 +-10.5. Are these significantly different?

<- The Referee is right and this error has been corrected in the manuscript. Now the sentence states: "Regarding variations in the fraction of plant forms, no significant differences were found between treatments."

->Page 11907, Line 15, is it Table 1 instead?

<- This typo has been corrected.

->Page 11907, Line 27. I am not sure do I understand what the authors mean with "As for chamber measurements,: : :" Did you measure optical properties both outside and inside the chamber? I could not see mention to that in MM.

<- We apologize for the confusion and we have explicitly added "Like GPP" instead.

->Page 11908. Line 21. Is there a typo in the "p<0.1"? The significance threshold is usually set to p values equal or below 0.05.

<- There is not typo here. We obtained p values of 0.0513 and 0.0878 for +NP and +N treatments, respectively, which barely would fail to reach statistical significance. However, if we consider sample size effects and the scatter in Fy760 owing to typical noise in the fluoresce signal, we could either set the threshold of <0.1 for significance or to say barely significant. We have added these considerations in the improved version of the manuscript.

-> Page 11909, Line22. Add "under high light" after photosynthetic capacity. A reference to the results where this is shown could be also added.

<- We have added the term and included the reference as suggested.

-> Page 11910, Lines 3-5. Point (2) should be rephrased. Increased photosynthetic capacity does not increase F per se, actually it should decrease it because photosynthesis and fluorescence compete for excitation. I believe the feedback the authors mean from the Cendrero-Mateo et al. paper refers to the simultaneous increase in

C5671

fluorescence and photosynthesis because of decreased NPQ. Rough suggestion: : : : "and on (2) the increased photosynthetic capacity that results in reduced NPQ activity and consequently increases the fluorescence signal (Cendrero-Mateo et al. 2015)."

<- We very much appreciate the expertise that the Reviewer brings to this paper and the sentence has been rewritten as suggested.

->Page 11911, Line 25. How was this 37.5

<- This is a result from the relative variation of modeled GPP (from 25 to 40 mmol CO_2 m-2 s-1) at highest NDVI values (see Fig. 9). Nevertheless, consistent with other comments from the Referee regarding Fig. 9, the paragraph in question has been rewritten, and no numbers is now given.

->Page 11913. Line 12 and Page 11903, Line 5. "Meteo-driven models" vs "Meteorology-driven methods". Better to use only one.

<-Corrected.