

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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Received and published: 25 August 2015

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

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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.

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. In addition,

2) From the M&M 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).

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”.

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?

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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?

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

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?

Minor corrections: - Page 11906. Lines 20-22. Is this correct? If I checked it right I am getting 79.7 ± 16.5 and 75.9 ± 10.5 . Are these significantly different?

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

-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 M&M.

-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.

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

-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 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).”

-Page 11911, Line 25. How was this 37.5% obtained?

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

Interactive comment on Biogeosciences Discuss., 12, 11891, 2015.

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12, C4682–C4685, 2015

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