

## ***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

-> Perez-Priego et al. report on an experiment in a Spanish oak savanna where the herbaceous understory has been to a N and P fertilizer application in a full factorial design. The authors measured the CO<sub>2</sub> gas exchange using ecosystem chambers and determined, notably from the same plots, hyperspectral reflectance and several

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canopy structural attributes (LAI, C/N contents). The objective of the paper is to assess how fertilizer application effects CO<sub>2</sub> gas exchange and hyperspectral reflectance and how to best model GPP using spectral vegetation indices with or without additional modifiers driven by meteorological parameters. I think this is a unique paper as it combines the 'classical' ecological approach of field manipulation with the question of how to improve remote sensing of GPP. The key point here is that by this experimental design the authors are able to produce GPP and spectral vegetation indices which are scale-consistent, in contrast to other attempts of this kind where coarse-scale satellite remote sensing is combined with eddy covariance flux estimates from time-varying flux footprints. The structure of the paper is OK and it is generally well written, although at times the style could be improved (it is however always clear what the authors intend to say). Methods appear sound and the graphical presentation is flawless. According to my opinion, the paper can thus be accepted after minor revisions.

<- The authors thank the Referee for this positive assessment. Our replies to specific comments are found below.

Detailed comments: -> (1) p. 11893, l. 3: while I am not a specialist for savanna ecosystems, but would not be 'understory' a suitable and more accessible term for what the authors refer to as 'herbaceous stratum'; if so, please replace throughout the paper

<- The authors agree that "understory" is a suite term for savanna ecosystems, however, in this case it can be confusing due to 1) the experiment was restricted to an open grassland area (out of the tree influence, and 2) "understory" is a general term that may include other plant forms (i.e. shrubs), which are absent in this experiment. For these reasons, we would rather prefer to keep the use of "herbaceous stratum".

->(2) p. 11894: l. 14-16: in my view LUE models operate solely on the assumption that LUE<sub>max</sub> is correct for the respective application; for example, you would not use the LUE<sub>max</sub> of a tropical forest for a desert ecosystem; neither should one use the same

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LUEmax for the same ecosystem if nutrient availability, which is known to affect LUE, is different

<- We fully agree with the Referee's comment and we have pointed out this in the paragraph. "ii) potential LUE (or maximum, LUE<sub>m</sub>), normally taken from look-up tables and associated with plant functional types"

-> (3) p. 11896, l. 12: another suitable reference would be Porcar-Castell et al. (2015) from the EuroSpec SI.

<- This reference has been included as suggested.

-> (4) p. 11896, l. 23: I am a strong believer in hypothesis-driven research; given the 'classical' ecological experimental design, this paper lends itself to formulate a few hypothesis, which would further strengthen the paper.

<- We thank the Referee for the suggestion and we will consider this and explicitly argue as working hypothesis that traditional LUE models driven by meteorological and phenological data (MM) entail a limited assessment of the environmental controls on GPP. More particularly, we test if the effects of varying nutrient availability on GPP estimates as tracked by chlorophyll fluorescence and PRI can be equally explained by meteorology-driven models. In such, we designed a factorial experiment with different N and P fertilization treatments in a Mediterranean savanna grassland to evaluate the effect of different nutrient supplies on photosynthesis (GPP) and optical properties to investigate the following: a) Nutrient controls on photosynthetic activity are expressed through changes in plant optical properties. b) Physiological changes under varying nutrient treatments impact chlorophyll fluorescence and PRI, which one correlate with photosynthesis? Is there an interaction effect in this relationship by treatments? c) Is MTCI a good descriptor of plant N content and photosynthesis? d) In addition to structural vegetation indices (MTCI, NDVI), PRI and Fy760 provide information that over-shadows the contribution of meteorological variability in explaining changes in GPP.

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-> (5) p. 11897, l. 19-24: the abbreviations for the treatments are not used consistently throughout the paper, e.g. sometimes +N or only N is used; make sure that the same abbreviations are used throughout the text, tables and figures.

<- In the manuscript the following convention was adopted: "+N" refers to Nitrogen addition treatment (see L20), while only "N" is used to refer to Nitrogen (N) content in plants. Like N, similar distinction for both "+P" and "P" abbreviations were taken. Following this convention, we have been careful revised that in the manuscript consistent with the Reviewer's concerns.

-> (6) p. 11900, l. 9: does 3min apply to the combined NEE and RECO measurement or individually to both (i.e. a total of 6min for NEE & RECO)? If so, I suppose that the temperature for the RECO measurement will be higher compared to the NEE measurement, which will bias estimated GPP. Is this an issue and can the authors quantify the effect? In this section it may also be worth stating that apparently a quadratic fit was applied to the dry mole fractions and the flux inferred from the first derivative at t=0 (even though this is detailed in Perez-Priego et al. 2015, this is fundamental information required here).

<- We thank the Referee for these questions and comments, which has forced us to realize that a more detailed description of the chamber method and flux calculation approaches are required and some sentences has been included. As it has been explained in the text, "The chamber was open and ventilated during 1 min prior to measurement, so that initial air composition and temperature in the confined environment of the chamber represented natural atmospheric conditions (as much NEE as Reco)". Considering ~4 min of delay between NEE and Reco measurements, comparable environments were shared for both measurements and hence no biases in GPP by temperature are expected.

Regarding, flux calculations we have added the following paragraph in the methods section (L265-272): "Shortly, the flux calculation algorithm reduced flux uncertainties

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by including the change-point detection method to determine the stabilization time, which defines the initial slope of the regressions, and a bootstrap resampling-based method to improve confidence in regression parameters and to optimize the number of data points used for flux calculation (Perez-Priego et al., 2015). In addition, a statistical analysis of residuals was performed to automatically detect the best fit among alternative regressions (i.e. quadratic, hyperbolic tangent saturating function, exponential, linear).”

-> (7) p. 11902, l. 21: if I understood the methods section correctly, gas exchange and hyperspectral measurements were done sequentially, but not simultaneously (even if the time difference may be small).

<- We agree, and have changed this accordingly. This now reads: “We evaluated direct relationships between midday GPP values (measurements taken around noon with the chamber) and sequentially measurements of Fy760. . . .”

-> (8) p. 11907, l. 20: I think with two months of data the authors should not attempt to assess any long-term effects (years to decades); probably the term 'season should be used here.

<- We agree that “long-term” is a poor choice of word; this is now referred to “season”.

-> (9) Fig. 1: the abbreviation SMANIE appears for the first time here and has not been explained before.

<- We agree and so the abbreviation has been explained in the very beginning of the experimental site and description section (L176-177 “A Small scale MANipulation Experiment (SMANIE) was set up in a Mediterranean savannah. . . .”

-> (10) Fig. 2: is it possible to re-scale the figs and move the title of sub-panel (b) into the panel for consistency with the other sub-panels?

<- We thank the suggestion but data visualization becomes worse when re-scaling the figures. For this reason we would prefer to keep the figure as it is.

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