

Interactive comment on “Decomposing reflectance spectra to track gross primary production in a subalpine evergreen forest” by Rui Cheng et al.

Anonymous Referee #1

Received and published: 26 March 2020

General comments: The manuscript "Decomposing reflectance spectra to track gross primary production in a subalpine evergreen forest" aims to investigate the link between seasonal changes in the canopy reflectance (400–900 nm) of a boreal forest and the GPP changes, measured from flux tower measurements. To do so, the authors apply a technique for decomposing the reflectance into independent components (ICA) and derive a PLSR-based factor for explaining the link with the parameter "LUEs/GPPmax".

Although the manuscript contains several interesting elements, a clear hypothesis is missing (including novel research questions) and several definitions and underlying mechanisms should be better explained. For example, the authors are interested in

[Printer-friendly version](#)

[Discussion paper](#)



the red-edge region where the chlorophylls absorb but don't present a clear strategy for detecting chlorophyll pigment changes (although they are later retrieved by inversion). It is well known that the Car/Chl ratio is the main driver of photosynthetic behaviour on a seasonal scale (L59-61), i.e. altering the ratio between energy dissipation and energy harvesting. Hence, on a seasonal scale the spectral variability would be expected to occur in the pigment absorption regions of those pigments. The authors should highlight which information can be potentially provided by their technique and how it improves (?) the tracking of GPP compared to the standardly used methods (e.g. VIs).

Further, the authors aim to evaluate the pigment driven spectral changes (where, when and why). In this regard the authors could further highlight the seasonal dynamics of the detected components in respect to the spring recovery in boreal forests. Does it provide more info compared to the VI dynamics?

Finally, there are several jumps in the storyline, use of unclear terminology/method descriptions (L141-143, L190-194) and missing parameters definitions (L187). The presentation of the results is sometimes fragmented (L183-185) or not clear from the graphs (L217-L218, GPPmax is not shown). All these aspects need to be thoroughly reviewed before acceptance of the manuscript.

Specific comments From L43-48 it could be misunderstood that LUE of deciduous forests is not affected by biotic factors, while LUE changes due to e.g. pigment composition occur in combination with structural changes, which in fact you can also term a "biotic" factor. The term "biotic" refers to higher-level ecosystem interactions and is less appropriate in the LUE-photosynthesis terminology here. Please rephrase. What is the link with the "differentiation in NPQ pathways" and SIF, which are suddenly mentioned at L75. Is this relevant for seasonal patterns/this manuscript? L77: you are comparing fluorescence radiance with reflectance, which varies strongly in the 400-900 region and is moreover a ratio, not a radiance to compare SIF with. L215-216: why would low PAR not drive photosynthesis? Please reformulate this sentence, pointing to the controlling factors in winter/spring. L78-81: The mechanisms are not clearly explained

[Printer-friendly version](#)[Discussion paper](#)

here. What about the seasonal radiance budget, i.e. the "abiotic" factors?

Methodology After filtering the data based on light conditions and snow, how many winter days actually remain? Please mention the amount of samples, for both winter and growing season, also in Fig. 1. Relative SIF: please elaborate on how the normalization is done (raw data, wavelength range). Since you argued in the introduction that the structural changes are less an issue for coniferous forest, what is the true (or expected) impact of this normalization for SIF? What is the difference with not normalizing? Did you quantify this? LUE_{light}/LUE_{total} : these are supposedly daily values? How APAR was defined/calculated based on the raw data and show a plot of the methodology described in L160. Moreover these parameters are not clearly presented later on and Fig.2 does not give a sufficient visual on the calculation/importance of these parameters. Are they relevant for the story? L155: It is claimed that PAR levels between 1000 and 1500 mol m⁻² s⁻¹ are reached throughout the whole year, but that is not what is seen from Fig. 3, showing PAR values hardly exceeding 1000 mol m⁻² s⁻¹. LUEs: this parameter suddenly appears at L187, without any previous definition! Also, what does the reader need to understand from the LUEs/GPPmax parameter? Please, elaborate the choice of this parameter and how it should be interpreted in terms of vegetation dynamics. Pigment contents: is there a reason why Chlorophyll content is lacking? This does not follow the line of the objectives. L190: rephrase this sentence for a better understanding of the final aim. The resulting coefficient is given somewhere or expected later in the results? Which four PLSR components are you referring to? L203: the raw input reflectance data is unclear here. Also, please further highlight which pigments you are inverting from the reflectance and why.

Results Section 3.1: this whole section refers to results about GPP max without clearly referring to results on this parameter. Please refer better to the results shown in Fig. 3 and check why LUE_{light} and LUE_{total} are not shown in the graph (but mentioned in the legend). L226: please refer first to the observations in the figure in the main text, and for further details refer in addition to the supplementary figures. Fig. 4: "Annual

BGD

Interactive
comment

Printer-friendly version

Discussion paper



mean reflectance”: correct this as the “Annual mean log scaled R” Section 3.2: The link between the seasonality of the spectral components and GPP max seems interesting, but there is a clear difference in the onset of the components 1 and 2 (the more dynamic ones) which might be in addition highlighted and of scientific interest. Section 3.3: The explanation of the methodology in this section needs to be improved. Please be more concrete in terminology (L278: transition period, noise) and what exactly you are referring to. L279-L281: What do you mean with that the high-frequency variations are not captured by any method? The PRI captures the variation of the most dominant feature in the PLSR coefficients. So why would these variations not be related to pigment content? Section 3.4: L302: Please refer to the graphs.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-41>, 2020.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

