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

Anonymous Referee #2

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This paper presents an exploratory study of a very nice data set studying 400-900 nm reflectance, fluorescence, and GPP along with other ancillary measurements including carotenoid composition at leaf scale at the Niwot Ridge, Colorado, USA site in a sub-alpine evergreen forest. As I don't believe this complement of data has been presented at such a site, the data analysis is appropriate for publication in Biogeoscience. However, there are several details that must be addressed before the paper is acceptable for publication. As the first reviewer provided a number of suggestions, this reviewer will try to present a few points not already covered there. The paper contained many small errors that, while minor, led to making the paper a difficult read. Hopefully this will be fixed in a revision. Otherwise I found the paper to be very informative and interesting.

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Discussion paper

General comments:

I have two main issues with the paper. The first is that SIF contains a component of PAR while reflectance does not. This makes it a bit unfair to compare any of the reflectance-based quantities directly with GPPmax (similarly affected by PAR as is SIF) and also to derive the PLSR reconstruction of GPPmax without consideration of PAR. It may be fine to do the reconstruction of other quantities (related to pigments) without account of PAR, but generally not GPPmax. A much better result may come from normalizing GPPmax with respect to PAR (or daily averaged PAR or daily averaged potential PAR) and performing the reconstruction on this quantity. This is particularly important in Fig. 9. Some statements may need to be modified after taking into account PAR (e.g., paragraph starting on L. 320).

The second comment relates to the terminology around the component analysis. It would be helpful if the reflectance can be written in the form of equations explaining the decomposition. Then it may be more clear to express what is being plotted. In my understanding of the terminology, a coefficient should be a number multiplied by a particular spectral component to reconstruct a given spectrum. The term temporal component is confusing to me as this is not what has been decomposed, but rather it is the coefficient of a given spectral component to reconstruct a spectrum observed at a particular time if I have understood correctly. The labeling of Fig. 6 is particularly difficult to understand since panel (a) shows much more than PLSR coefficients. The line labeled GPP_max would be more clear if it had PLSR coefficient in the label.

Specific comments:

L. 118. VI's are normalized such that at least some of the solar geometry effects are removed. This may not be the case for reflectance in general.

L. 142. Normalized at which wavelength(s) exactly? I'm not sure I agree that normalizing by reflected radiation is going to properly "account for the complexity of signal due to canopy structure" at this particular site. Please provide more justification of this

statement.

Since it is mentioned on L. 183 that 3 components explain more than 99.99% of variance, can you state how much variance is explained by each of the components shown.

L. 261, it is mentioned that Tair and VPD covary with GPPmax. Please provide some numbers here such as correlations.

I am confused as to what is meant by "short-term" on line 262 as a few lines above it is said to be the smoothest. Some may take short-term to mean daily. In that case, we wouldn't expect it to be smooth.

Have you definitively shown here that the green band captures variations in LUE? Isn't this only inferred?

Fig. 5, something appears incorrect with the r2 value shown in panel (f).

L. 319, What exactly is meant by diurnal? The most common use of this word in my field pertains to "of or during the day" which is commonly taken to mean sub-daily.

Same paragraph: In this work, it is not definitively shown that SIF tracks seasonal or diurnal variations better than reflectance. While SIF shows slightly higher r2, the differences were not shown to be statistically significant. It is curious that CCI gives a higher r2 value with respect to GPPmax than the PLSR analysis and the PRI gives the same value as PLSR. Also the sample of points looks different in Figs. 5 and 6.

Fig. 9: There is no goodness of fit metric here relative to measurement uncertainties. To make it more clear the fits should be shown with the observations and the residuals and fit properly evaluated with standard metrics. Otherwise the differences are not convincing.

L. 334, I may have missed something but I didn't see how this feature was shown to be directly related to LUE in the paper. This may be inferred but it wasn't directly shown.

Detailed comments:

There are a number of typos that need to be fixed, for example subscripts (L. 1, L. 226, L. 234, Fig. 3, Fig. 6 panel (b)).

There are a number of statements that need to be clarified or corrected for language (see L. 2, for example, "Estimating . . . is a primary uncertainty" would be better phrased as "Estimation of...corresponds to a primary source of uncertainty" or similar). See also lines 11-12 (unclear sentence, L. 14, etc.).

Line 21: Satellites do not measure GPP, rather GPP can be inferred and usually make use of other data.

It's a little confusing in L. 159 to start with "To implement Eq. (1), then define a particular case for Eq. 1. Please rephrase.

L. 164-166. It's not clear at this point what the meteorological data are included for. L. 165 would be more clear to say that daily mean . . . were computed from . . ..

The first reviewer was unclear about what LUEs/GPPmax is. I think I figured it out but it took a lot of time and was very unclear.

Line 217. Is LUElight the same as LUElightL defined above?

L. 226: Confusion regarding Fig. 3 (not S3) but also most of which is repeated in Fig. S2 but with the lines that are referred to in Fig. 3. Suggest to include only one figure with all the lines (in the main manuscript). A similar thing happens with Fig. 9 and D1. Suggest to include only one of these.

Fig. 3: Specify that these are daily-averaged quantities?

Fig. 4 caption: Only the 2nd component shows the carotenoid Jacobian.

Sect. 3.2, first par. There is a lot of information in this paragraph. It might be more effective if it was split up.

L. 268, the word "thus" here is confusing.

Fig. 6: Panel (a) labeling is very confusing. First, the title of panel (a) as well as the label on right side is confusing as these are not really coefficients are they, they are either components of combinations of components (caption is also unclear)? The caption states that the overlaid solid line is the 2nd ICA component, but there are two solid lines. The blue line in the legend is labeled as GPPmax, but it isn't really GPPmax as labeled in the bottom. Would suggest to just remove the titles of both panels.

L. 297 should be "support".

It would be better to subscript the small letters in Cchl, Ccar, and Cant.

Fig. B1 caption should say theoretical maximum (or clear sky)

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