Biogeosciences Discuss., 7, C5159–C5164, 2011
www.biogeosciences-discuss.net/7/C5159/2011/

Biogeosciences Discussions

Interactive comment on "First observations of global and seasonal terrestrial chlorophyll fluorescence from space" *by* J. Joiner et al.

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Received and published: 25 February 2011

The authors thank referee #1 for constructive comments that have helped to improve the manuscript. We added acknowledgment of these helpful comments in the revised version. We include here comments from the review that require a response.

"The main criticisms I have belongs to a) the simplistic radiative transfer formulation (Eq. 1) and b) the limited discussion of the retrieval accuracy.

"a) Equation 1 formulates the top-of-canopy radiance. The investigated signal, however, was measured with a space-borne sensor. Even though the atmospheric impact in terms of absorption is minor and the spectral dependency of scattering is constant for the investigated spectral range, both terms should be included in this equation (and

the following ones)."

We agree with this statement. We now start with a more basic description of the observed intensity and show the impact of various assumptions in subsequent equations. Please see the supplement for more detail.

"b) Remotely measured Fs is a complex signal and reflects i) the physiological response of the plant, ii) the amount of green biomass in the sensors field of view (FOV), iii) the amount of absorbed photosynthetic active radiation (APAR), and iv) effects related to the retrieval and the measurement itself. The calculation of FSyield (it's your "scaled Fs") compensates for variations of APAR (for the "scaled Fs", there must be a stronger link between EVI and Fs due to the remaining structural relationship). Remaining variations of FSyield can be attributed to i, ii and iv. Assuming that the EVI reflects the structural component (or the potential photosynthesis) (ii), then, I agree with your statement (page 8296, line 10-19) that a 1:1 relationship shouldn't be necessarily exist. So, the obvious scattering in your graphs (Fig. 13-15) can be partly! attributed to the existing difference between potential and actual photosynthesis.

"But, the observed scattering is also caused by uncertainties of the retrieval itself!! The evaluation of the methods reliability and the retrieval precision requires a proper discussion of this aspect. Such a discussion might be important as well to continue the development of this promising approach!"

We completely agree with the referee on this point and have included more discussion in the revised version. Firstly, we have now included calculated retrieval precisions. Secondly, we give a more detailed discussion on the potential contributors to the scatter as you outlined above. Please see the supplement for more detail. In addition, we have removed the comparison between EVI and the fluorescence efficiency (F scaled by both PAR and FPAR) as scaled-F is the more appropriate quantity for comparison as you point out above.

"The calculation of the FSyield (your "scaled fluorescence") seems difficult. Can you

suggest some strategies to provide more reasonable fAPAR values in the near future?"

We feel that it is beyond the scope of the paper to discuss strategies of providing an improved fAPAR product, though of course we would like to see improvements made. We hope that the developers of this product will develop such strategies. Also note that we made some errors in geolocation of the fAPAR product in the discussion paper. These have been fixed in the revised version.

"Please discuss strategies for a validation of the results (e.g. a comparison of the MODIS GPP product and Fs (not FSyield!!) or the evaluation of seasonal signals using tall eddy-covariance towers)."

First, one might ask how do you validate NDVI or EVI on such coarse scales, except to develop time series that appear reasonable? That is really the only strategy we can offer to validate the F signal. Replication over multiple years is needed on key study areas such as those used here, although other ones might be more relevant for longer term evaluations. Flux tower sites typically represent areas <1 km, so may present some difficulties in validating 10km2 or larger averaged regions. They may be useful for gross quality assurance, but true validation is a stretch. Clearly, developing a validation plan for Fs measured from satellite should be a goal of the new ESA FLEX mission that we could adopt. Meanwhile, specific sites observed repetitively, and compared with some in situ measurements, would be useful. The MODIS GPP product is partly derived from satellite data and partly derived from a model and therefore should not really be considered as a means of validating F. We have added a brief discussion of some of these aspects in the conclusions section.

page 8284, line 3-13: Please include a short argumentation related to approaches linking space-borne PRI observations and LUE (e.g. Drolet, RSE 98, pp 212-224 or Hilker, RSE, 114, pp 2863-2874)

We added a brief mention of the Drolet work in the conclusions.

- Page 8284 – line 16: Please, rephrase "reflectance effect" to e.g. "reflectance signal"

Change made.

- page 8285, first sentences: Please, rephrase to "In air- and space-borne applications, one must additionally account for atmospheric absorption and scattering..."

Change made.

- page 8285, line 4: Measured radiance signals of non-fluorescent surfaces of the same image can be used and to separate Fs from R and to account for path scattered radiance. This approach does not allow the correction of FS re-absorption due to atmospheric transmissivity. You should not "say atmospheric correction" in this context. Please rephrase to "a measurement of a non-fluorescensing surface can be used to separate both the emitted and reflected radiation fluxes"

Change made.

- page 8287, line 24: You could refer Guanter et al. 2010. They investigated the dependency of the O2 line depth and surface pressure.

Change made.

- page 8288, line 26: You already introduced F as abbreviation for fluorescence, please remove either the abbreviation of the written word.

Change made (we removed the words).

- page 8289, line 11: Please, rephrase to "We could also account for atmospheric scattering..."

Change made

- page 8289, line 11-12: Please indicate the used radiative transfer model and how the model was used to account for atmospheric interaction.

In these lines, we state that "we could account for scattering and absorption using a

radiative transfer model." However, we have not used a radiative transfer model here. We have tried to make this more clear in our revised version that goes into more detail about the simplifying assumptions we made in our modeling of the radiance. We also added "though we have not done so here" to the above-mentioned sentence.

- page 8289, line 11-12: You state that the spectral dependence of atmospheric scattering is constant over the investigated spectral range. But, scattering leads to a proportionally different filling-in of the line and, consequently, to an offset in the retrieved Fs signal. Could you provide a short statement about the impact of atmospheric scattering on the in-filling? For my understanding, this should have implications on the spatio-temporal variability of Fs as the aerosol load may change significantly over time and between different parts of the Earth.

To be clear, atmospheric Rayleigh scattering does not fill in the line. Atmospheric rotational-Raman scattering (RRS) does cause filling-in of solar and telluric lines. We had provided a reference to Sioris et al., who stated that the effect of rotational-Raman scattering at these wavelengths is negligible. However, we have since done our own calculations at various solar zenith angles and provide results in a revised version (see supplement). We have removed the paragraph with the Sioris reference as it is not a correct statement that RRS filling-in is negligible. In the simulation section, we now show the maximum filling-in due to rotational Raman scattering as a function of surface albedo (for 70 degrees solar zenith angle) and provide additional discussion on this point. We thank the reviewer for bringing up this point.

- page 8292, line 10, 11: You introduced F as abbreviation for fluorescence, please replace Ff with F.

We use Ff here to differentiate it with F. Ff is the fluorescence flux. Assuming the fluorescence is isotropic, they will differ by pi and of course the units are different. We added some text to clarify this.

- page 8293, line 10: Please, rephrase to "a widely used reflectance-based vegetation

index"

Change made

- page 8297, line 1: Please correct "Fluorescence"

Correction made

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C5159/2011/bgd-7-C5159-2011-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 7, 8281, 2010.