Atmos. Meas. Tech. Discuss., 5, C969–C971, 2012
www.atmos-meas-tech-discuss.net/5/C969/2012/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Chlorophyll fluorescence remote sensing from space in scattering atmospheres: implications for its retrieval and interferences with atmospheric CO_2 retrievals" *by* C. Frankenberg et al.

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Received and published: 16 May 2012

I am very impressed with the recent development in remote sensing of passive fluorescence and successful retrieval of global fluorescence using GOSAT data as reported in this paper. Unfortunately, the interpretation of the resulting far-red fluorescence signal (c. F760), in terms of what might or might not tell about the seasonal physiology of photosynthesis, does not match the advances of the remote sensing community. From an ecophysiologist point-of-view I feel that the field should advance simultaneously in all fronts if a solid link between the dynamics of photosynthesis and those of satellite

retrieved fluorescence is to be found. In this respect, the interaction between the seasonal physiology of photosynthesis and the F760 signal are pretty much at an infant state. Leaf fluorescence emission spectra in vivo have been widely characterized under controlled conditions (but see my clarification below), yet it remains still unknown to what extent the SEASONAL changes in F760 can be attributed to adjustments in light use efficiency (the positive scenario), and how much of it responds to changes in greenness or PSI absorption cross section (the not so positive scenario). And let's remember that greenness indexes (especially if they do not saturate) may yield very good correlations with GPP under certain conditions, therefore, even if F760 was a proxy of chlorophyll content I would still expect it to correlate relatively well with GPP.

This being said, I would like to comment on the expected share of PSI fluorescence to total fluorescence in the far-red side of the emission spectra. I will use the same citation previously referred in this discussion for argumentation, i.e. Buschmann (2007) Photosynthesis Research 92:261-271 (but see also the seminal work by Pfündel 1998 in Photosynthesis Research 1998, 56:185-195):

["The proportion of PS I-fluorescence in the long wavelength fluorescence depends on the stage of the light-induced photosynthetic induction: it has been estimated to reach about 50% at the ground Chl fluorescence F0 at the onset of illumination of a dark-adapted leaf and about 10% at maximum Chl fluorescence FM after the application of saturating light (Peterson et al. 2001; Pfündel 1998)."]

There are a few critical things to be highlighted from this paragraph:

1) Those experiments were conducted in dark-adapted leaves. This means that those leaves did not present regulative thermal energy dissipation or NPQ, a process that substantially decreases PSII fluorescence but is thought to have little effect on PSI fluorescence. Therefore, in the presence of NPQ the relative share of PSI fluorescence is expected to increase even further.

2) The value that should be used as "reference" is not that during the Fm level (when

PSI contribution is 10%) but that of Fo (when PSI can be higher than 50%). To obtain Fm all PSII reaction centres are closed with a saturating light pulse and photochemical quenching tends to zero, therefore PSII fluorescence is maximal, and the ratio of PSII/PSI increases above normal levels, however this situation is never met in nature. The Fs is much closer to the Fo than to the Fm, thus if at all, the 50% may be a better estimate.

3) Satellite observations are obtained under illumination and therefore not from dark-acclimated samples. This means that fluorescence retrievals are acquired in the presence of NPQ which may dramatically decrease fluorescence levels even below the Fo levels of non-stressed plant material (as the one used in the previous studies). In down-regulated vegetation such as that exposed to drought or cold stress, Fs may decrease manyfold relative to the reference non-stressed Fo level (see e.g. Fig 2c on the annual dynamics of F, Porcar-Castell 2011, Physiologia Plantarum 143:139-153, or Fig.1 in Porcar-Castell et al 2008, Tree Physiology 28:1475-1482). Under these conditions, and assuming that NPQ operates only in PSII, the contribution of PSI fluorescence to the F760 signal could be significantly higher than 50%, maybe close to 100%?

These are just some open questions that require still leaf-level conclusive research, yet it is clear that the contribution of PSI on the F760 is all but insignificant. I think until we are not able to answer these questions we should avoid publishing statements that, based on the assumption that passive F emanates exclusively from PSII, may give the impression that F760 is related to the adjustments in photosynthetic light use efficiency, as it might turn out that what we are capturing with F760 is simply greenness.

These aspects should at least be introduced in articles that treat the link between passive F and photosynthesis, such as this one.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 2487, 2012.