

## General comments

The paper stems from the activities of a large European project and has the objective of assessing the utility of remotely sensed LAI and leaf chlorophyll and nitrogen (N) estimation methods using data collected in 5 sites, mostly located in Northern Europe (with the exception of an Italian site). A second objective was that of assessing the distribution and size of vegetation N pools in the 5 sites.

My overall impression is that the data and the methods employed did not allow to answer satisfactorily to such rather different objectives. For the first objective, not particularly original since dozens of papers have been written on this topic, a more thorough sampling and an independent validation should have been performed. More emphasis should have been given to the intrinsic difference between empirical (SVIs) and more general (and innovative) physically-based model inversion methods such as REGFLEC. Moreover more attention should have been given to the uncertainties and errors inherent in "ground truth" data which were essentially indirect estimates of the target variables. The results are perhaps presented too optimistically, since the best of the different approaches were combined excluding (admittedly) data with vertical gradient profiles, which are perhaps more common than it was found in this paper.

For the second objective, in my opinion the data did not cover an adequate spatial and temporal extent (being just a snapshot) to allow a substantiate explanation of the differences among the landscapes in terms of vegetation N pools. In this respect it should be paid more attention to the fact that the comparison between sites is much less straightforward than it would have been with natural vegetation, since crops with different growing cycles are present in the sites.

## Specific & minor points (reference is made to page P and line L numbers):

Title: actually canopy and not leaf Chl and N were predicted, thus probably the title should be changed accordingly.

P10151 L6: "...data and are not verified...."

P10151 L26: here and later on in the Discussion, I would not use the expression "further improved", since in one case single land use categories (i.e. crops!) were used and in the other single sites but different "land use categories", a rather different way of looking at results.

P10152 L15-19: I don't grasp the logic of this sentence beginning with "Despite"...

P10153 L11: I would call them variables rather than products.

P10157 L4- P10158 L20: I don't understand why the names of the sites and their geographic coordinates and altitudes are omitted.

P10159 L21-22: Here is in my opinion one of the major weaknesses of the paper: inadequate sampling. The authors should better explain how the 3x3 m areas were placed within the 10x10 m areas and justify the representativeness of these areas in terms of SPOT satellite pixel sizes and georeferencing error (typically one pixel or more...).

P10159 L24 - P10160 L6: the authors should acknowledge that LAI-2000 data are indirect estimates of LAI, representing more properly "effective" (i.e. not accounting for clumping) PAI (plant area index), i.e. not differentiating between leaves and other plant organs. Here the sampling scheme is reported to follow transects: how do they relate to the 3x3 areas mentioned before? One or two transects with 4 measurement points in a 3x3 area? Are you sure that the number of points was enough? What could be the error and uncertainty in the estimated "ground truth" LAI value?

P10160 L28: something wrong here since in the paper by Porra et al. (1989) equations for other solvents (methanol, NN-DMF) but not ethanol are reported.

P10161 L9: it would be useful to report the equation best fitting all data (e.g. to allow comparison with other calibrations reported in the literature).

P10161 L13: Houborg and Boegh 2008 is not in the references.

P10161 L23: you should report exactly how you computed canopy chlorophyll and N. What could be the uncertainty of these estimated "ground truth" variables?

P10164 L24-18: the description of the REGFLEC algorithm is not clear. I understand that it should be synthetic, but I had to read the references to figure out how it works!

P10165 L23 and further on: the comparison between vegetation status among sites would have made more sense for natural vegetation, or in case you had the same crop in all sites, but here you have crops with different sowing and harvest dates...

P10169 L3-L11: actually in many cases the vertical chlorophyll concentration profile has a bell-shaped form (e.g. Winterhalter et al., 2012; Ciganda et al., 2012, Remote Sens. Environ. 126:240-247) that could possibly result in a non significant linear regression. So the criteria used to differentiate between poor and strong vertical structure seem too simplistic to me, leading perhaps to an underestimation of the canopies having "strong" vertical profile (only 20%).

P10169 L25-L27: probably the major weakness of this paper (or at least one of the least clear points) is in the "validation" of the estimates of LAI (and other variables). It seems as if validation was not independent from calibration: i.e. did you develop statistical models for SVIs and then used them on an independent data set or not? In the latter case these are not actual LAI "predictions" to compare with "measured" LAI.

P10170 L11: a citation is needed here.

P10172 L10: actually in Table 5 GNDVI for CHLc seems significant for PL.

P10172 L25: table 3 rather than Fig.2.

P10175 L24: how could the selection of only those canopies without vertical gradient profiles could affect the generalization of the estimation methods proposed to the regional scale, e.g. from an operative point of view?

P1017 L19 : Houborg et al 2007 and Houborg and Boegh 2008 not in the references.