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Biogeosciences Discussions

Interactive comment on "Remote sensing of LAI, chlorophyll and leaf nitrogen pools of crop- and grasslands in five European landscapes" *by* E. Boegh et al.

Anonymous Referee #2

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This paper sets out "to assess the utility of different remote sensing-based methods for regional mapping of CHLl, Nl and LAI in crop- and grasslands". To do so, they use SPOT satellite data to calculate various SVIs, and relate these to field measurements on the ground, made at the same time as the satellite data acquisitions. This is a very worthy aim, and the testing satellite-based estimates of vegetation properties is an area that needs much improving. This paper represents a reasonable attempt, and is useful in looking at a range of sites from across Europe, and collecting ground-based data at all of them. However, I feel there are various weaknesses in their approach and data analysis, and much more could be done. I would recommend some revisions before this is published.

1. The stated aim was to assess the capability of the selected remote sensing methods to quantify LAI, CHLl and Nl over a large range of environmental conditions in Europe. The crux of this is Figure 6, which shows the field measurements against the satellite estimates. However, the authors cherry-pick the methods which work best for each site, and combine these to give a misleading result. This is not a useful validation test. In any real application of the method, we would not know a priori which algorithm to use. Figure 6 needs to be expanded considerably to show the results for all data and each algorithm in a number of panels in the figure. Table 4 (rows for 'All') show the stats on the agreement for this comparison, but it is worth showing it graphically.

2. In both Fig 6 and Tables 4 & 5, only canopies without strong vertical profiles in chlorophyll are included. However, from optimisation theory, an exponential decline in N is expected, so this should be the norm rather than the exception. These points should be included, perhaps displayed with a different symbol in the graphs, and results of analyses shown with/without these points. Perhaps plot deviance between observations and SVI-estimates against the slope dChl/dh.

3. Throughout the results, discussion & conclusions, the authors seem to have reversed the logic of the validation test. They say that by removing canopies with strong vertical gradients and horizontal clumping, or by focussing on a single land use type, they can improve the predictive capability. In any real application, we want a method that is generally applicable, without any a priori knowledge of the canopy/surface type. The key point here is that the predictive capability declines as the range of canopy/surface types increases. This is an interesting and publishable result, which merits analysis that is not presented here, and the authors seem compelled to put a very positive interpretation on the comparison, to an extent that could be misleading.

4. Could the CORINE land cover data be used as an additional data input? The methods could be calibrated for each CORINE land cover type separately, which seems to work better. This is using prior knowledge that is easily available, but perhaps the spatial scales don't match well.

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5. Something more imaginative might be done with the SVIs. Could they by combined in some multivariate way (PCA, EOFs, neural nets, etc.) to get the best from each algorithm. This could be applied to the raw data or or the SVIs themselves.

6. It is interesting that REGFLEC seems to perform the best when the data are pooled, but not when applied to a single land cover type. This is presumably because the algorithm can account for some of the complexities that different canopy structures and back ground surface introduce. Given that this algortihm has many extra parameters, it should of course perform better, but analysing the circumstances where it does might merit some more discussion.

7. Some more thought might be given to evaluating the performance of the SVIs. Presenting Tables 4 & 5 graphically might help, as scatterplots or Taylor diagrams (see DOI: 10.1029/2000JD900719). What relative importance should be given to r2, RMSE, ME or other measures, in this context? It needs to be made absolutely clear whether any calibration was done on any of the SVIs or REGFLEC model - are all the comparisons tests on independent data, so true predictions?

8. The authors mention the mismatch in spatial scales common when comparing satellite data with ground-based observations. The problem is less here because they have 10 or 20 m resiolution data, but still exists. At the UK site, they are comparing field measurements in a 0.25 m2 area with a 400 m2 pixel, with no obvious strategy to bridge that gap. The power analysis should demonstrate that they have enough field samples, such that the uncertainty around the mean for the 100 or 400 m2 pixel is less than the precision on the satellite-derived estimate. Can they show this? The power analysis as it is applied seems to be used for outlier detection, by comparing with "error margins", but this seems unfathomable to me.

9. Can the location of the field measurements be shown explicitly in Fig 1? I get no sense of how large or widely distributed the field plots are in relation to the satellite pixels.

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10. Precisely what is meant by the "soil line" needs explaining clearly, or removing. This journal has a general readership, and remote sensing jargon should not be used. Generally, the language needs tightening up in places, e.g. "Predictability" is used to mean "predictive capability".

11. p 10169: "s" is not defined I think. Slope of dChl/dh I think.

12. The "statistical significance" of relationships is repeatedly quoted, with p values. This implies a test of the null hypothesis "no relationship between SVIs and the surface properties", which seems irrelevant to me. I'd suggest removing all references to null hypothesis testing.

13. Table 3 caption is very confusingly worded. I think they just mean they calculate the variance at different strata, in ANOVA terminology.

14. Fig 4 - the z-axis needs to be explained clearly. I think this is a histogram in two dimensions, with colour scale showing the number of pixels. Axes need units.

15. Fig 6 might have error bars in the x- (and y?) dimension.

16. Fig 8. Why not show the distribution of the field observations as well, rather than just a single point?

17. To be clear, conversions between leaf-scale and canopy-scale nitrogen and chlorophyll contents should be made explicit. Is it simply Nc = Nl x LAI, or are different canopy strata integrated separately?

Interactive comment on Biogeosciences Discuss., 9, 10149, 2012.