Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-228-RC1, 2017

Biogeosciences
Discussions
Open Access

Interactive comment on "Regional detection of canopy nitrogen in Mediterranean forests using the spaceborne MERIS Terrestrial Chlorophyll Index" *by* Yasmina Loozen et al.

Anonymous Referee #1

Received and published: 5 September 2017

The manuscript shows an interesting study on the use of MERIS data to analyze empirical relationships between MTCI and ground measurements of forest canopy N content and concentration. Foliar N influences a variety of important ecosystem processes so it is clear the interest of exploring the capacity for remote detection of canopy N at regional scales from space-based platforms and the potential of new generation of sensors such as those included in the Copernicus program. However, direct estimation of N in fresh vegetation using remote sensing data is challenging due to its weak effect on leaf reflectance so the influence of structural properties of the canopy and other potential confounding factors related with the input data are key issues to be explored.

The paper is well written and also well-structured and the research questions addressed are relevant and clearly fall within the scope of Biogeosciences. However, my main concern about this work is that, at some point, the paper could be read as a search for correlations without a thoughtful discussion on the different confounding factors that could potentially affect to the observed relationship between satellite and ground data and how these factors could impact the results. A key element in this study is related with the intrinsic limitations of the input data: spatial and temporal mismatch but also, for example, the method used to scale from leaf to canopy N using field sampling strategies. In this work allometric equations are used to relate the diameter of the branches to the leaves dry weight in order to estimate canopy N content. It would be interesting to discuss the accuracy of this method compared to others proposed in the literature to estimate canopy foliar mass per species at the stand level. It would be also interesting to know what is the inter-annual variation of N (ground measurements) in the study region in order to evaluate how this can affect to the discrepancy between timing of ground and satellite data.

Another important issue in this work is the lack of assessment of robustness of empirical models applied using either independent data or statistical techniques (bootstrap). This may be critical when the relationships found could depend on the covariance with other variables as is typically the case in the canopy N estimation from remote sensing. Finally, I also miss in the discussion how the authors consider the results could be potentially useful for monitoring canopy N at regional scale considering the strength of the relationships found and the estimation errors (not analyzed in the paper). Specific comments addressing particular scientific/technical/formal issues follow:

Page 5 line 139. Complementary o alternative reference on methodology applied?

Page 5 line 143. Correct . . ..foliar biomass (N g per square meter. . ..

Page 5 line 153. Reword to clarify content and avoid repetitions

Page 6 line 180. Why the MERIS 300m full resolution product was not used instead?

Page 7 lines 197-199. What about other land cover changes as those caused by forest fires (quite frequent in the study region), where they investigated and filtered?

Page 7 sections 2.3.2 and 2.3.3. Would be interesting to know the number of plots per pixel (average, min and max) at the different spatial resolutions.

Page 8 line 238. Foliar biomass is used in the calculation of canopy N content so the correlation is obviously strongest

Page 9 line 254. Higher instead of lower

Page 9 line 269. R2 for Quercus ilex?

Page 9 section 4.1. Could the authors elaborate here on how this could affect to the regional estimation of canopy N using new generation Sentinel-2 and 3 with improved spatial resolutions?

Page 11 line 315. Any hypothesis on the stronger relationship found for DBF plots? Further investigation on the proportion of the variance explained by other potential confounding factors would be desirable (same in lines 329 and 341)

Page 11 lines 332-335. This has been already stated in the results sections. This apply for other paragraphs in this section, authors should avoid to repeat the results and focus on the discussion

Page 12 lines 152-153. I would recommend to include the analysis in this paper using information acquired in the forest inventory used for the study.

Page 20 FIGURE 1. Please clarify if the plots represented in the map are all the forest inventory plots (2300?) or 1075 (after temporal and spatial filtering) or 846/841 finally used in the analysis. I would recommend including only the plots used in the analysis.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-228, 2017.