Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-228-RC2, 2017

Biogeosciences Discussions

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 #2

Received and published: 20 October 2017

General comments: The paper aims to investigate the potential of using MTCI to map regional variations in canopy nitrogen (N). The study uses field measurements of canopy N for a large number of forest plots situated across Catalonia to derive empirical relationships between N and MTCI data across a range of spatial resolutions (1 - 20 km). The study also aims to identify the influence of plant functional type on the observed relationships. Whilst the premise of the work may be interesting, there are a number of questions and comments, some of which are fairly fundamental, which I feel need addressing before this manuscript can be considered for publication. The comments are provided in the hope that they may help improve the manuscript and its subsequent impact.

Specific comments: I am not entirely convinced of the justification for reducing the spatial resolution of the MTCI data. Why degrade the 1 km product? The MERIS sensor on board ENVISAT is no longer operational (which the author's should note). The authors do note that a variation of the MTCI can be calculated from Sentinel-2 but this is a sensor with a higher spatial resolution then MERIS so what is the justification for making the data worse? Especially since the forest plots were substantially smaller than the original 1 km pixel size in the first instance. Averaging 6 m plots over a 1 km grid would "reduce small-scale variations (line 279)" so why 5, 10, 15 and 20 km also? Without this information the paper appears to be more of an academic exercise as opposed to addressing a tangible issue.

One of the main justifications for the study is that "limited research has been conducted to sense canopy N in Mediterranean ecosystems and even more so in Mediterranean forests", yet there is no discussion of the importance of these ecosystems, or their N content. More information should be included to justify the significance of this sentence.

More information is required on how the forest plot data are deemed suitable for comparison with the MTCI data. There are several questions here:

1. Is the year of data collection an issue for the correlation? Perhaps colouring the points in figure 4 based on year of in situ collection may be useful e.g. were there any climatically anomalous years that could have influenced the MTCI relationships?

2. How well do the 6m forest data plots represent the 1, 5, 15 and 20 km grid scales? There isn't any information as to how many points were included in each grid square when the data were resampled at each resolution. What was the distribution of values (mean, SD)?

3. Can homogenous species plots be observed from satellite imagery at 5 – 20 km resolution? Surely the plots are going to be mixed species at this scale?

The results presented, whilst statistically significant have quite low r2 values, which

indicates that the precision with which N can be predicted will be low, even though there is a statistically significant relationship between the two variables. The authors do not comment on this but I think they should as this has practical implications for their suggested approach.

It would be useful for the authors to suggest possible reasons why the reported statistically significant regressions are only explain 20 - 30% of the variation at best. The authors indicate that these r2 values are somewhat lower than MODIS so why not just use MODIS?

Technical corrections: The first sentence of the abstract is quite long. Consider fragmenting and re-wording to improve impact.

Line 11 and throughout – Data "is" should be changed to data "are" since data are plural

Line 13 etc. – The abstract should include some justification as to why the work is important. This could be more clearly explained in the abstract as opposed to simply saying x did this and we are doing that. The key question is why?

Line 31: Delete "," after processes

Line 35: Insert "," after (N g m-2)

Line 48: Delete "Currently"

Line 49: Insert "," either side of from and sensors

Line 48 – 52: This is a very long sentence. Consider fragmenting.

Line 53: No need for a new paragraph

Line 68: "were aimed" is an odd choice of words. Consider re-wording

Line74: Do the authors mean "a few studies" or "few studies"? It's not clear as no references are referred to.

Line 83: "stated that the NIR – canopy N relationship was not necessarily spurious as plant traits have been known to covary along the leaf economic spectrum" This statement needs further explanation. What is meant by the leaf economic spectrum?

Line 89: "MTCI time series could be applied to estimate canopy N at a larger scale" Be careful with the terms scale here. Do you mean over a larger spatial extent?

Line 106: Suggests that there are 1075 forest plots but line 123 suggest that there are 2300 and in line 2017 there are 846 plots. Were some removed from the sample?

Line 110: What are the re-sampled resolutions and what is the justification for this?

Line 117: duplicate word "create"

Line 150: "Several (up to two times) " does not make sense. Several suggests three or more. Consider re-wording.

Line 200: MTCI was not re-sampled as the product was already a 1 km product

Line 303-204: "This enabled us to investigate the relationships between MTCI and canopy N data independently of differences in initial support size." I don't entirely agree. Just because they now match on a spatial grid does not mean that the difference in sampling support size no longer matters. The crucial point is how well do the 6 m forest data represent the 1 km grid scale? Anything can be re-sampled. Whether it makes sense to do so is a different question.

Section 2.3.3. It seems a bit odd to investigate relationships at a lower resolution before you investigate it at the original spatial resolution.

Line 215: Refer to section numbers as opposed to "explained above"

Lines 219 and 220: delete the word "then"

Line 223: "The spatial analyses were done with the PCRaster software" It is not clear what spatial analyses were "done". Consider re-wording.

Figure3: I am not sure what the purpose of this figure is since some of the variables being correlated are actually included in the calculation of others e.g. biomass and N concentration are both used to calculate N content – they are bound to be correlated. Hence line 238 is not really a finding.

Line 282: I don't understand what this sentence means I'm afraid "This shows that, when the influence of the discrepancy between the original datasets was taken into account, MTCI and canopy N data were linked" what discrepancies were observed?

Line 294 "there is no general agreement about MTCI ability for canopy N[%] detection across vegetation and sensor types" Can the authors bring any insights as to why this may be the case? What are the issues?

Line 315-316 Consider re-wording. Also note that there were only 15 plots of Fagus sylvatica! Can you make such a conclusion based on relatively few samples?

Line 348 "Other authors, although agreeing that canopy structural properties needed to be accounted for, suggested that a direct biochemical link between canopy N and reflectance data was not necessary to detect canopy N with reflectance data (Ollinger et al., 2013;Townsend et al., 2013)." What did the authors suggest was necessary?

Section 4.4 doesn't really come to any conclusions or suggest reasons for the PFT differences and so it is somewhat superfluous as it stands. Better to integrate this in a wider discussion or include some more detailed interpretation of the data.

Lines 359-3622. I do not follow this point here. What treatments were required and what "might reveal laborious" Consider re-wording.

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