I would like to thank you the authors for the meticulous review and response to the comments on the first manuscript version. In this new version authors have addressed most of the issues raised during the interactive revision process including additional information that helps to understand methods and results, as is the case for the temporal analysis of the canopy N field measurements and the improved statistical processes. However I still see some weak points in the paper:

1. In my opinion authors have not jet provided a proper justification on the usefulness of the statistical analysis using resampled MTCI images to lower spatial resolution. In fact, there is a kind of contradiction in the manuscript between this analysis and the information provided by the authors in the introduction and discussion about the future potential of canopy N estimation form RS using new generation of sensor with improved spatial resolution. I would find the analysis useful if the authors wanted to demonstrate that sensors with lower spatial resolution can be potentially used to obtain global estimations of canopy N, but, as this is not the case, I would find more convenient to undertake an analysis that allows to demonstrate the sensitivity of the statistical relationships found to the field data (sample size and distribution). This is an important issue raised by the authors in the discussion (section4-5). Ground canopy N observations are necessary to calibrate and validate models at regional-global scales. In this context, an interesting (and I would say feasible) output of this work could be a sensitivity analysis on the model performance according to field data availability.
2. I still miss in the discussion a more "quantitative" consideration on the potential of the results obtained to feed global vegetation models. Authors argue in their response that their study contributes to the ongoing discussion on canopy N estimations on larger areas using RS but this is, in my opinion, a quite diffuse argument. I would expect a more detailed discussion on how much the estimations should be improved to provide useful input to those models (what is the uncertainty in canopy N that can be considered acceptable for the models? And specifically for Mediterranean environments?)
3. In the discussion authors compare their results (in terms of r2) with other works were similar relationships have been found between canopy N and vegetation indices but they do not mention that other studies do not include the temporal dimension. Temporal variability of vegetation due to phenology should not be ignored when estimates are based in secondary relationships as is the case with N vs vegetation indices and, therefore, studies that including or not this temporal dimension are not fully comparable.

I have also some comments addressing technical/formal issues referred to manuscript version 3:

Abstract line 12. Remote sensing and vegetation indices are not excluding terms, I would recommend rephrasing.

Abstract line 19. I would say "original" instead of "initial higher"

Section 2.2.1 Authors mention that "all foliar cohorts in the canopy were included in the leaf sample" but, was the % of new-old leaves in the crown taken into account during the sampling or the data processing? The N content can greatly differ depending on the leaf age so, in certain phonological periods this need to be considered to obtain an accurate estimation of canopy N.

Section 2.2.2. I think authors should mention here Sentinel-3 OLCI sensor as the most direct inheritor of MERIS ENVISAT.

Section 2.3.1 line 207. It is not clear why you need to resample the landcover map to the MTCI images resolution. If I properly understand you just want to identify and mask the field plots that changed from forest to other non forest covers. If so, you would just mask those field plots located in a landcover map pixel classified as those covers excluded from the analysis.

Line 222. In the title of this section and all through the manuscript I recommend to replace "initial higher" by "original 1Km" spatial resolution.

Line 281. Authors mean here statistically significant?

Line 288. P-value of this relationship?

Lines 407-408. Consider rephrasing to avoid repetition (addition…adding..additional)

Figure 1. I would recommend to add a couple of zoom windows showing the MERIS MTCI 1 km grid on areas with high and low density of field sampling points.