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Interactive comment on "Spatial variation and linkages of soil and vegetation in the Siberian Arctic tundra – coupling field observations with remote sensing data" *by* Juha Mikola et al.

Anonymous Referee #2

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Juha Mikola and colleagues present a study from northern Siberia that focused on (1) spatial variation in plant and soil attributes within a tundra ecosystem, (2) co-variation in these attributes, and (3) the potential to map these attributes using remote sensing. The researchers show that plant and soil attributes (e.g., plant biomass, soil organic matter content) differed among land cover types and that both moss biomass and vascular plant leaf area index (LAI) were weakly to moderately correlated with several soil attributes. Furthermore, they examined whether the plant and soil attributes could be mapped using the normalized difference vegetation index (NDVI) derived from very high spatial resolution satellite imagery that was acquired at different points during the growing seasons. This comparison showed that moss biomass was most closely

related to early summer NDVI, whereas vascular plant LAI was more closely related to mid-summer NDVI, which suggests that multi-temporal imagery may be useful for quantifying different aspects of plant and soil attributes in tundra ecosystems. The researchers conclude that spatial extrapolation of plant and soil attributes may require the use of land cover maps and field sampling within land cover types rather than linking field measurements directly with remote-sensing observations. In general, the study is robust and multi-faceted, and the manuscript is very well written. Overall, the study makes a valuable contribution to arctic ecology and would be well-suited for Biogeosciences, though could benefit from some refinements detailed below.

General comments

(i) I agree with reviewer 1 that the abstract is too long and detailed. The primary findings and implications would be better highlighted if the abstract was condensed.

(ii) One of the primary conclusions from this study is that spatial extrapolation of plant and soil attributes will require using land cover maps and field sampling within different land cover types. This approach contrasts with the direct remote sensing approach that involves (1) developing statistical relationship between field and surface reflectance measurements and then (2) modeling plant/soil attributes across a broader area by applying these statistical relationships to wall-to-wall surface reflectance measurements. The authors' conclusion is based on the observation that NDVI often explained little of the spatial variation in plant and soil attributes across the network of plots. This comparison alone does not seem like an adequate basis from which to draw the conclusion stated above. A direct remote sensing approach does not need to be based solely on NDVI, but rather plant/soil attributes could be predicted using all the spectral bands and plus derived texture metrics and spectral indices. Further analysis might support the authors' current conclusions, but the current conclusion seems premature given the analysis presented.

(iii) The authors estimated the total amount of leaf area, plant biomass, and soil organic mass that occurred within several land cover types found in their study area; however, these numbers do not include estimates of uncertainty. The lack of uncertainty estimates also fits with my comment above (ii). The authors could estimate uncertainty in these totals based on variation in attributes within each land cover type or could perhaps use a Monte Carlo approach in which they account for variation with each land cover type as well as uncertainty in the land cover map.

(iv) The tea bag decomposition measurements don't seem to fit with any of the stated objectives and are not mentioned in the discussion. The manuscript as already has quite a few elements, so I'd suggest dropping those measurements from the manuscript and focusing on the core elements.

(v) It would be good to note in the discussion that the remote sensing observations were not acquired concurrent with field sampling, which introduces uncertainty into the comparisons between NDVI and field measurements. The QuickBird imagery was acquired almost a decade prior to field sampling, whereas the WorldView-2 images were acquired with a year or two of field sampling. These time lags could make it harder to relate field measurements to NDVI, especially for the QuickBird imagery.

Specific comments

(i) P4, L10: Spell out "DD" the first time it is used.

(ii) P4, L37-39: Please clarify whether you harvested live vascular shoot biomass, or live + standing dead vascular shoot biomass. Also, please describe how you defined the bottom of the moss layer.

(iii) P5, L1: Define leaf area index (m2 leaf m-2 ground) and whether LAI was based on projected leaf area, hemi-leaf area, or two-sided leaf area. It seems you used projected leaf area.

(iv) P5, L26-27: I'd encourage the authors to put all of the plot-level measurements in the supplemental material, as well as the current summary for each land cover type.

The current summary table should also probably include the standard deviation of each measurement for each land cover type.

(v) P6, L5: You might add that NDVI has been used not only for "spatial examination of LAI" but also for mapping plant aboveground biomass in tundra ecosystem (e.g., Raynolds et al. 2012, Berner et al. 2018). Raynolds: http://www.tandfonline.com/doi/abs/10.1080/01431161.2011.609188 Berner: http://iopscience.iop.org/article/10.1088/1748-9326/aaaa9a

(vi) P6, L8: Technically, you generated a digital surface model (DSM) rather than a digital elevation model (DEM). A DSM includes the height of vegetation and other features, while a DEM represents bare-year elevation.

(vii) P6, L10: Define "ground control point" acronym (GCP) in this sentence, which is the first time the term is mentioned.

(viii) P 13, L33-38: What is the range in elevation among the field plots? It is probably quite small. Could it be that topography wasn't a strong predictor of plant/soil attributes because the digital surface model was not accurate enough to differentiate small, but ecologically important differences in elevation among plots? Topography might be a stronger predictor where there is greater topographic variation among field plots.

Tables and figures

F1. It is hard for me to differentiate some of the lines used in the figure. Could these be plotted using color, or a variety of line types?

F3. Spell out "OM" in the figure legend before using the abbreviation.

F4. Specify that the plotting symbols represent averages and error bars represent 85% CI.

F7. Increase the size of text in the figure.

F8. Proved a little more detail in the legend, such as how the land cover map was derived.

F9. Most of the legend is contained with in parentheses and separated by a bunch of semi-colons and comma. Breaking those five lines into several sentences would make it easier to read.

T2. Perhaps note in the table caption that these numbers are derived using the multi-response permutation procedure.

T3. The column names “early season SOM” and “late season SOM” are somewhat confusing. Maybe it would be clearer to label those two columns as “SOM in unfrozen soil (Gg)” and then have sub-column names labeled “Early season” and “Late season”.

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