Earth Syst. Sci. Data Discuss.,
doi:10.5194/essd-2016-36-RC3, 2016

Interactive comment on "Understanding the representativeness of FLUXNET for upscaling carbon flux from eddy covariance measurements" *by* Jitendra Kumar et al.

Anonymous Referee #3

Received and published: 7 October 2016

Kumar et al present a method and associated datasets for the representativeness of FLUXNET sites as well as a method to produce gridded flux maps based on the representativeness analysis. The topic itself is very important, and the paper is well written. However, I see many caveats of the analysis.

Major points:

Representativeness analysis: Concept: There is no introduction or discussion of what is meant by representativeness. The representativeness of "sites" described by static biotic (e.g. vegetation type) and abiotic (climate, soil, topography) conditions should be related to a certain variable of the sites, e.g. GPP. I would argue that the representativeness of sites with respect to GPP is different from the representativeness of sites

Printer-friendly version

Discussion paper

with respect to NEE or with respect to biodiversity or whatever. Suppose GPP would only depend on radiation and suppose NEE would only depend on time since last disturbance, then for GPP representativeness should refer to sampling existing radiation conditions, and for NEE should refer to sampling existing conditions of time since last two disturbance. The two maps would look entirely different. Basically, variables considered in representativeness analysis should be specific to a certain property or flux of interest and only relevant and driving variables should be considered. Here, the authors do the example for GPP and use only static climate, soil, and topographic variables. The selection of variables seems to be ad hoc and whatever is available at high resolution. There are no variables with respect to vegetation properties which would actually be available too like maps of vegetation type, fapar, tree cover fraction etc. Now comes the temporal aspect: one could argue that a site in the temperate zone samples temperature and radiation conditions from "tundra" to "tropical" over the course of the seasons. Which role does this play for representativeness?

Method: The results of the representativeness analysis depend on the variables that are plugged in, their correlation structure, and the chosen distance metric. It is not clear how heuristic choices influence the results qualitatively or quantitatively. Would e.g. running a PCA, and rescaling the first few components to represent environmental conditions yield the same results as plugging in all variables individually?

Analysis: Instead of analyzing the representativeness of each year by considering only flux tower sites operating at that year only I would find the cumulative effect, i.e. considering sites of this and previous years at least as relevant and informative. .

"Upscaling" The method is my opinion conceptually flawed. The authors consider a certain location, year, and month and then take the weighted mean of GPP of similar mean static environmental conditions for the same year and month. It could for example happen that for a pixel in the northern hemisphere in July flux measurements from the southern hemisphere with inverse seasonal cycle are considered because they appear similar in the space of static environmental conditions. Using static variables to map

Printer-friendly version

Discussion paper

seasonal and interannual variability is in itself highly questionable. The time varying coverage of FLUXNET data availability will create artefacts and discontinuities of the GPP product, which was also mentioned by the authors. The upscaling method itself is heuristic – there is no learning procedure involved to find a robust mapping for GPP based on environmental variables. There is no assessment of how the method works at flux tower sites. It would be super easy to do a leave one tower out cross-validation to evaluate how well it works – it's not shown and I guess it would show that it does not work. The authors' comparison against MTE-GPP for each year makes little sense in my opinion because spatial patterns of GPP won't change much between years (GPP will always be high in the tropics and low in dry and cold regions). If the authors wanted to compare interannual variability the mean per pixel should be removed to compare anomalies. Given that there are other approaches based on training machine learning algorithms on the market that make conceptually more sense, that have been evaluated, and are quite well established, I honestly do not see the point of proposing this method as a new useful approach for upscaling FLUXNET measurements.

Unjustified remarks and overstatements: There are many unjustified and overselling statements in the manuscript. For example: abstract (line 13): "optimal use"; page 10 (line 2-6; "Upscaling . . .[]"): sounds general but is not!; page 10 (line 26): "preserve variability" – I believe the method is not preserving variability but introducing a lot of artefacts. Page 13 (line 12-14): "our approach is better"; page 13 (line 30): again, there is no evidence that this method is preserving any useful variability; page 17 (line 4-6): "exhibits year to year variability in accuracy".

Minor points: Adding equations on top of describing the maths with text is necessary

Jung et al 2009 seems to be the wrong paper for the MTE product which is introduced in Jung et al 2011

Interactive comment on Earth Syst. Sci. Data Discuss., doi:10.5194/essd-2016-36, 2016.