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

Anonymous Referee #1

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This study presents an assessment of the representativeness of FLUXNET based on the recently released FLUXNET2015 data set. In light of their past work looking at the representativeness of the AmeriFlux (Hargrove et al., 2003) and 2007 Fluxnet (Sundareshwar et al., 2007)networks, this is a timely and important effort. Studies like this inform users of such data about how representative their data may be beyond the narrow confines of a flux footprint. And there is precedence indicating that similar forests may function similarly despite wide differences in distance, as is case of the comparison of carbon fluxes of beech forests in France and Denmark (Granier et al., 2002). Such a study is also critical for the use and interpretation of flux data by global modeling exercises. We know we can't measure fluxes everywhere, but if the sparse network

is representative of greater climate and ecological spaces, then these measurements can infer information about locales where no measurements exist.

Introduction

The authors do a good job justifying the topic of the paper and describing work that has been done. This effort is especially timely since the FLUXNET 2015 dataset was released this past July. As more and more scientists use this data, having a paper discussing the representativeness of these data will be very useful to the synthesis, remote sensing and modeling communities.

Materials and Methods

The method relies on maps of climate, soils and ecosystems, and given this information it tells us how well the current networks are sampling the ecosystems of the globe. Such an effort is instructive for guiding the placement of additional towers and indicating a priority.

Of course the method is not perfect and is lacking in assessing the representativeness of sites given similar stand age and time since disturbance. Incorporating stand age maps is a direction to think about in future incarnations of this work (Pan et al., 2011). The approach also misses out on the complex mapping of crops in the corn/soybean belt. Since C3 and C4 crops differ so much in their photosynthetic potential and net and gross carbon fluxes (Suyker and Verma, 2012), one could expect sizable uncertainties associated with maps in this region. I'd like to see the authors give an open and unbiased critique of the pros and cons of their representative mapping method and where the approach needs to go into the future as we glean more and better information of the Earth system.

A strength of this work is its discrimination and analysis of the potential (with 700+) network and the actual (160+) network in the living database. Efforts like this can be important for recruiting more outstanding data into the global fold of information.

The paper does a nice job producing maps of these different networks.

Much of this analysis rests on a diverse list of bioclimatic variables, mapped at 4 km resolution. It is unclear if all variables are needed, what their uncertainty is as they are spatially filled, and if all the variables are needed. In addition, in some regions seasonal climate may be better than annual averages or sum. Here, I applaud the authors as they consider quarterly statistics and are agnostic to the timing, giving them flexibility for north and south hemispheres and Mediterranean ecosystems with a winter/spring growing season.

Splitting and lumping is always an intense argument among ecologists. Parsimony has its strength and here I see 10 ecoregions are describes. More information on why this value would be helpful. In other words, what happens if there are more or less?

Figure 3. I like the examples of how representative 2 candidate sites may be. However the authors need to do a better job instructing the reader on the nuance of the grey scale. Is 0 good, why, and how bad is 0.10? In other words we need some better context to interpret the numerical values of the grey scale. A rainbow color map may be more instructive. Plus the figure is vague to what the grey scale is. Nothing is said in the figure caption, nor does the text clearly state if it relates to any equation in particular. While I like the topic, the authors need to work a bit more on being clear and telling a clearer narrative.

Figure 4 is very important as it tells us how representative the 164 site dataset is. Now in reality there remain data in the 2007 La Thuile dataset that for practical reasons did not migrate to the 2015 Fluxnet database, but are still publicly available. So seeing a map with sites from both datasets could be instructive, too. Please add an additional map with the sites in the 2007 and 2015 datasets. The range of the grey scale in Figure 4 is only between 0 and 0.025. What does this mean? Again a rainbow color palette may be better in distinguishing representativeness better. At present all I can assume is white is good.

I would also like to see a potential representative map based on all the registered sites and compare them with the actual network. This is missing and needed information by this analysis.

It is interesting to see the missing regions like Latin America, parts of Asia, Africa and the Pacific Northwest. Now FLUXNET Canada had a number of sites in the Pacific Northwest. I would hope this dark region would lighten up with their inclusion.

Figure 5 is interesting. I like the more detailed and in depth analysis of this paper and looking at the history and growth of the network.

Figures 1 and 6 and Table 2 tell us nothing about the assigned ecoregions in the maps. The authors need to pay more attention to specific details. We need either a table, legend or caption telling us what regions 1 through 10 refer to. More attention to detail is needed here and throughout the paper.

Table 2 is interesting and important in terms of documenting ecoregions that are relatively over and under sampled. Again this analysis is important for helping the network evolve and improve, either with new sites or better recruiting of existing data from under reported regions. We do know there are many more flux towers across China and Brazil and a few in Argentina and Equador. So there is hope.

This paper does something new and different from the earlier incarnations by assessing the representativeness of GPP. On one hand, this maybe is a good idea, given the complex use of climate, flux networks and remote sensing by Jung et al, Beer et al and Xiao et al to produce both global and continental flux maps. Yet, as I read the material I see a lacking of connection between the first part of the paper and this part. The question to me revolves around the uncertainty in global GPP given the actual flux network. I think this team would do better service comparing estimates of GPP with a sparse actual network and with the ideal denser registered network. As I read on and came back to this section, I think my problem in understanding what is going on stems from the very, very vague description of what this team did to 'upscale GPP', compared

to the MTE approach of Jung et al. Much more and better information is needed for understanding what you did.

Results Figures 7 and 8, the legend is impossible to read as the numbers are too faint. Please consider reproduction. Again I feel that the authors have gone off on a tangent and I would argue that they redefine and confine the focus towards representativeness and how increasing and decreasing sites affect integrations of GPP. The comparison here with the MTE approach should be removed and can be the topic of a different independent paper. Careful editing is needed to tell a consistent story. I suggest referring to Josh Schimel's excellent book on writing science.

Figure 9 would be more informative if you could compare information for these latitude belts given the number of towers and uncertainties associated with more or less towers. As you know the tropics are sparse with towers so their uncertainty is high relative to what one may deduce from MTE. Try and do this type of representative analysis. Figures 10 and 11 seem to be a stab at doing this but maybe rethink how you displace and compare the data.

In conclusion, this paper has many strengths and has potential to be a fine and novel contribution. But it needs editing, revision and refining its analysis. I felt that the second part on GPP went off on a tangent, especially since this section was not introduced or explained well in terms of the new method and why. I think it can be fixed and so recommend major revision. Plus this portion was not well described in terms of operation or why the authors digressed in this direction. it is actually a different paper, and given that this was supposed to be a brief contribution, is very much off target.

Personally if this is to be a brief contribution I think there is enough material to produce representative maps based on the 2015 data set, the combined 2007 and 2015 dataset and all registered sites and follow up with a critique on the strengths and weaknesses of this mapping method and where it can and should go.

Finally most figures need better description, legends and bolder labeling

Specific comments

Line 19, You should find a primary the citation to estimates for global photosynthesis rather than the textbook of Schlesinger and Berkhardt. Many fine papers and a review (Anav et al., 2015) exists on this topic.

References

Anav, A. et al., 2015. Spatiotemporal patterns of terrestrial gross primary production: A review. Reviews of Geophysics: n/a-n/a. Granier, A., Pilegaard, K. and Jensen, N.O., 2002. Similar net ecosystem exchange of beech stands located in France and Denmark. Agricultural and Forest Meteorology, 114(1-2): 75-82. Hargrove, W.W., Hoffman, F.M. and Law , B., 2003. New analysis reveals representativeness of the AmeriFlux network. EOS, 84: 529. Pan, Y. et al., 2011. Age structure and disturbance legacy of North American forests. Biogeosciences, 8(3): 715-732. Sundareshwar, P.V. et al., 2007. ENVIRONMENT: Environmental Monitoring Network for India Science, 316(5822): 204-205. Suyker, A.E. and Verma, S.B., 2012. Gross primary production and ecosystem respiration of irrigated and rainfed maize–soybean cropping systems over 8 years. Agricultural and Forest Meteorology, 165: 12-24.

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