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Interactive comment on "Predicting carbon dioxide and energy fluxes across global FLUXNET sites with regression algorithms" *by* Gianluca Tramontana et al.

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Dear Referee, Thanks very much for providing detailed comments to our work. Please find enclosed the responses to all comments point-by-point.

Comment 1: Tramontana et al. present a study in which they have fit various empirical models to CO2 water and energy fluxes across eddy-covariance sites. The results are clear and unsurprising: the statistical fitting methods all performed comparably, and the energy fluxes were more easily predicted by the statistical models. The study is well executed and no doubt will be well cited by follow-on studies that use this dataset for research. That said, I was somewhat disappointed at the level of insight the results conveyed. It is not clear what we have learned beyond a statistical comparison of fits. The

results are presented as dense tables of statistics (even the figures are graphical representations of statistical tables) where fits are classified as better or worse than others, but with little or no discussion or interpretation of the underlying biogeosciences.

Reply 1: We thank the reviewer for these comments and we agree that results and discussions should be restructured in order to bring a more interesting and clear message. We will also revise the Introduction section to frame our work in more broad and relevant questions and dedicate space to discussion about how this work is relevant for answering ecological questions.

Comment 2: The manuscript would clearly benefit from a more descriptive comparison of modeled vs. data. For example, I would suggest presenting Figures B1 and B2 in the main text. Perhaps see Mahecha et al. for ideas on how to gain more insight from comparisons of models and observations. Mahecha, M. D. et al. Comparing observations and process-based simulations of biosphere-atmosphere exchanges on multiple timescales. J. Geophys. Res. 115, G02003 (2010).

Reply 2: We thank Referee #3 for the suggestion, but we think that the paper would lose focus when including a too detailed site-by-site analysis as was presented in Mahecha et al. We will restructure and improve the presentation of the results and discussion sections and we consider to incorporate figures B1 and B2 in the main text as suggested by the reviewer.

Comment 3: One important note is that GPP and RE are modeled. From the methods it appeared that gap-filled data were also included in the fitted data. Some discussion on comparing models with modeled data is merited.

Reply 3: It is correct that GPP and RE are not direct measurements but are derived using models where model parameters are estimated in short temporal moving windows. To acknowledge this source of uncertainty we employed GPP and RE estimates from two independent flux partitioning methods. The first extrapolates daytime ecosystem respiration using fitted relationships on the basis of nighttime data (where RE is measured due to the absence of GPP) whereas the second uses daytime NEE and an hyperbolic light response curve to derive GPP and RE. Both methods yield highly consistent results. The difference between flux partitioning methods turns out to be even smaller than the spread across ML algorithms. We plan to insert a paragraph in the discussion section on this aspect and the model-to-model comparison issue. Regarding the fact that data are gap-filled, we filtered the data and periods with more than 20% of gap filled data with low confidence were not used in upscaling. As such the influence of gap filling was minimized. Restricting the training data set of the ML methods to periods with 100% of measured fluxes is impossible because almost no data would be available at the time resolution used. It is also important to consider that the gap filling algorithm utilizes highly localized and site specific relationships between fluxes and meteorological conditions (MDS method, Reichstein et al 2005), while the ML cross-validation presented in the paper are based only on data from other sites. We will insert a paragraph in the discussion on this 'data quantity vs data quality' trade-off.

Comment 4: The authors briefly reference observational uncertainty when considering their results but it is not clear to what extent they have accounted for uncertainty. Do the models fall within the uncertainty of the observations?

Reply 4: We did not account for the propagation of the measurement uncertainties in a formal way; however, we will add a paragraph in the discussion about this issue. The random uncertainty of fluxes can be thoroughly quantified but we guess it does not have a big impact because: (a) it diminishes quickly as one aggregates to daily or even eight days values, and (b) the risk of model's bias is reduced with random uncertainties. The bigger problem is related to the systematic uncertainties for which we only have some heuristic approaches to assess them (u*, different flux partitioning methods, energy balance closure . . .). We will add a sentence explaining that a rigorous quantification of measurement uncertainties, both random and systematic, would allow for propagating those formally with some of the machine learning methods used.

Comment 5: The main benefit of such regression algorithms in the context of Fluxnet is

scaling. It would greatly increase the impact of the paper if the authors used the trained algorithms to scale each of the fluxes to the globe. This would be relatively easy to do, and the difference between the global estimates would be much more insightful than the statistics currently presented.

Reply 5: We agree this would add significant value in the context of scaling FLUXNET. A companion paper that uses our results as a point of departure is under preparation. It will feature global estimates as well as wall-to-wall maps.

Comment 6: The manuscript would benefit from revisions for the correct use of English. Minor comments: Line 31: "ML and setups"?

Reply 6: Different ML and experimental setups. If ML comparison will be moved in supplementary materials, we can leave only experimental setups.

Comment 7: Line 41: Updated 2013 IPCC reference, Line 44: "are equal" Line 45: "accounted for" Line 59: Perhaps cite Moffat et al. here, as it contains a good discussion of the relative benefits of both approaches. Moffat, A. M., Beckstein, C., Churkina, G., Mund, M. &Heimann, M. Characterization of ecosystem responses to climatic controls using artificial neural networks.Glob.Chang.Biol.16, 2737–2749 (2010). Line 61: "generally come from" Line 78: "The ML tools used span" Line 113: "we removed 5%" Line 294: "with respect to"

Reply 7: Thank you, the point outlined in minor comments will be changed as suggested.

Comment 8: Line 105: So gap-filled data of high confidence are being included? Some discussion on the dangers of fitting a model to modeled data might be warranted.

Reply 8: We are going to add discussion on this issue in the methodological related discussion.

Comment 9: Line 294-296: On what spatial and temporal scale? Daily NEE is typically not affected by external factors. The sentence reads as a result of the study but in reality it is a hypothesis you propose to explain the lack of model fit. You do not identify management influences or lagged effects.

Reply 9: Yes. We will clarify it in the discussion of the revised manuscript

Comment 10: Line 298: How were the uncertainties in H, LE and NEE quantified? I do not see that presented anywhere. It is not clear where your claim that the uncertainties were larger comes from.

Reply 10: We did not account for the measurement uncertainties in a formal way.

Comment 11: Line 340: This sentence is not clear

Reply 11: Sorry for the misunderstanding. The sentence will be removed from the new discussion section.

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