

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

**Anonymous Referee #3**

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Tramontana et al. present a study in which they have fit various empirical models to CO<sub>2</sub> 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.

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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).

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.

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?

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.

The manuscript would benefit from revisions for the correct use of English.

Minor comments:

Line 31: “ML and setups”?

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.

C2

& 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 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.

Line 113: “we removed 5%”

Line 294: “with respect to”

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.

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.

Line 340: This sentence is not clear.

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