

## ***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: In “Predicting carbon dioxide and energy fluxes across global FLUXNET sites with regression algorithms” the authors cross-validate an ensemble of machine learning methods to document the performance of these methods in terms of their spatio-temporal performance. This study is very useful given the role of eddy covariance observations in land-atmosphere studies and the increasing importance of some of the upscaled EC-products in model validation and data analysis. In my opinion the study falls well within the scope of Biogeosciences and addresses a topic that is of

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interest to the journal’s readership. The work underlying the study is of high quality, however, the current presentation can be much improved. If the authors would try to separate the results and discussion, it would become apparent that there is hardly any discussion. Despite the carefully worded objectives, the reader is left with a “so-what” feeling. The way the objectives are worded is too technical and is unlikely to excite many readers. That would be a pity as the results deserve better.

Reply 1: We thank the reviewer for his positive comments on the quality and relevance of our work and more important for the critical view of the structure. We agree with the reviewer that an improved presentation of the material will make the paper more exciting and we will follow the reviewer’s suggestion to frame the objectives of the paper in an attractive way for a broader audience. We will also insert a paragraph in the introduction to clarify that this paper presents the backbone of an ensemble of global gridded flux products generated by the FLUXCOM initiative (which will be introduced in follow-up manuscripts). We only partially agree with the reviewer that there is hardly any discussion – there is substantial discussion in particular related to methodological aspects, which play an essential role in the paper. However we take the point and we are going to add some discussion points concerning biogeosciences topics and limitation of proposed approach that, we agree, are currently missing. We will carefully consider to separate Results and Discussion in the revised version of the manuscript

Comment 2: Are you looking for the best method or do you want to quantify the up-scaling uncertainty? Both perspectives could be of interest but from the conclusions I understood that all ML results will be archived and that the ensemble will be distributed. If my understanding is correct, this information should already be presented in the introduction. If this is indeed the context of the study, searching for the best ML method becomes less relevant but estimating spatio-temporal patterns in uncertainty becomes even more relevant as users may want to know the uncertainty of the ensemble mean.

Reply 2: We inserted a paragraph in the Introduction to clarify that the cross-validation experiment presented in this manuscript is part of a project that aims to deploy an en-

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semble of globally upscaled fluxes (CO<sub>2</sub> and energy) using data-driven models (FLUX-COM). As correctly pointed out by the reviewer, most users will be interested in using the ensemble median of different machine learning (ML) methods; therefore, we focused the paper on the performance of the median ensemble. This point will be further clarified in the new Introduction section. A section showing also the consistency among predictions by different machine learning methods is currently presented (section 3.4) but its relevance to the aims of the manuscript is marginal. We will likely move it to the supplementary material to make the manuscript more concise.

Comment 3: Also, the reader may want to know how much the uncertainty can be reduced by adding remote sensing and meteorological information in the upscaling process.

Reply 3: This is an interesting and important question for which we have designed the two different experimental set-ups. It is a particularly important question because the use of information from in-situ measured meteorological data implies a trade-off with introducing additional uncertainty inherent the gridded meteorological data needed for the global flux products (as discussed in different places in the manuscript). The comparison between ML based on only satellite drivers and in situ meteorological ones is a key point of our manuscript. We will sharpen this aspect in the appropriate places of the manuscript (Introduction and Discussion) which we believe will satisfy the reviewer's comment.

Comment 4: Listing the current limitations (saturation point) would be very useful, for example, is there anything to gain by adding meteorological data when upscaling NEE?

Reply 4: Identifying and discussing the limitations of machine learning based upscaling is the overarching objective of this manuscript and the Results and Discussion sections are dedicated to that with the main findings summarized in the final section of the paper. Reviewer 2 raises an interesting point about possible saturation points in the predictions and we are working to add a section or a paragraph on this specific as-

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pect. Detecting possible saturation points in the predictions is however not trivial. For example, the uncertainties of the measured fluxes grow with their magnitude such that there will always be observed points that are above the largest predicted value. Other limitations are due to the nature of some of the machine learning used in the ensemble (e.g. Random Forest or Artificial Neural Network) that generally do not provide output values outside their training domains. All this will be discussed in the new manuscript version.

Comment 5: Several interesting findings are not further explored, for example, line 329 reads "suggested that the choice of the explanatory variable had higher impact than the choice of the ML technique for the pattern of predictions". This is a very useful and important finding but it is not at all discussed. There are too many loose ends such as the paragraph on line 317 that reads "Nonetheless, the differences between the experimental setups were less appreciable." A paragraph should have an introduction, a body and a concluding phrase signifying the implication of the result/discussion. This is often missing leaving it to the reader to guess what the authors want to say. Both the structure and language of the manuscript could be improved.

Reply 5: We thank the reviewer for identifying this problem; in the revised manuscript we will address these issues with the help of a proofreader native speaker to help readability and avoid loose end sentences.

Comment 6: The authors choose to use their objectives to structure the paper. I find the objectives very technical and they seems to overlook some of the more interesting questions and answers the study could provide. As an alternative the manuscript could discuss the possibilities and limitations of spatial upscaling and then the possibilities and limitations of temporal upscaling. Defining more general overarching objectives is likely going to result in a better structure and discussion.

Reply 6: We agree that the original version of the manuscript had a very technical structure. In the revised manuscript we will place the objectives of the paper in the

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frame of more general questions that are relevant for a broader audience, in particular to potential users of the global products generated by FLUXCOM. We need however to keep the technical depth and precision for 'good scientific practice' since the methodology presented here is the basis for global flux products. To improve readability we will move some more technical details to the supplementary information (SI). We thank the reviewer for the suggestion on how to split the discussion. We carefully thought about splitting it into 'spatial' and 'temporal' upscaling. We came to the conclusion that structuring the discussion in 'methodological' and 'biogeochemical' questions is more appropriate and appealing.

Comment 7: For the typos and grammar ask help from one of the three native speakers on the manuscript. It makes me wonder whether all co-authors even made the effort to read the manuscript. The display items show a lot of information but not in a way that is easy to interpret or a way that at first sight supports the conclusions. The challenge of synthesis study such as this one is to summarize the information in easy to grasp figures and tables. In my opinion the authors failed in doing so. This issue is apparent from the first paragraph of the results where Table 3 is cited in support of the statement that "The ensemble median estimate always outperformed the median performance of ML-specific methods" but the way I read this table it does not contain information of the specific methods.

Reply 7: The revised manuscript will be proofread and edited by a native speaker with a focus on distilling the volume of information into a coherent storyline that supports the conclusions. In addition the final papers are proofread also by the journal. We will ensure a high quality of figures and tables that convey the main messages and reference them at the appropriate places in the text.

Comment 8: The detailed information could be moved to the appendices. Prepare figures that support the main message(s) of this study, for example, a figure that shows how some temporal characteristics are lost for certain fluxes and/or a figure/map that shows the regions where the methods diverge most.

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Reply 8: Thank you for suggestion. As outlined before, the overall presentation of the material will be substantially modified in the revised version. We prefer not add maps showing the uncertainty at global scale because this is subject of another manuscript (in preparation) on the global products but we will add figures bringing the same type of message.

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