

# ***Interactive comment on “Evaluating the potential of large scale simulations to predict carbon fluxes of terrestrial ecosystems over a European Eddy Covariance network” by M. Balzarolo et al.***

## **Anonymous Referee #2**

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This paper presents a study of land surface models in the application of NEE and GPP estimates comparing with eddy flux measurements. This is a very important issue currently. Therefore, this paper meets the scope of biogeoscience well. However, over all this is a very disappointing paper with several essential academic problems and was ugly written. It seems just a start of a proposal without clear conclusions. There's no significant scientific contribution at all. I strongly suggest that this paper should not be published.

Scientific problems:

#1: Section 2.2: The two reasons they used gridded ERA-I data rather than obser-

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vations are far from sufficiency. Indeed, site level evaluation work usually use site level environmental forcing variables observations. How to compare modeled results with measurements using unsuitable forcing variables? In some cases, there are no enough good records, a compare work need to be made first before using proxy data as drivers.

#2: How to distinguish these impacts together as “a system (P11864, L21)”? Does that mean at the beginning the authors know they will get bad/different results comparing tower measurements? The last paragraph in section 2.2 seems conflict with previous statements.

#3: P11864, L14 “For that reason, also the model vegetation type selection for a particular tower location is according to the model climate data set rather than the vegetation type of the tower location.” This is unacceptable at all. In this way, wrong vegetation types were used during modeling. This will lead the results nonsense except there’s another experiment for comparison.

#4: The authors brought “system” errors in this study and used other papers to explain something (P11870-P11872) or just make some conclusions without proofs. For example:

P11870, L7: “which impact stomatal conductance and photosynthetic activity (Schaefer et al., 2012).”

P11870, L24: “Therefore, errors in any of the atmospheric variables can have a marked impact on LAI (Szczypta, 2012).

P11871, L7: “This analysis confirms that the performance of the models in predicting NEE is closely related to climate and site characteristics.”

P11872, L16: “It could be linked to an incorrect phenological and LAI description.”

In this way, there’s nothing could be archived because there’s no quantification made by this paper.

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#5 the conclusion section. As I mentioned in #4, the results of this paper could not lead such a conclusion as the authors said. P11874, L6-9: On one hand, the authors want to evaluate “the accuracy of three different LSMs”; On the other hand, they bring “system errors” together using tower-unrelated forcing variables and tower-unrelated vegetation types. Therefore, their “more comprehensive validation” is not real. They need to show many more comparative experiments to make such conclusions. This big mistake can be very visible and clear if the authors separate the results and discussion sections.

They just offered a “plan (LAI, land management, drought . . .)” or guessed something. The essential mistake is that they brought “system” errors but did not quantify each part clearly. So, there is no significant scientific contribution. This draft seems like a simple start of a proposal, and it is far from a scientific paper for publication.

Writing problems (I only list a few of them):

#1, P11858, L1: first sentence is too long.

#2: about the efficiency (E) in section 2.4, could the authors offer some references? It seems like variance contribution.

#3: P11860, L19, where’s Zhao et al., 2012 in the references section?!

#4: P11868, L5-15, using the word “anomalies” could simplify this verbose paragraph about IAV.

#5: The author used “ $-\infty$  to 1 (P11867, L11)” at first, but 50% was used in result section (P11869, L20-22). Be coherent please!

#6: (P11872, L4-7), please do not repeat the figure caption.

#7: The authors should be very careful with references. Otherwise, such an “easy search topic” paper will mislead the young PhDs when they follow the story outline. For example, (P 11859, L3), Jung et al (2010) report a decline in evapotranspiration using

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FLUXNET measurements, and attribute this trend to limited soil moisture. There's no relation between this paper and author's statements "the mechanisms that drive the net carbon uptake ..."

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**BGD**

10, C4866–C4869, 2013

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