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Comment

Interactive comment on “Multi-model analysis of terrestrial carbon cycles in Japan: reducing uncertainties in model outputs among different terrestrial biosphere models using flux observations” by K. Ichii et al.

K. Ichii et al.

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Comment: 1. This paper uses eddy flux data to improve the performance of terrestrial biosphere models, and then compares the performance of these models for predicting carbon fluxes. This is a useful contribution to the areas of data assimilation and carbon cycling. The following issues should be taken care of before the manuscript can be accepted.

Response: No Revision. Thank you very much for constructive comments.

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Comment: 2. The authors did not make it clear how they adjusted the model parameters using eddy flux data. Is this a data assimilation approach? What is it? How did you adjust the model parameters? Details should be provided as this is an important part for understanding why the use of eddy flux data improved the performance of the models.

Response: Manuscript Revision.

1. The model improvement processes are done by iteration (hand-by-hand approach) except for SVM (SVM tuning is done by optimization). Although assimilation and optimization approaches are developed, these are for a specific model (e.g. Ichii et al., 2009 cited in the manuscript and other many papers for optimization and assimilation). In other words, currently, to apply assimilation and optimization approach for model comparison (multi-model analysis) is very difficult. To clarify the method of parameter calibration, we modified the 1st paragraph in section 3.1 as ‘Then we tuned all models to fit the observed GPP, RE, and NEP data by adjusting the model parameters iteratively.’

2. Details in model parameter adjustment are basically written in the original manuscript in Appendix. To clarify them more, we revised some description of model parameter calibrations.

3. We agree that it’s very important to establish some optimization or assimilation approach for systematic model improvement. Therefore, we added to 1st paragraph in section 5.2 (Potential limitations) as ‘Fourth, more objective methods of model parameter calibration such as to set a cost-function and apply optimization routine are expected. In this study, parameter calibration is done by iteration, and part of model differences may be reduced by applying these methods.’

Comment: 3. The use of “uncertainty” is very confusing in the manuscript. It should not be interchangeably used with “difference” or “discrepancy”. The authors mentioned the uncertainties among various models again and again, but these are not uncertainties

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but differences or discrepancies.

Response: Manuscript Revision.

1. Change 'uncertainty' to 'difference' throughout the manuscript if 'uncertainties' can be replaced by 'differences' as suggested.

Comment: Machine learning approaches should not be classified into terrestrial biosphere models. Moreover, unlike terrestrial biosphere models, the default parameters of the Support Vector Machine approach were obtained using eddy flux measurements. The Support Vector Machine approach (both text and associated components of tables and figures) should be removed.

Response: No Revision

We think that machine learning approach (empirical approach) should also be included as a member of terrestrial biosphere model. Reasons are as follows;

1. Machine learning approaches are based on statistics, therefore, sometimes called as 'statistical models or data-oriented models' and became more important recently (e.g. Xiao et al., 2008, Xiao et al., 2010, Yang et al., 2007, Jung et al., 2009).

2. Statistical models have been included as a member of model intercomparisons in some studies (e.g. Vetter et al., 2007 (cited in the manuscript)).

3. Increasing the number of models can reach more general conclusions and insights. Therefore, including of SVM has advantage.

4. The reviewer pointed out that default parameters were obtained using eddy flux measurements (but from Ameriflux sites). In our analysis, the model was improved by using flux sites in Japan. Therefore, we can say that adding local flux observation sites is important to improve the model.

5. We think that there is no conclusive classification of 'terrestrial biosphere models' currently. Therefore, we cannot exclude SVM from models.

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Comment: Lines 21-23: “Flux observation data significantly improved terrestrial biosphere models, not only on a point scale but also on spatial scales”. This statement is too strong. The use of eddy flux only optimized model parameters, but did not improve model structure and algorithms at all. This statement should be rephrased to something like this: “The use of eddy flux data optimized model parameters and thereby improved the performance of the models for predicting NEE”.

Response: Manuscript Revision.

1. As suggested, we changed as “The use of eddy flux data refined model parameters and thereby improved the performance of the models for predicting NEP.”

Comment: Lines 9-14, page 8458. The motivation of these previous model intercomparison efforts was not to quantify the uncertainties in the models and to identify the causes of the uncertainties, but to compare the performance of these models for simulating carbon and/or water fluxes. Again, uncertainties and differences (discrepancies) should not be used interchangeably.

Response: Manuscript Revision.

Change as suggested. ‘To quantify the uncertainties in the terrestrial biosphere models and to determine the causes of these uncertainties’ to ‘To compare the performance of these models for simulating carbon and/or water fluxes and evaluate differences among model,’

Comment: Lines 1-2, page 8459. “however, larger uncertainties in each biosphere model remain owing to validation with observations.” This statement does not make sense. Did you mean “owing to calibration (or parameter optimization) with observations”?

Response: Manuscript Revision. Revised as suggested: ‘owing to lack of calibration’.

Comment: Following this statement, the authors argued that “previous model intercomparison projects generally lacked detailed validations, resulting in errors in the

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simulations.” (lines 4-6, page 8459). This makes no sense. Did you mean detailed calibration or model optimization? Optimization and validation are two totally different concepts, and should be not used interchangeably. Moreover, not all errors in simulations are from parameters. Model structures and algorithms could lead to substantial uncertainties in simulations.

Response: Manuscript Revision.

1. As suggested, we meant ‘previous model intercomparison projects generally lacked detailed model calibration, resulting in errors in the simulations.’ We revised the manuscript.

2. We agree that model structure and algorithms could lead to substantial uncertainties in simulations. We added it in discussion section as a future potential improvement of the study as ‘Fifth, we only focused on model parameter calibration in this study. To reduce model uncertainties more comprehensively, we also need to analyze the uncertainties in model structure and algorithm.’

Comment: Lines 23-29, page 8459. Are these three bullets really the goals of the manuscript? If so, the manuscript did not reach these goals. Apparently, the actual goal of the manuscript is to compare the performance of various models for predicting carbon fluxes and to reduce the uncertainties in model simulations and the differences among models using eddy flux data.

Response: Manuscript Revision.

1. This paragraph described how our study meets CarboEastAsia projects. Therefore, these descriptions basically do not consistent with our goal of the study in a strict sense. However, this paper gives a first step toward these goals, and also described in the text as ‘As a first step, we selected a test site in Japan where the flux network was dense and could be used as a case study.’ (Introduction section)

2. We changed the description as ‘Among the eight ultimate purposes of the CarboEas-

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tAsia A3 Foresight program (<http://www.carboeasia.org>), our group is ultimately aiming at three goals' (Section 1, last paragraph, first sentence).

Comment: Lines 22-27, page 8467. The explanation for the underestimation of the seasonal magnitude of carbon fluxes is quite right. How does the underestimation of GPP lead to a significant change in Re? NEE is the difference of GPP and Re. An increase in Re can also lead to a reduction in net carbon uptake.

Response: Manuscript Revision.

1. Since these simulations assume the quasi-equilibrium state of ecosystems, GPP should be closely balanced with Re. Therefore, GPP underestimation also affects Re.
2. Differences in biomass and soil carbon may be a cause of it. We show the biomass and soil carbon estimation in Table S1 for comparison (see supplement of response to referee 1's comment). However, we cannot make a conclusion at current stage.
3. Since NEE is a small differences of GPP and Re, it is difficult to discuss the cause of underestimation. We changed the description to 'This underestimation is probably the result of inappropriate modeling of photosynthesis activities, which results in underestimation of RE. Since NEP is a difference of GPP and RE, these small biases may affect NEP.'

Comment: The improved model analysis at the point scale does not mean much. You used the eddy flux data to optimize the model parameters, and then compared the new simulations with eddy flux data. Of course the simulations will have better agreement with observations.

Response: No Revision

1. We basically agree that improved model simulations will have better agreement with observation, however don't agree that improved model analysis does not mean much. By establishing the improved model, differences among models are also reduced in the spatial simulation as well as point one. In addition, the further comparison of uncali-

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brated parameters or observed physiological parameters will potentially help to identify the cause of model uncertainties.

Comment: Lines 25-26, page 8470. This statement is not supported by data. The numbers are indeed different. But how did you know which estimates are more accurate? Without the support of independent, spatially explicit data, how did you know the new model simulations led to improved estimates of carbon budget across Japan?

Response: Manuscript Revision.

1. We agree that we cannot know which estimates are more accurate. Therefore, we changed the sentence to 'Annual total statistics in Japan were also greatly changed by the model refinement' as suggested.

Comment: Lines 10-12, page 8463. Specify the version of MODIS data products used (Collection 4 or 5).

Response: Manuscript Revision

Collection 5 for point analysis and Collection 4 for spatial analysis. At the time of analysis, MODIS Collection 5 data were released, however, we could not use it due to technical problems. We clarified the version of MODIS data in the text.

Comment: Lines 24-27, page 8463. Explain why you used NCEP/NCAR reanalysis for point analysis rather than the AMeDAS data collected in Japan with much higher spatial resolution.

Response: No Revision

The corrected NCEP/NCAR data (used in this study) have several advantages compared with the data used in the spatial run.

1. The climate data used in the spatial run is based on observed temperature, precipitation, and wind speed only. Solar radiation and humidity related parameters were estimated by MTCLIM model (described in section 2.3.3.), which adds additional bi-

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ases.

2. The climate data used in the spatial run is based on the meteorological observation network, which do not always represent flux site meteorology.

3. The corrected NCEP/NCAR data are created through adjustment with flux site meteorology. Therefore, the data can solve above mentioned problems.

Comment: There is redundancy in the results section. The whole section can be reduced and be more concise.

Response: Manuscript Revision.

1. We eliminated redundancy especially in section 4.1, and text was substantially reduced.

Comment: Line 22, page 8458. “more reliable estimates” were mentioned twice.

Response: Manuscript Revision

Done as suggested.

Comment: Lines 20-21, page 8466. “seven models out of eight” should be “seven out of eight”.

Response: Manuscript Revision

Done as suggested.

Interactive comment on Biogeosciences Discuss., 6, 8455, 2009.

BGD

6, C4783–C4790, 2010

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