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Biogeosciences Discussions

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.

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Comment: This study shows that uncertainty in simulated carbon budgets can be significantly reduced when the models are calibrated with flux observations in East Asia. I think that this study is helpful to identify current status and improvement strategy of the model for simulation of carbon budget in East Asia. Some explanation and description are missing in the text, which gives some confusion in understanding paper. Therefore, I suggest authors to revise manuscript according to following comments.

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

Comment: 1. Clarify the method which has been used for parameter calibration at each site? That is, what is cost function for parameter calibration.

Response: Manuscript Revision 1. In this study, parameter calibration is based on the user's iteration (by hand). Application of cost function or optimization approach to parameter calibration is beyond the scope of the study, because these approaches is basically developed in each specific model, and creating common routine is very difficult at current stage. There are many studies which established optimization or cost function, however, have never applied it for multi-model analysis. 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. We agree that it's very important to establish some cost function or optimization 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: 2. Authors mentioned that Appendix A described the model initialization or spin-up processes. But Appendix A does not have any description about the model initialization or spin-up processes. Add description about the model initialization or spin-up processes.

Response: Manuscript Revision We added model initialization/spinup processes in each model (except for SVM which does not require model spinup) in the end of each subsection in Appendix A. Basically, all models adopted commonly used procedure for model spinup, i.e. running the model for long-period using climate input repeatedly.

Comment: 3. On page 9, following sentence is obscure in meaning. "For point analysis, we used either observed data from each flux site or long-term climate analysis data

(1948-2006) from NCEP/NCAR." Does that mean that some site used observed data and other sites used reanalysis data? Or reanalysis data were used for gap-filling of observed data? Or Reanalysis data were used for spin up run? Clarify the sentence.

Response: Manuscript Revision No. Some models used site observed climate data (SVM, CASA, TOPS, Biome-BGC, LPJ and TRIFFID) and others (DAYCENT, VISIT, and SEIB-DGVM) used the data from corrected NCEP/NCAR (NCEP/NCAR data corrected by flux site meteorology). It is based on each user's opinion. To clarify it, we added in section 3.1 as 'For point analysis, we used either observed climate data from each flux site (SVM, CASA, TOPS, Biome-BGC, LPJ, and TRIFFID) or long-term climate reanalysis data (1948-2006) from NCEP/NCAR reanalysis 1 (Kalnay et al., 1996) that was extracted at the corresponding pixel and corrected using site observations (DAYCENT, VISIT and SEIB-DGVM).

Comment: 4. Explain why you used NCEP/NCAR reanalysis data for point analysis instead of climate dataset at a 4-km spatial resolution which has been used for spatial analysis.

Response: No Revision. The corrected NCEP/NCAR data (used in this study) have several advantages compared with the data used in the spatial run. We think it's a minor issue, and did not describe in the manuscript. 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 biases. 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: 5. Ecosystem respiration rate depends on carbon pool amounts. Do the used ecosystem models simulate carbon pool amount reasonably at each site? Men-

tion about that in the text.

Response: No Revision (but showing results in this document) 1. We compared modeled biomass and soil carbon (Table S1 in supplement), and found that estimations are generally consistent with commonly known statistics. Unfortunately, we don't have direct observation for these sites. We also found estimated carbon pool amounts show small differences between original and improved models with smaller differences among models by the model improvement (especially for biomass carbon pools). 2. We think assumption of equilibrium of carbon cycle in the model (lack of site history in the simulation) is one of the main cause of inaccuracy of respiration modeling. (These have already been mentioned in Section 5.2, Last paragraph). 3. We think detailed analysis of the relation between carbon pools and fluxes are beyond the scope of the study. We need more detailed observations such as carbon pools and turnover times and feel that these analyses are needed to be done based on each single model. These potential problems were described in section 6, 1st paragraph as a future task of the study.

Comment: 6. Large improvement of improved model is shown at FJY site. Table 1 indicates that dominant species at FJY site are pine and oak. But in model set up for FJY site, needle leaf forest has been used. Large improvement may be partly due that the site is misrepresented as evergreen needle leaf instead of mixed forest in model set up. Suggest possible reasons for large improvement of improved model at FJY site.

Response: No Revision We guess the reviewer misunderstood. In Table 1, we described that only 'Red Pine' is dominant species in FJY site (no description on 'oak'). The description is consistent with that in AsiaFlux Website (http://asiaflux.yonsei.kr/network/003FJY_1.html).

Comment: 7. Fig. 1 shows that most land cover is mixed forest. But in this study, parameter calibration was made for ENF, DBF and DNF. For spatial simulation, how did you consider mixed forest? Describe it.

Response: Manuscript Revision. 1. We basically used DBF parameter (shared parameter for TKY and TSE sites) for mixed forest except for VISIT and DGVMs. Land cover is not prescribed in DGVMs simulations, therefore, this comment is not applicable to DGVMs. VISIT used TKY parameter (DBF) as mixed forest (because parameters for TKY and TSE are separately calibrated). We added them to the text (Appendix A).

Comment: 8. Vegetation type of both TKY and TSE sites are DBF. How did you calibrate parameters at these sites, separately or together? If you have done calibration at each site separately, are the calibrated parameters from two sites similar each other and how did you make calibrated parameters for DBF for spatial simulation? Describe it.

Response: Manuscript Revision 1. We basically used a shared parameter set for TKY and TSE sites except for VISIT. VISIT calibrated TKY and TSE parameters separately. We added in the text (Appendix A).

Comment: 9. Some models used satellite-based LAI while others predicted LAI. LAI is very important variable for GPP calculation. Difference of GPP among models can be partly due to the difference of LAI among models. Did you compare LAI distribution of models? Mention comparison results in the text.

Response: No Revision. 1. We compared LAI seasonality from both satellite-based and model-based ones (see Figure S1 in supplement), however, we could not obtain clear relation between differences of LAI and model-based carbon cycles. We agree that some of the model differences are characterized by LAI differences, however, its effect might be small because of high LAI in these sites. 2. Currently, we don't have reliable observation of LAI seasonality, therefore, accurate ones are hardly obtained. In addition, MODIS LAI also has a bias (discussed elsewhere). We described the importance of LAI comparison in the section 6 (conclusion section) as an additional data for model constraints). 3. We added LAI as a requirement of further improvement in the text as 'Although there are still uncertainties in the model algorithm, these

uncertainties will be fully explored in the future through more rigorous investigations and by adding other constraints to the model, such as biomass and soil carbon and LAI, as well as some water cycle estimations.' In the 1st paragraph, section 6.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/6/C4777/2010/bgd-6-C4777-2010-supplement.pdf

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