Responses to reviewers' comments.

Ueyama et al.
Simulating carbon and water cycles of larch forests in East Asia by the BIOME-BGC model with AsiaFlux data
Manuscript for Biogeosciences (CarboEastAsia Special Issue)

All revisions are marked as red in the revised manuscript.

Comment 1:

Percent Leaf Nitrogen in Rubisco (Section 3.2): onset day and length of growing period are not defined. A sentence or two describing these variables would be helpful.

Response:

Thank you very much for the useful comments. Based on the comments, we added the definition of the variables as follows:

"The onset day and growing season length are calculated by the BIOME-BGC model based on an empirical phenology model (White et al., 1997)."

Comment 2:

Snow insulation effect (Section 3.2): The authors take care to explain the scientific basis for modifying most variables, but do not do so here. The reader needs a scientific explanation for why this parameter was reduced. The authors mention equifinality; is it possible that by reducing the sublimation term that snowfall was deeper than observed? In this case, the insulation effect may be caused by deeper snow, not by an incorrect insulation parameter. I am not disputing the papers, but I think a little more explanation would be helpful.

Response:

The discrepancy of soil temperature during snow period of TMK was probably caused by the difference in snow depth, snow density and soil porosity among the sites. To explain the possible reason, we added the sentence as follows:

"this was probably caused by the overestimation of the snow insulation effect probably because the difference in snow depth, snow density, and soil porosity among the sites."

Comment 3:

Spinup (Section 3.3): This section might benefit from some clarification. It sounds like

meteorological observations from the EC flux towers were used in spinup rather than long‐term records, due to data drop‐outs. For sites YLF, NEL and TUR the winter periods were filled with NCDC data. What NCDC product was used? For spinup, it is desirable (when possible) to incorporate a long‐term meteorological record so that anomalies around a mean state have minimal effect. Stations TMK, SKT and NEL have 3 years of data (growing season only for NEL), which I assume is repeated multiple times to force the spinup. Stations LSH, YLF and TUR have only one year of data. If this single year is meteorologically close to the mean, then the spinup is reasonable; otherwise, the model is spun up to an anomalous state. Do we know how 2004 fits into the meteorological mean at LSH, YLF and TUR when compared to long‐term observations? Since NCDC data is being used to fill winter observations at several sites anyway, why not use NCDC data for the entire spinup? I assume that the NCDC data record is long, and will contain variations around the mean annual cycle.

Response:

We used the NCDC data of "Global Surface Summary of Day version 8", and have revised the manuscript by adding the information.

As reviewer pointed out, incorporating the long-term climate data may improve the model performance. However, the availability of climate data is limited, and use of the long-term NCDC data near the station has induced the biased results because the locations between the NCDC and our flux-sites were strictly different. In addition, the NCDC data does not contain solar radiation and VPD data, and use of satellite data (e.g., ISCCP-FD data) as the model input could induce further uncertainties. Although we used the winter data for YLF, NEL, and TUR, the vegetation during those periods was not active owing to the sever weather, and the bias might be small. Therefore, considering the availability of current measurement framework, we believe that our simulation was the best estimate. The uncertainties induced by using the short-term climate have shown in section 4-4. By incorporating the reviewer's comment, we more discussed the uncertainties in this section as follows: "Although the meteorological years used in this study were not substantially deviated from the long-term average (Nakai et al., 2008; Ohta et al., 2008; Hirata et al., 2007), this initialization might lead to some biases in the simulation."

Comment 4:

Canopy interception coefficient (Section 3.4): Changing this value by an order of magnitude for a single site needs more justification than "in order to reproduce the observed ET". The text as is sounds like arbitrary tuning; the reader needs to be told why this was done, and why it is reasonable to do so.

Response:

We do not have a clear answer for this inconsistent parameter in LSH, but it was possibly caused by the limited footprint in this site (Dr. H. Wang, personal communication) as we have already described in section 4-2. Takagi et al. (2008; presentation in AsiaFlux workshop) also reported that the observed carbon fluxes at this site showed somewhat different feature compared with those at the other larch sites. By incorporating the reviewer's comment, we more discussed in the section 4.4 as follows: "The parameter for the canopy interception was needed to be site specific for the LSH site, which was probably caused by the limited footprint in this site (Dr. H. Wang, personal communication)"

Comment 5:

The authors say "The improved model reasonably reproduced the carbon and water fluxes at the daily, monthly and annual time scales." This is not strictly true: model daily water fluxes are poorly correlated to observations for both the base and improved models and they discuss this in Section 4.4. This feature of model performance troubles me, as carbon and water fluxes are tightly coupled. If the problem is with precipitation interception in the model or with limitations in the EC method during precipitation events, then subsampling the data for dry days should show an improvement. Was this done? If so it might be helpful to mention and/or show the results. This part of the comparison gives me the most discomfort; if BIOME‐BGC is unable to capture synoptic‐scale variability in fluxes, then my confidence in the model's ability to respond to variability on longer temporal scales (and this longerterm variability will likely be more subtle) is suspect. If the problems at the daily scale can be explained, then do so. I really think this aspect of the research needs more explanation.

Response:

We revised the sentence to describe existence of the uncertainties in daily water fluxes as "The improved model reasonably reproduced the carbon and water fluxes at the daily, monthly, and annual time scales, although the daily water flux still had uncertainties.".

According to the reviewer's comment, we conducted the similar comparison by sub-sampling the data for dry days, but the performance was not improved. Although the model still has the uncertainties in daily water flux, the monthly and annual fluxes was well reproduced, and the intercepted evaporation was also consistent with the observed ones (Ohta et al., 2001; Hydrol. Process, 15, 1459-1476, 2001). We believe that the model is well captured the carbon and water fluxes as possible as we validate. By incorporating

the reviewer's comment, we more describe the need for further validation-studies for the water fluxes the in section 4.4 as "Consequently, further investigation for both observation and modeling might be required to reveal the water exchanges.".

Comment 6:

Climate anomalies (Section 4.3): This section is difficult to decipher. First, the case labels (ta, tc, te, etc) are not described or shown on the table. Second, flipping between the text and a 33x18 table is extremely difficult to follow. There is a lot of information here to digest. Why was radiation used as a sensitivity parameter? Is there any reason to expect a 3 - sigma change in solar radiation in response to climate change? I think this particular test is irrelevant, unless it is to show that the TMK site is very cloudy and therefore a light - limited environment. The authors downplay the fact that NEE is the small difference between large gross fluxes, but this is exactly the point; small relative changes to gross fluxes under climate change may have a large impact on overall carbon flux, even perhaps changing the sign. I think this section need clarifying, and I would prefer a different method of displaying the results. Many of the values in the table are near 100, and therefore not valuable to display. Might there be a way to graphically show only the important results?

Response:

1. Following the suggestion, we added the case labels on the Table 5.
2. As reviewer suggested, many of the values in the table are near 100%, and it might be difficult to show the results. We have already added the underlines to show the important values referred in the manuscript. We believe that showing the values as table is better presentation for showing the figure, such as bar chart, in terms of reducing the vagueness, and also believe that it is important to show what climate variability was important or not. Part of the reason of difficulty to read the table could be the missing of case, and we have revised the table by adding the case.
3. We showed that radiation variation is also important in carbon cycle (see Table 5), therefore, we need radiation as sensitivity parameter. In addition, the applied radiation bias of 3-sigma is reasonable since it is simply based on the historical climate variability.
4. According to the review's comment, we have more discussed the issues that NEE is a small differences in the large two terms as follows: "The sensitivity study also indicated that the small changes in GPP and RE induced large shift in the annual carbon balance, and the balance could be quite sensitive to the seasonal weather anomalies.".

We thank all the reviews comments and constructive discussion.