Review of **Simulating carbon and water cycles of larch forests in East Asia by the BIOME-BGC model with AsiaFlux data**.

The authors identify a biome that is important to regional surface flux and has not been rigorously evaluated by models. They compare observed fluxes of carbon and water to 'base state' model fluxes and identify mechanisms that might explain discrepancies. They then modify the model and re-evaluate the simulated flux.

The approach of the paper is valid and the method is reasonable. However, I believe that some minor modifications would be appropriate. Some specifics:

- 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.
- 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 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.
- 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.
- 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.
- 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 longer-term 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.

- 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?

I realize that I have a lot of comments here, but I believe that any suggested changes are minor. Most can be addressed with a few words or sentences. The most involved changes that I suggest involve minor rewording and perhaps modifying a few of the plots. I think the overall structure of the paper is good, and I think the scientific question is valid. The approach is straightforward and (with a few exceptions) easy to follow. My formal recommendation as a reviewer is to accept this paper with minor revisions.