Response to Referee #1 (Assistant Professor, Matthias Morzfeld)

General comments: The paper presents a description of the application of a particle filter to assimilate leaf are index observations into a global vegetation model. The paper is concise, and presents only the necessary information, no long background/review section is provided. I find the numerical experiments and results convincing (pending specific comments below).

Response: Thank you very much for the useful, constructive comments to improve the paper. We will revise the manuscript accordingly. Our point-by-point responses are shown in blue.

My main concern is that this paper might not be suitable for Nonlinear Processes in Geophysics. The reason is that the journal emphasizes new methods, applied to realistic problems. The paper simply presents an "old" method applied to a new problem. I find it interesting to read that a particle filter can solve an important and "real" data assimilation, however the general NPG readership might get bored. The authors should decide wether NPG is the best journal to reach the audience they want to reach. This is also reflected by the references, only few of which are to articles in journals similar to NPG. I suspect that this paper would also make a fine contribution in a journal that is more focused on, e.g., Earth system modeling. The authors may want to consider going that route.

Response: We would really appreciate the kind suggestion. After carefully considering other potential options, we reached to the conclusion that we would like to have this paper published in NPG because the main focus of this paper is methodology. It would not be trivial if well-known, “old” methods work with a new problem. This study focuses on the methodological development and performs simple experiments at a single location with only a couple of plant functional types (PFTs). Future studies will include spatial distributions with more PFTs, and will be more relevant to domain-specific journals. We would like to add descriptions regarding this point in the revised manuscript.

Specific comments:

1. I wonder if there is any sensitivity to how repeated particles are perturbed after resampling. The authors chose a random perturbation, but miss to motivate their choice. I think the paper should contain numerical experiments where it is shown that either the method is robust to

(small) changes in how repeated particles are perturbed, or it should be reported how the perturbations influence the results.

Response: Following the suggestion, we performed additional experiments with different random perturbation settings, and found that the filter collapsed for biomass with smaller perturbation settings, though estimated model parameters and other state variables were estimated accurately without collapse. We will add a new section in the revised manuscript to show these results and to discuss the sensitivity to the perturbation settings.

2. The number of particle used is typically important for the results one obtains with a particle filter. Indeed, much of the meteorological literature says that the number of particles required is excessive. To address this issue, I would suggest to run more numerical experiments with a varying number of particles. One can then compute, e.g., means and variances, and check that the method has converged when, e.g., 8000 particles are used. Specifically, I suggest experiments with 4000, 8000 and perhaps 16000 particles (if possible).

Response: Following the suggestion, we performed additional experiments with different particle sizes ranging from 500 to 16000. The results showed that the filter collapsed for biomass with 4000 particles or less when we keep the same random perturbation setting. Also, the model parameters and other state variables were not estimated accurately with 1000 particles or less with a smaller random perturbation setting. We will include the sensitivity to the particle sizes along with the sensitivity to the random perturbation settings (cf. previous comment) in the revised manuscript.

3. I wonder what happens when the data assimilation is initialized with a "smaller" initial uncertainty. The authors define intervals for the parameters, but do not mention how they came up with these intervals. It would be interesting to see what happens when these intervals are shortened or widened. In particular, the particle filter has no mechanism to bring the parameters to values that are not contained within the initial set. This could make things difficult for the "real life" application. Again, I suggest to investigate this issue with more numerical experiments.

Response: We would appreciate the suggestion to perform additional sensitivity experiments on the initial parameter uncertainties. We selected the intervals of the initial parameters based on the ecological knowledge from the previous studies (Kolari

et al., 2006; Zeng et al., 2011; Zhao et al., 2015; Takagi et al., 2015). We will add the specific references in the revised manuscript.

Following the comment, we performed additional experiments with different initial parameter uncertainties. The results showed that the parameters were not estimated accurately with wider initial intervals when 2000 particles or less are used, although they were estimated accurately with narrower initial intervals. With 8000 particles, the parameters were estimated accurately even with wider initial intervals. Sampling a wider interval with a smaller particle size generally reduces the particle density, or the effective number of particles, so that the results seem to be reasonable. We will include these sensitivity results in the revised manuscript.

4. In figs.4 (right column), 5d (right column), 7 (right column), and 8d (right column): it seems that the data assimilation only impacts the parameter estimates for parts of the year, however data are assimilated every 4 days. The authors miss to provide a clear explanation of why that is the case.

Response: We assimilated the LAI only when greater than 0.5, therefore, data assimilation has impacts only in the summer when the leaves appear (i.e., LAI > 0.5). We will add this discussion in the revised manuscript.

5. I would remove all NODA figures, as they do not really carry information. It is clear that when no data assimilation is used, no parameter is changed.

Response: We believe that it would be important to show NODA figures to highlight the impact of data assimilation. Since this is the first time to apply DA to SEIB-DGVM, it is unclear what impact DA would bring. Therefore, we would really like to keep the side-by-side comparison of the experiments with and without DA.

Technical corrections:
I find the use of "newly" in the first sentence of the abstract a bit unusual. I would suggest to re-formulate this sentence. The sentence also appears again later on (p.2. line 6, p.6 line 27), and there it should also be changed.

Response: We will correct these sentences in the revised manuscript.