

Interactive comment on “Non-Gaussian data assimilation of satellite-based Leaf Area Index observations with an individual-based dynamic global vegetation model” by H. Arakida et al.

M. Morzfeld (Referee)

mmo@math.arizona.edu

Received and published: 1 July 2016

General comments: The paper presents a description of the application of a particle filter to assimilate leaf area 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).

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 as-

C1

simulation, however the general NPG readership might get bored. The authors should decide whether 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.

Specific comments:

(1) I wonder if there is any sensitivity to how repeated particles are perturbed after re-sampling. 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.

(2) The number of particles 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).

(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.

(4) In figs.4 (right column), 5d (right column), 7 (right column), and 8d (right column): it

C2

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.

(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.

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.

Interactive comment on Nonlin. Processes Geophys. Discuss., doi:10.5194/npg-2016-30, 2016.