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Geoscientific Model Development Discussions

Interactive comment on "Comparison of Different Sequential Assimilation Algorithms for Satellite-derived Leaf Area Index Using the Data Assimilation Research Testbed (lanai)" *by* Xiao-Lu Ling et al.

Anonymous Referee #2

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1 OVERVIEW

The paper proposes to compare the performance of four data assimilation (DA) algorithms in assimilating GLASS LAI within the CLM4CN land surface model (LSM) using the DART toolbox (version lanai). The four algorithms are: the Kalman filter (KF), an Ensemble Kalman Filter (EnKF), the Ensemble Adjustment Kalman Filter (EAKF) and a particle filter (PF). The authors show that the EAKF produces LAI estimates that are the closest to the assimilated observations. They also study the influence of observation

selection on LAI estimates compared to assimilated observations.

2 GENERAL COMMENTS

The objective of comparing assimilation methods for assimilating LAI in Land Data Assimilation Systems (LDASs) is fair and the choice of the various methods looks sound. The work belongs to a now long list of papers comparing DA methods in LDASs, most of them focusing on soil moisture. The novelty of the paper lies in the comparison of several DA methods assimilating LAI on global scale. Unfortunately the paper in its current form suffers from several issues that prevent it to be published as is. In particular:

- I think your results lack of analysis and validation. You only focus on assimilating GLASS LAI and compare newly LAI estimates with assimilated observations by computing RMSE. By using this sole criterion, you may miss something. The following analyses are missing:
  - The paper misses an analysis on the evolution of variances or ensemble spread of your LAI estimates.
  - You only focus on estimated LAI but your state vector also include Leaf C and Leaf N. How do these two variables evolve in time with DA?
  - You do not validate your approach with independent datasets. To validate a DA system, it is usual to compare control variables or other variables to independent datasets in order to check if assimilation has a positive impact. I suggest you use in-situ observations of LAI or use satellite estimates of evapotranspiration or gross primary production (estimates of both quantities have been shown improved by assimilating LAI) that are independent from the GLASS LAI product to validate your approach more thoroughly.

- Too many details in the description of the experimental setup are missing. For example:
  - Which period of time does your experiment cover? You have atmospheric forcing covering the period 1998-2010 but you only show results for the year 2002. Does that mean your experiment only cover one year? If so, this is not enough to determiner seasonal tendencies. Adding another year of experiment would reinforce your conclusions. If your experiment covers more than a year, please show results for the other years.
  - At which resolution do your run CLM4CN? In Figure 1, you show pictures at 1.0° resolution. Does that mean you run your LSM at the same resolution? Also, I thought that the GLASS LAI dataset was available at 0.05° resolution. Do you do interpolation in order to create the LAI you assimilate?
  - What kind of criterion do you use for observation selection? Is it when "the observed LAI is three times larger than the bias between the simulation and the observations" (l 16-17, p. 13)?

  I know it is impossible to include every detail in a paper or in supplementary materials. But I would like to remind the authors that every reader should be able to reproduce the experiment you conducted after reading a paper. In current form, your paper does not satisfy this important criterion.

- Too many details are also missing in the description of the DA methods you use.
  - I suspect your DA system works pointwise meaning you do not consider spatial covariances in KF, EnKF and EAKF. This is a strong hypothesis (perfectly respectable one). Could you confirm or reject my claim? If true, you should emphasize that point in your paper. If not, the whole analysis of spatial covariances is missing.

  - Could you recall in the paper the different equations involved for each DA method you use? Since it is a paper that compares various DA methods, the reader would benefit from having those written.
  - From what I read, it is impossible to determine which version of the particle filter you are using. Do you use the traditional Sequential Importance Resampling (SIR) filter from Gordon et al. (1993) or do you use more evolved techniques to counteract the degeneracy of the particle filter?
  - To run each member of your ensemble, you use 40 different atmospheric forcings selected from the 80-members DART/CAM4 dataset. How do you select them? Are they representative of the spread (uncertainty) of the whole 80-members atmospheric forcing dataset? If you select them randomly, you may have under-sampling issues (increasing the risk of filter divergence either for EnKF, EAKF and PF). Could you elaborate more on that subject?
  - Ensemble Kalman Filters (either what you call EnKF and EAKF) underestimate systematically variances. What do you do to counteract this problem? Do you use inflation (additive, multiplicative)? If so, how? If not, why?

As you can see the list of my comments is quite long. I do detail few of them in the next section. Nevertheless, I still consider the paper worth to be published if all points are addressed and, therefore, ask for a major revision.

3 SPECIFIC COMMENTS

- About the (lanai) in the title, could you make it more explicit that lanai is a version of DART in the title? It is confusing for the reader if she/he does not know what DART is.

- p. 1, l. 13-14, "*To improve the ability to simulate land surface water and energy balances*", since you show nothing related land surface water or energy fluxes, I suggest you to remove that comment.

- p. 1, l. 23, "*The PF algorithm performs worse than the EAKF and EnKF . . .*". You only consider RMSE as a criterion using for the PF the sampled mean. While using the mean makes sense for Ensemble Kalman Filters, for PF you have more freedom, one could use the particle with the biggest weight (a posteriori maximum for the pdf) for example. Could you add nuance to this statement?

- The introduction tends to mix general DA references to LDAS references making unclear for reading. I suggest you split your review in different paragraphs, one dedicated to DA in general, one dedicated to LDASs and one to the assimilation of LAI. Also many references are missing. Among others:

  - for DA in general: Bannister (2016), Vetra-Carvalho et al. (2018),
  - for LDASs: Lahoz and De Lannoy (2014), Reichle et al. (2014), De Lannoy et al. (2016), Sawada et al. (2015), Sawada (2018)
  - for assimilation of LAI: Sabater et al. (2008), Ines et al. (2013), Jin et al. (2018), Fox et al. (2018)

  Those references should help you build a thorough introduction.

- In section 2.2, can you recall that you use the lanai version of DART?

- Section 2.3.1 about the Kalman Filter (KF). The KF can only be used if your model is linear. Is your LSM linear between two times of observations (roughly 8 days)? If so please indicate what makes CLM4CN linear (as most LSMs are not!). If not, what you are using is rather an Extended Kalman Filter (EKF), in that case, how do you propagate the error covariance matrix from one time of observation to another i.e. how do you calculate the Jacobian matrix of your model?

- Section 2.3.2 about the Ensemble Kalman Filter. What you call the Ensemble Kalman Filter (EnKF) is likely the stochastic Ensemble Kalman Filter introduced by Burgers et al. (1998) and Houtekamer and Mitchell (1998) meaning that observations are perturbed for each member of the ensemble. Could you confirm it? And if so, please refer to those two papers.

- p. 5, l. 33. Eq (1) is false. The denominator of the fraction should be $\sigma_o^p + \sigma_{jo}^p$

- p. 6, l. 8. The variables involved in Eq. (2) are not defined.

- Section 2.5. You put Table 1 in section 2.5 but there is no mention in the text of the observation proportion you perform. Could you add sentences on that subject in section 2.5?

- p. 6, l. 29. You refer to the GLASS LAI dataset but afterwards you instead call them MODIS LAI. While I know GLASS LAI is from MODIS from 2002, it is rather confusing. Could you harmonize your notation?

- p. 7, Fig 1. There is no scale for Figure 1

- p. 8, l. 5-6. "*Figure 4 presents the root mean square errors (RMSEs) . . .*" Strictly speaking, they are not RMSEs but RMSDs (root-mean square differences) since your observations are not perfect. Please replace RMSE by RMSD.

- p. 10, Fig. 4 It looks like the assimilation is far less efficient in the boreal area than in other places. Can you explain why?

- p. 10, Fig 5. The RMSE for EnKF is not consistent to what is shown in Fig 4 (EnKF and EAKF give close results). Can you explain why?

- p. 11, Fig 6. I cannot read the figure. Can you make it bigger?

- p. 13, Fig 8. Have you compared LAI estimates (when you use observation selection) with every obs of LAI or only with those selected? It is rather normal that RMSDs are larger when you do not assimilate every observation than when you do. It would be worth comparing LAI estimates (when you use/do not use observation selection) with the selected observations only and see if you obtain smaller RMSDs.

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