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Biogeosciences Discussions

Interactive comment on "Simultaneous assimilation of satellite and eddy covariance data for improving terrestrial water and carbon simulations at a semi-arid woodland site in Botswana" *by* T. Kato et al.

Anonymous Referee #1

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General comments:

This study investigates the use of environmental observations to improve the estimation of the parameters of a land surface model over an African savannah site. Although there is a need to investigate data assimilation issues in view of the use of future satellite sensors able to observe land surfaces at a high spatial resolution, the subject and methods of the study are not new. The authors should clearly demonstrate the added value of this study. The introduction should be revised in order to better describe the focus of this study. Generally, the model and CCDAS descriptions are too elusive. Re-

ferring to past publications is not enough and more details must be given in the text so that the reader can understand the protocol used by the authors. The model description given in the Supplement should be completed by an Annex to the main manuscript part including the definition and the role of the parameters listed in Table 2. The result Section is very short (only 2 Tables and 5 figures) and this gives the impression that the data were not completely analyzed, and modeling issues not completely addressed.

The results indicate that the maximum plant-available soil moisture Wmax is the more sensitive parameter. This is not a surprise. While ecophysiological parameters can be derived from an analysis of the literature, Wmax is a local property resulting from the soil characteristics and the vegetation type. Wmax is not directly observable and constitutes the most uncertain parameter in land surface models. This rises the question of the usefulness of the complex optimization procedure used in this study. One could have tried to search for physiological parameter values using local observations or a literature review, and optimize Wmax, only, or together with one or two key parameters of photosynthesis and plant growth. Trying to optimize 24 parameters for 2 PFTs at the same time may be disinformative for most of them.

Recommendation: The paper cannot be published in Biogeosciences in the present form. Major revisions are needed.

Particular comments:

Figures: Fig. 1 is never mentioned in the text. Figure 5 is cited before Fig. 2. Figure 2 is difficult to interpret (too many points). It should be split into several sub-figures.

P. 3617 (abstract): the sentence “The closest agreement is found for each observed data stream when only the same data stream is assimilated” is unclear. Moreover, the last sentence of the abstract is confusing as LHF and satellite FAPAR are not measured at the same spatial scales.

Sect. 1 (Introduction): The open literature concerning the data assimilation related

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to vegetation variables is not completely described. Nice references can be found in Biogeosciences and in other journals. The vision the authors have of data assimilation is a bit restrictive. This paper considers the optimization of model parameters, while in monitoring systems data assimilation consists in integrating observations into models in order to continuously update the modeled state variables (e.g. Barbu et al. 2011).

P. 3619, L. 5: “biososphere” ?

P. 3619, L. 7 and P. 3620, L. 7: again, LHF observations are local, and the rationale for trying to merge them with low resolution variables has to be explained/justified.

P. 3621, L. 12: LHF: what about the closure of the energy balance for this site ? It should be mentioned here that the energy balance is not closed for this site.

P. 3622 (top): what is the spatial resolution of the FAPAR product ?

P. 3622, L. 25: what is the time step of the BETHY model (hourly ?, daily ?). Two or three more sentences are needed in order to describe this model further. Since FAPAR is assimilated, it should be made clear how FAPAR is simulated by the model.

P. 3623, Sect. 2.3: More details should be given on the assimilation algorithm. In particular, the difference between prognostic and diagnostic variables should be made clear. The very large bias between the observed and prior FAPAR is a big issue. The assimilated variables should not be biased too much.

P. 3624, Sect. 2.4: the considered biomes includes 2 PFTs (trees and C4 grass), it should be made clear which parameter values apply to the 2 PFTs or not. It seems to me that most parameters should display contrasting values from trees to C4 grass. I was not able to see these differences in Table 2. The prior Wmax value in Table 2 (1500mm) is completely unrealistic. Why using such a value ? I have the impression that the authors prescribed unrealistic parameter values on purpose in order to show a dramatic impact of the CCDAS. The prior parameter values should be based on published standard values of these parameters.

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P. 3625, L. 19: the substantial reduction in the Wmax value is mainly due to the unrealistic prior value.

P. 3627 (top): why is there a time lag between the observed and simulated maximum FAPAR in Fig. 3 ?

P. 3627, L. 17: Wmax=332mm is not a small value (see for example Calvet et al., GMD, 2012). “General belief” is not a proper reference.

P. 3629, L. 8: “paramters” ?

P. 3629, L. 10: does it mean that the functional relationship between Vmax and fci is not sufficiently accounted for by the BETHY model (at least for trees) ?

P. 3631, L. 9-21: Problems in assimilating FAPAR may be caused by the way this quantity is simulated by the model. More details have to be given about the simulation of FAPAR and about the radiative transfer model used in BETHY. Please explain why the joint assimilation of LHF and FAPAR degrades the LHF score that much. Please explain why the assimilation does not significantly improve the GPP simulation.

P. 3632, L. 15: The FAPAR scale issue is even more acute for SMOS (spatial resolution of about 40km)... However, a number of authors have shown that low resolution products can correlate quite well with local in situ soil moisture observations.

Interactive comment on Biogeosciences Discuss., 9, 3615, 2012.