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Interactive comment on "Mapping spatially-temporally continuous shortwave albedo for global land surface from MODIS data" *by* N. Liu et al.

Anonymous Referee #2

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1 GENERAL COMMENTS

The manuscript presents a method to post-process surface albedo retrievals to get harmonized, global daily estimates of satellite based surface albedo. The method is based on a statistical calibration of a smoothing kernel that is applied to surface albedo retrievals with gaps. In general the paper is of interest for the land surface remote sensing community. While the method itself is described with sufficient detail in the paper, the presentation of the actual results of the method is rather poor. I therefore recommend major revisions of the manuscript. My comments are detailed below.

2 MAJOR COMMENTS

1. section 2: The various data sources are not clear. Authors use MODIS surface albedo data for the calibration of their correction method. But it is not clear at all, how the original GLASS surface albedo is estimated. Some references are given, but no satellite or sensor is mentioned. Further clarification is needed here.

2. eq. (1): the residual term $\epsilon_{\Delta k}$ is missing in the equation and only mentioned in the text

3. p9048,L5: The authors assume that the PDFs of the albedo conditioned by the retrieval and conditioned by the climatological prior are independent. I think that this assumption is not valid, as the retrieved albedo and climatological mean albedo should be highly correlated with each other. The authors need to clarify this point and assess the impact of this assumption on the retrieval method.

4. The method proposed by the authors is based on a statistical calibration of the relationship between the albedo of day k and day $k+\Delta k$. In addition, they use a climatological mean prior for further constraining the albedo retrieval in a Bayesian framework. In the end, they estimate an albedo vaue for each day together with its uncertainty. In section 3, the details of the method are outlined. In general I was missing any cross reference to variational parameter estimation methods in the paper. In general, a variational method would be the appropriate method for constraining such a multidimensional statistical problem like the one adressed in the paper. The authors need to clarify in which sense their method is comparable or different to classical variational approaches which are minimizing a cost function

5. The general assumption of the manuscript is that there is stable correlation between albedo of day k and day $k + \Delta k$. This assumption is valid and the authors

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show empirically from the data that strong correlations exist. I was however expecting that the authors would be able to estimate some characteristic time scales of the surface albedo. In a variational optimization scheme, one would take the decorrelation of albedo in time into account using e.g. an exponential term like $exp(-\tau t)$. Is τ assumed to be constant in the mansucript? I guess so, as correlations between k and $k + \Delta k$ are calculated with same lags. If so, is this really a valid assumption? Temporal decorrelation of albedo should be faster in spring or autumn than during the peak of the vegetation season. The authors are asked to more critically discuss the assumptions made in their manuscript in that sense and to show whether a constant lag is a sufficient approach.

6. Spatial resolution: The authors aim for a 1km, daily surface albedo product. The prior information they are using is however based on a 5km resolution (p.9050,L1) dataset. Why do they use 5km data, when MODIS is available at higher spatial resolutions? How is the discrepancy in spatial scales considered in the retrieval approach?

7. The authors provide in Table 4 a statistical comparison of the different albedo products. For this comparison, they provide linear correlation measures for zonal mean values. The found correlation coefficients are very high. Zonal means are a first step to compare the different products, but proper approach would show results on a pixel level basis. The authors can easily provide correlation maps, slope/intercept maps and RMSE maps based on the data they have. I guess that this will show a more heterogeneous picture, indicating areas where the different products agree and disagree. I sugest to replace the zonal mean analysis by a more spatially discretized presentation (maps).

8. p9051,L7: The authors motivate the usage of MODIS MCD43B3 product by its *great stability*. In fact my personal experience shows that the MODIS surface albedo product is actually not stable in time and contains a lot of rapid changes

in the signal. The signal to noise ratio is rather low in the timeseries. One can easily see this when looking e.g. on the MODIS subset website for some temporal surface albedo profiles (http://daac.ornl.gov/MODIS/). What might be the impact of noisy input to the stability of the algorithm developed by the authors?

9. Merit?: After reading the paper, the reader is left with the question of the scientific merit of the method. The mansucript is rather poor in providing a proper validation of the results and differences to existing products. The authors basically show that their method is capable of filling the gaps, but it is not clear at all from the manuscript where the majority of additional information is coming from. The authors have a framework that allows to quantify the impact of the prior and the actual retrievals on the posterior albedo estimates. I would expect the authors to ellaborate more clearly, in which regions/times the information deviates from the prior or not. Further, I would expect a more thorough validation of the final results which goes far beyond the timeseries shown in Fig.3. To provide a proper justification for the method suggested in the paper, I would expect that the authors show if their method is superior compared to some standard techniques like e.g. Savitzky-Golay filters.

3 MINOR COMMENTS

- Abstract: The abstract contains a lot of acronyms like GLASS or GLASS02A2x ... which are not understandable by the reader which has not read the paper. It is suggested to revise the abstract and avoid acronyms where possible or introduce them already within the abstract.
- p9045,L4: GCOS2006 is an outdated reference. The most recent GCOS supplement on ECVs is from 2011 (see GCOS website); update needed

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- Table1: Data gaps are given in percentage, but the temporal baseline is not clear. Is it days/days or 8-days/8-days? Clarification is needed here
- Fig.2: This figure is not very informative. It basically shows the filling of the datagaps using the suggested method. I recommend to include additional panels showing the absolute and relative differences between the two data products where applicable.
- eq.6: details on the meaning of δd and λ are missing in the text

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 9043, 2012.