Biogeosciences Discuss., 6, C3769–C3772, 2009
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Biogeosciences Discussions

Interactive comment on "Upscaling of gross ecosystem production to the landscape scale using multi-temporal Landsat images, eddy covariance measurements and a footprint model" *by* B. Chen et al.

Anonymous Referee #1

Received and published: 30 December 2009

General comments

The authors announce an upscaling framework to produce landscape scale flux estimates for GPP. Generally speaking, this subject is relevant and should be interesting for the readers of Biogeosciences, since it would enhance our capabilities to make use of the growing eddy-covariance databases, and at the same time improve validation of remote sensing products, However, what the authors actually do is simply compare eddy-covariance fluxes with spatially distributed flux estimates based on Landsat data, using an Eulerian flux footprint model to connect both data sources. All elements in-

volved in their framework, i.e. the eddy-covariance dataset, the footprint model, and the algorithms to derive remote sensing GPP, have been published elsewhere.

My main point of criticism concerns the poor presentation of the results, and the neglect of uncertainties. The 'effectiveness' of their method is demonstrated in 2 graphs and 4 numbers. These absolute numbers on annually averaged GPP are meaningless without uncertainty ranges. Differences between footprint and 'equal' integration are minimal, and no residuals or other statistical properties are given to indicate the goodness of the fit. As briefly discussed by the authors, the seasonal dynamics are not well met, indicating that their optimized model version might as well get a closer annual mean for a completely wrong reasons. The authors mention a couple of important uncertainty sources in the discussions section, but why don't they treat them explicitly in a quantitative way? How reliable are those results if you consider footprint uncertainty, corrections to be included in the remote sensing data, representativeness of the tower-based air temperature data in spatial mode, etc? And what about the very simple approach to split NEE into GPP and RE, and the eddy-covariance flux estimates overall? I'm certain that if all those factors would be included into this analysis, the differences between footprint and equal integration would be dwarfed by the associated uncertainties.

Besides this most important shortcoming, there is a number of minor items that contribute to the rather poor quality of this publication (see specific comments below). None of the elements involved in the presented framework is original, and it does not appear that the pointed out differences between footprint and equal integration are significant when uncertainties are considered. Therefore, I see only two valid result aspects in the current version of this manuscript: (i) the authors demonstrate a fair match between eddy-covariance fluxes and remote sensing products for GPP; and (ii) there are no significant differences between the footprint and equal integration and the use of a single Landsat pixel for the given site. And even for these aspects, uncertainties are not given, and important parts of the methodologies remain unclear in the text. I

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6, C3769–C3772, 2009

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therefore recommend to reject this manuscript from publication in BioGeoSciences.

Specific comments

- the title is misleading: There is no upscaling in this manuscript, just a comparison of remote sensing GPP vs. eddy-covariance GPP involving a footprint model

- There is a lot of talk about MODIS data, including detailed specifications, in the introduction (e,g, p11320, ll.18ff), but it is never used in the methods described. So either the descriptions are not relevant, or the authors should make clear where this data source is used in their approach.

- The authors mention scales larger than landscape at several places, but it is never clear how the presented work relates to larger scale processes, or how they plan to get there in future work.

- Based on the outline of the 'upscaling' framework (Section 2.3), steps iii) and iv) remain either undocumented (which parameters were optimized, and how?) or not treated at all (there's no upscaling to larger scales, or comparison to MODIS, in this paper).

- Concerning optimization, why do the 3 parameters in the Tm function remain static, even though the authors even mention in the discussion section that these might be responsible for the seasonally varying offsets? Why was no sensitivity study conducted for these parameters?

- The first paragraph of the discussions section it totally out of place, since this paper only treats remote sensing data as 'static' input, i.e. the authors neither develop new algorithms nor test existing ones for their accuracy.

- The Landsat datasets used for this study need to be better documented. What about data gaps, the influence of clouds, atmospheric corrections? Since you present continuous time series in Figure 7, I suppose there must be some interpolation algorithm to fill gaps.

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6, C3769–C3772, 2009

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Interactive comment on Biogeosciences Discuss., 6, 11317, 2009.