Responses to comments of Referee #1

General comments
Question (Q)
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 involved in their framework, i.e. the eddy-covariance dataset, the footprint model, and the algorithms to derive remote sensing GPP, have been published elsewhere.

Answer (A)
A major revision has been made. All the referee's concerns have been addressed in the revised version. The revised manuscript really enhance our capabilities to make use of the growing eddy-covariance databases, and at the same time improve validation of remote sensing products in term of accurately estimating landscape/regional GPP. *See page 7 lines 5-17.*

Q:
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.

A:
The presentation of the results in the revision were reorganised and significantly improved. All associated uncertainties and biases were quantitatively assessed. These biases include EC measurements and C flux partitioning (*see Section 2.2.2, page 9 line 18 –page 11 line 21*), the time-series data of vegetation indices from LANDSAT satellite images (*see Section 2.3.3, page 13 and page 15 lines 9-14*), the VPM model's

inputs (*see page 27 lines 5-10*) and footprint modeling (*see page 25 line 14 – page 26 line 15*). The modeling results were statistically assessed and quantitatively compared with EC-derived GPP and the MODIS products (*see Section 3.5, page 20 line 18 – page 21 line 13 and Section 4.3, page 22 line 3 – page 23 line 20*).

Q:
Besides this most important shortcoming, there are 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 therefore recommend to reject this manuscript from publication in BioGeoSciences.
A:
The revised version is significantly different from the previous version. The shortcoming pointed out here has been overcome. The revised manuscript focuses on the upscaling based on Landsat data and data-model assimilation. The issue on the tower location biases has been removed. We didn't compare the differences between the footprint weighted and equally integrated GPP in the new version.

Q:
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.
A:
The title was changed and the new title matches its content.

Q:
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.
A:
In the revised version, MODIS data were really included and the data sources are given clearly (*see Section 2.3.1, page 12 lines 3-9*).

Q:
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.

A:

The developed upscaling algorithm was verified in the EC-tower footprint area and applied to a large area of 30 km × 30 km.

Q:

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?

A:

The upscaling framework was presented clearly in the revised version. *See Section 3*. Nine parameters were optimised (*see Table 3*) and the parameters were allowed to vary seasonally.

Q:

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.

A:

In the optimized modeling scenario, the model parameters and inputs varied seasonally in stead of "static", so the first paragraph of the discussions is required.

Q**:**

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.

A:

The remote sensing data and data processing were given in Section 2.3 in the revised version.