

Interactive
Comment

***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 #2

Received and published: 22 February 2010

General comments:

The topic of the paper is quite important for reliable quantification of carbon budgets on regional to continental scales, i.e. scales corresponding to those of climate anomalies as well as scales relevant for political reasons. The stated objective of the paper is to assess the performance of vegetation indices to predict seasonal fluxes and to develop an upscaling approach. However, the paper does not show that those objectives have been achieved. I have to second the first reviewer in saying that for this, a proper assessment of model performance is required, that specifically addresses

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the capability of the model to capture seasonal dynamics as well as to capture spatial gradients, using appropriate statistical measures. The analysis should go further than the current manuscript: using the eddy covariance data, diagnostic model parameters should be optimized, and the impact of using footprint weighting should be objectively assessed. If there is no significant difference, there is no real justification in using the more complex high resolution model, at least for the investigated site. To justify the term “upscaling”, multiple sites at different locations need to be included. I would therefore recommend to reject the paper from publication.

Minor comments:

The term model-data-fusion is usually used for approaches that combine different data streams in a quantitative way with the aim of constraining unknown (or less well known) parameters. It is unclear which parameters have been optimized in this study. The authors show a comparison of a diagnostic model for GPP (weighted by footprints) with EC derived GPP, but a simple comparison is not a model-data-fusion.

The statement “The footprint integrated GPP values were closer to EC derived GPP values than the “equally” integrated GPP and the tower pixel’s GPP values though their differences were small” seems not justified without any uncertainty estimate or statistical information. How significant are the claimed differences? For the annual mean, the modeled GPP for the Tower pixel seems to agree better with the EC derived GPP than the footprint weighted modeled GPP. This questions the whole approach of using footprint weighted averages.

The description of the diagnostic model seems to indicate that most of the model is very similar to the VPM model (Xiao et al., 2004). It should be clearly state what the differences to the VPM are.

Footprint: it is unclear if the cumulated bi-weekly values includes day and night time periods. For GPP at least one should not include night time footprints.

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Some detailed comments:

Pg 11320, ln 11: remove comma in “it has been proved that, it is an extremely”

Pg 11322 ln 8: replace “the result of unique southeast monsoon” by “the result of a unique southeast monsoon”

Pg 11322 ln 9: the numbers given for slopes should have units

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

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