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Biogeosciences Discussions

Interactive comment on "Consistent assimilation of MERIS FAPAR and atmospheric CO_2 into a terrestrial vegetation model and interactive mission benefit analysis" *by* T. Kaminski et al.

Anonymous Referee #3

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In this paper the authors describe the simultaneous assimilation of of atmospheric CO2 data from flask measurements and satellite derived estimates of the fraction of absorbed photosynthetically active radiation (fAPAR) in a model of terrestrial vegetation. In addition the authors present a tool for interactive mission benefit analysis. On balance I think this is a good paper that makes some interesting points, but that it's novelty is limited: it provides some new technical insights compared to recent papers from the same group, but no new real science as such.

The paper is very well written and I only have a couple of minor notes about the clarity of the text (see below). Figures are well presented and appropriate and the authors

have done a good job of conveying some quite technical concepts. I think this paper is fit for publication with only minor modification but have a few suggestions that the authors may wish to consider.

On the first paragraph of page 10768 the authors justify equating the modelled and satellite fAPAR on the basis that it results in the same amount of energy entering the photosynthetic mechanism of the plant. I accept this point but I think there are other knock-on effects that need to be considered. Leaf area index (LAI) controls variables other than fAPAR inside models, for example it provides the total surface area available for gas exchange. Presumably assimilating fAPAR adjusts LAI in some way (for example by adjusting parameters that control allocation to leaves)? The resulting LAI will be an effective one that gives the correct fAPAR, but I think it will result in biases elsewhere; perhaps in transpiration. I don't think this point needs exploring in detail in this manuscript, but I would be grateful if the authors could include and additional sentence or two at the end of the paragraph stating whether or not they expect such biases to be induced by this assumption.

On page 10771 the authors refer to "the in situ network of the carbon cycle." I suspect this should be the "Infrastructure for Measurements of the European Carbon Cycle." Please check and amend if required.

Page 10773, site-scale assimilation: this seems a little out of place to me. I'm not sure what it brings to this paper, and it is not clear that there is anything new beyond the Knorr et al. 2010 paper. I apologise if I've missed the point here, but I think this section could probably be removed.

Page 10774: Scaling by averaging fAPAR over a grid cell weighted by areal proportion of the PFT. It seems to me that a better approach here would be to generate average fAPAR for each PFT for each model grid cell and then assimilate the same information from the satellite data, which could be averaged using a map of the PFTs. Has this been tried? If not, would it be feasible to do this in this paper or do the model runs take

to long? I would be genuinely interested to know if it improved results.

Page 10776, around line 20. Should there be a cross reference to Figure 8 somewhere here?

Page 10776+: Mission benefit analysis. This is in many ways one of the most interesting sections of the paper but there is not much discussion of the results. I think the manuscript would benefit from a bit more discussion here. In the conclusions you make the point that sensor resolution is not critical for data assimilation, but presumably this is a very model specific result?

Interactive comment on Biogeosciences Discuss., 8, 10761, 2011.