Biogeosciences Discuss., 11, C7313–C7315, 2014
www.biogeosciences-discuss.net/11/C7313/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Thermal-based modeling of coupled carbon, water and energy fluxes using nominal light use efficiencies constrained by leaf chlorophyll observations" *by* M. A. Schull et al.

Anonymous Referee #3

Received and published: 11 December 2014

The authors report on a new parameterisation of a key parameter, the nominal LUE (beta_n) for the coupled energy balance carbon cycle model TSEB-LUE. Overall the work is very solid: the authors are able to rely on an impressive set of field data (eddy covariance flux measurements, ancillary meteorological data, biophysical data, ...) from four field sites (rainfed/irrigated maize/soybean); the paper is well written, the presentation is solid; discussion and conclusions are appropriately based on the results.

Having said this, the paper still left me somewhat unsatisfied as the major finding is actually incremental: when replacing the constant beta_n parameter with the new parameterisation (which is a function of time-varying leaf chlorophyll content), the authors find

that the canopy photosynthesis simulations (and less so evapotranspiration) improve. This, in my view, is not surprising as the new model has more degrees of freedom – the authors would have been able to achieve the same results simply by fitting a polynomial to the residuals. The latter (provocative) comment is of course stupid, as the novel aspect of this study is that the authors are able to relate changes in beta_n over time to changes in the leaf chlorophyll content which, in theory, enables remote estimation of beta_n. I suggest the authors to further work on this innovative aspect of their study in order to make this a more significant paper.

To this end I have the following suggestions: (i) To me it is striking that, despite differences between rainfed/irrigated maize/soybean, the same relationship (Fig. 4) can be used (although separate relationships were not explored). This merits further analysis. The authors discuss that differences in canopy structure (leaf angle distribution, planting density) may be responsible for the observed deviations from the fitted line. This would be an area that would merit further analysis to explore the hypothesis made using for example a mathematical model of canopy radiative transfer and leaf photosynthesis. Possibly, the structural differences between the different canopies could be accounted for, making the relationship more universal. On a plant physiological ground the convergence between a C3 and C4 plant to the same relationship merits further discussion as well. (ii) The chlorophyll content measurements were inferred from hyperspectral reflectance measurements at the leaf level, calibrated against chlorophyll extractions, which were scaled up to the canopy level. When TSEB-LUE is driven only by remote sensing data, the question arises on the relationship between the up-scaled leaf level data used in this study and corresponding RS measurements. This is something that the authors at least should address in the discussion/conclusion. Possibly, remote sensing of the chlorophyll content may introduce uncertainty into the estimation of beta_n which negates the advantage of the proposed parameterisation (e.g. would further reduce the R^2 in Fig. 4). This is in particular an issue as any chlorophyll content inferred from RS will have a "canopy structure" effect, similar to the author's arguments regarding variability in Fig. 4. A great addition would be hyperspectral ecosystem-scale data from field spectrometry or airborne remote sensing to actually demonstrate this effect.

Minor comments: p. 14135, l. 17-19: in the equation Re however has a positive sign

---

Interactive comment on Biogeosciences Discuss., 11, 14133, 2014.