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Geoscientific
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Discussions

Interactive comment on "A comprehensive set of benchmark tests for a land surface model of simultaneous fluxes of water and carbon at both the global and seasonal scale" *by* E. Blyth et al.

Anonymous Referee #1

Received and published: 2 December 2010

The manuscript by Blyth et al. describes the evaluation of a land surface model (JULES) using multiple carbon cycle and hydrology datasets. The authors carefully chose a suite of datasets that span multiple biomes and allow for the diagnosis of regional biases in surface energy exchange and ecosystem processes. The paper is concisely written and will be of broad interest to the geosciences modeling community. I thought particularly novel and interesting elements of the manuscript included the way the authors combined carbon and water diagnostics and the way they used basin-scale observations of runoff to constrain the balance between precipitation and evapotranspiration.

My suggestions for improvement focus on two themes. The first is that the authors be more precise in the language and description of some of the different tests. This may require a not insignificant amount of work on the part of the authors. The second is that the authors provide the reader with more information about their model system and the means by which they made the comparisons described in the paper. Most of these suggestions are related to specific points described below.

Title: It wasn't clear to this reviewer whether the final clause "at both the global and seasonal scale" was necessary.

Abstract: Please consider providing a broader motivating sentence at the beginning. Also, please define more precisely what atmospheric CO2 data you used in your analysis. Were these the flask observations from the NOAA Global Monitoring Division network, the merged data from the NOAA GlobalView system, CSIRO, or another set? It also may be possible to merge the two paragraphs here together.

In the introduction, other relevant carbon-water cycle interactions that may be worth reviewing include the impacts of deforestation on precipitation and climate in the Amazon, and more broadly the impacts of forest cover change at different latitudes on land surface temperature, as modulated through impacts on ET and cloud albedo (e.g. Bala, Caldeira et al.).

The Carbon Land Model Intercomparison Project as described by Randerson et al. (2009) is independent from the Cadule et al. (2009) analysis of coupled model performance. In the sentence as currently written, this issue is somewhat ambiguous.

Please define all the acronyms. I did not know for example, what GSWP2 meant.

Section 2.1. What set of years did you use to construct mean annual cycles from the Fluxnet data? Please provide this information to the reader. (Sorry if I missed it.). You could also add this to each site as a column in Table 1.

Section 2.2. It was unclear to this reviewer what atmospheric transport model you used to compare the JULES fluxes to the atmospheric monitoring stations (or if it was internal from an atmospheric coupling with the Unified model). If it was the one referenced by Kaminski et al., it is important to note if you used the adjoint function directly or if you used the forward model, and if so with what set of winds. Perhaps also change the text to use the active voice to make it clearer to the reader that you are using the Kaminski model. In this context, it may be worth reminding the reviewer how this model did in TRANSCOM with respect to the simulation of the annual cycle. If it is from the coupled Hadley model, were the winds from reanalysis or GCM derived? Much more detail is needed to allow the reader to carefully evaluate whether the approach taken by the authors is state-of-the-art and whether it represents a strong or weak constraint, given known uncertainties and biases with the representation of mixing processes in atmospheric models (e.g. Yang et al. 2007, Stephens et al. 2007).

It is crucial, if comparisons are being made with stations in the Southern Hemisphere, that the authors include fluxes from ocean exchange and fossil fuel emissions as these emissions can significantly change the amplitude and phase of the seasonal cycle (e.g., Randerson et al., 1997).

Also, for the atmospheric CO2 comparisons, the units in the current manuscript are in concentration (moles per meterˆ3). If at all possible, please consider converting the units to dry air mixing ratios (the widely adopted international standard for measuring atmospheric trace gases). For CO2, mixing ratios are commonly expressed as parts per million (ppm).

It was a concern to this reviewer that the ASC station had a seasonal amplitude that was larger than that observed at Point Barrow, Alaska. This is unusual and the authors may wish to investigate the causes of this (and explain this to the reader).

Section 2.2. What set of years did you use to construct mean annual cycles from the atmospheric CO2 mixing ratio data? Please provide this information to the reader. The amplitude of the annual cycle has been changing over time (e.g. Piao et al. Nature).

Section 2.3. Please provide the reader with more detailed information (including a ftp or http site), a version, and a reference for the GRDC river runoff dataset.

Section 2.5. Please provide the reader more information on how, exactly, the different PFTs from Foley were aggregated into the PFTs used by the JULES model.

In Table 2, it may be worth adding a footnote to clarify for the reader that the metric is the monthly mean RMSE for this version of JULES with the observations, or something of similar effect.

For figure 2, the authors may wish to express ET in units of mm/day or a similar unit, or potentially change ET to latent heat, which seems more commonly used with units of energy.

The units for figures 3 and 4 need adjusting from millimols to micromols. This is probably a simple error from the plotting package not being able to handle Greek symbols.

Interactive comment on Geosci. Model Dev. Discuss., 3, 1829, 2010.