Biogeosciences Discuss.,
doi:10.5194/bg-2016-142-RC2, 2016

Biogeosciences
Discussions

Interactive comment on "Evaluation of modeled global carbon dynamics: analysis based on global carbon flux and above-ground biomass data" *by* Bao - Lin Xue et al.

Anonymous Referee #2

Received and published: 23 May 2016

Though I would like to be encouraging of work in the general direction of confronting ecosystem process models with emerging data, unfortunately this effort does not provide a good example. There are many problems with this work including many detailed below, but the biggest problem is that the findings, interpretations, and conclusions are not at all supported by the work that has been done (see V. below).

I. Model Calibration Is Not Described: The paper suggests that it performs a model calibration but there is no information on this. A set of model (IBIS) parameters are apparently calibrated with flux tower data on GPP and ET from select sites and with plot-level aboveground biomass data. However, there is no description of the model calibration, and no parameter uncertainty or parameter correlation (equifinality) analy-

sis.

II. Data Sources Are Not Disclosed: The paper does not cite its data source(s) for the plot-level aboveground biomass dataset that it apparently used for calibration (though maybe just for evaluation?). It is suggested that most of the data come from China, though the Figure S1 shows a broad global distribution. Individual citations for all data sources must be provided, and the methods must explain the methods of data collection for each of those sources. It's inadequate to simply say "from the literature" and show a map of locations.

III. Model SetUp Incompletely Described: There is no description of the modeling procedure. Was a spin-up performed to bring carbon pools to some equilibrium state? How were PFTs assigned to grid cells, and/or does the model simulate PFT distributions that match with the other datasets? There is a risk of the wrong PFTs being simulated, for example where land use has substantially altered the PFT from a model-estimated dominant PFT (e.g. if deforestation removed trees with grasses, crops, or savanna instead).

IV. Model Evaluation (Simply Comparing to Data) Does Not Go Far Enough: This paper's main point is that new datasets need to be used to confront models and improve them. However, the paper offers nothing to improve the model that is used. Discrepancies are shown but there is no new insight about why, or how the model structure or parameters would best be modified to come to resolution with the data, where appropriate.

V. Findings and Conclusions Do Not Follow from Results and Do Not Advance Science in a Useful Way: The paper purports to show the following but each is poorly substantiated if at all.

1) Claim: Results of a DGVM can be sensitive to the meteorological driver data that are used but that parameter uncertainties are more important. Concern: This is known, and in fact is not precisely shown here. The paper does not compare sensitivity to

parameter values in any way and thus cannot make this claim.

2) Claim: Bias or error in GPP caused by meterological data can be transferred to AGB carbon stock. Concern: This is already known, and in fact is not precisely shown here (paper does not show that GPP bias or error relates to AGB bias or error).

3) Claim: To improve model accuracy, modelers should pay attention to both model parameter calibration and meterological drivers, with a focus on the former. Concern: This is known, and again, is not evidenced by anything in the present study.

4) Claim: DGVMs are useful tools for simulation of regional- and global-scale carbon dynamics. Concern: No doubt they are but this is not a conclusion of the study.

5) Claim: Discrepances were observed between model-derived and observed spatial patterns of AGB for Amazonian forests, mainly because of the unique parameter set used in the model. Concern: Only a single parameter set was tested so you cannot claim that that is the source of the mismatch. Model structure could be a source of mismatch. Meteorological driver data could too. Nothing presented supports this claim.

6) Claim: The conclusions of our research highlight the necessity of considering heterogeneity of key model physiological parameters in modeling global AGB. Concern: This is already well established and not at all demonstrated by the present study.

7) Claim: The research also shows that to simulate large-scale carbon dynamics, both carbon flux and AGB data are necessary to constrain the model. Concern: There is nothing here to support this claim. The study does constrain with C flux only, with AGB data only, and then with both to show that both are needed to recover key metrics of carbon dynamics. This claim is another throw away with no substance in the current paper.

VI. It is Unclear Why FLUXNET Upscaled Product Is Newly Estimated: The calibrated model is then compared against a flux-tower upscaled GPP and ET product (Jung et al. 2011) but was actually re-estimated here for some unknown reason, and came up

with substantially different results.

VII. Study Involves a New Phenology Model That is Untested with No Evaluation: When you introduce a new model component such as the phenology model used here it is fitting to evaluate if that model component performs well compared to data. This is, in fact, part of the point of the paper, however the idea seems to have been missed with respect to this paper's new implementation of the phenology component in IBIS.

VIII. This paper does not appear to adhere to the FLUXNET Data Fair Use Policy. It does not cite the appropriate papers and does not include appropriate acknowledgement.

IX. References missing: e.g. Stockli et al. 2008 is not in the references list, maybe others.

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