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Interactive comment on "Field data to benchmark the carbon-cycle models for tropical forests" *by* Deborah A. Clark et al.

Deborah A. Clark et al.

deborahanneclark@gmail.com

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Authors' responses to comments of Referee #1 (21 July 2017):

We much appreciate this referee's constructive and thoughtful comments. Below we have pasted in the entire review, and we have inserted our responses to the suggestions and questions (indicated by bracketing stars).

Interactive comment on "Field data to benchmark the carbon-cycle models for tropical forests" by Deborah A. Clark et al. Anonymous Referee #1

Review The article presents a critical review of the currently available data to test the land- surface and vegetation/carbon dynamic components of Earth System Models in lowland tropical forests. After introducing general criteria for ïňĄeld data to be useful

and trustable with regards to benchmark model results (Section 2 and 3), the authors reviewed the available data and associated issues in terms of standing carbon pools (LAI, aboveground and belowground biomass pools, soil organic carbon), ecosystem ïňĆuxes (NEE, GPP, respiration, NPP, litterfall) and tree mortality. A couple of ïňĄnal sections mentioned ecosystem C-ïňĆuxes sensitivity to climatic trends and availability of local meteorological conditions. The article is very well-written and touches on a sensitive topic of interest for the ïňĄeld experimentalist and modeling communities, bringing upfront many issues that are well-known but not explicitly written in a scientiïňĄc manuscript. It is a review, therefore does not introduce any new methods or dataset but it clearly frames the picture of data availability for model comparison in the tropics. Therefore, the article type should be "Reviews and syntheses" and not "Research article".

We agree.

My knowledge of the empirical literature of tropical forest is forcefully partial but I have the impression the authors are including most of what I am aware of in their discussion, even though the reference selection is by far not exhaustive, especially for the ïňĆuw-tower data. In any case, I strongly agree with almost everything is written in the manuscript and I have mostly suggestions on points to stress or minor comments as listed below. What clearly emerges from the manuscript is a gloomy picture of data availability to benchmark model in lowland tropical forests that let me wonder if the correct title should rather be "Field data to benchmark the carbon-cycle models for tropical forests are mostly lacking". Unfortunately, such unsatisfactory amount of data availability corresponds to reality; we do not have almost anything to compare models with at the landscape scale in the tropics. This remarks the challenge of collecting model-meaningful data or add reasonable uncertainty bounds to the current available data in the tropics.

It further suggests that some of the model-to-data comparisons carried out in the past for tropical forests might have compared apples to oranges or that the conïňĄdence given to certain type of “observed” data (e.g., GPP or aboveground wood production) in previous articles is unjustiïňĄed. This criticism, which I completely share, is evident throughout the article but it is never really made explicit and probably can be reinforced in a revised version.

We are happy to add explicit statements to reinforce the points in the paper about this problem of inappropriate comparisons in past model-to-data evaluation studies.

Additionally, I suggest reinforcing a few other points.

(i) It is extremely important collecting sub-daily resolution meteorological data for a number of variables simply to run the models. Practically, for many experimental sites, these data are missing and modelers have to rely on “re-analysis forcing” introducing additional discrepancies in the model-to-data comparison.

We very much agree with this point, and would be happy to add a strong statement about it to the conclusions section.

(ii) Analysis of climatic sensitivities as the one reported in Table 12 (results from Clark et al 2013) are fundamental because they allow to test if the mechanistic nature of the model is able to capture the correct direction and magnitude of a given response and are typically less subjected to local biases than matching a given carbon pool or an uncertain ïňĆux.

We agree, and would be happy to add a strong statement about it in the conclusions section and abstract.

(iii) Given the paucity of data and their uncertainties, there should be some clear statement about the weakness of automatic calibration or to “force” models to reproduce as close as possible speciïňĄc observations or set of observations (e.g., eddy covariance ïňĆuxes). With all the issues described in this manuscript, it is very unlikely that we are able to constrain several of model parameters using current data. In other words, there should be an effort from modelers in using observations very critically and not blindly

and from experimentalists in communicating properly the limitations of measurements and accept model estimates (which are, at least, constrained by mass and energy conservation) critically and not simply as “wrong numbers.

We agree. We will edit as appropriate to reinforce this point.

Minor Comments

Page 2, Line 1-4 and elsewhere. I wonder if the article really needs these citations at the beginning of sections. I ïňĄnd a bit unconventional for a review paper and the main message of the citation can be or is already embedded in the main text.

We feel the value of including these quotations lies in explicitly presenting current thinking of the research community with respect to the central points of the adjacent section. Just citing those papers in the text would not provide this context so clearly, especially for those readers less familiar with those publications.

Page 2 and Line 10-13. There are also studies that attributes a large part of the variability of the land CO_2 sink to semi-arid ecosystems (Ahlstrom et al 2015). Maybe it can be mentioned together with the role of tropical forest.

Lines 10-13 address specifically the negative relation between tropical temperatures and the atmospherically-inferred C balance of the land tropics in toto (i.e., the joint effect of tropical forests, tropical semi-arid systems, agricultural systems, etc.). We then turn to questions directly related to tropical forests, the focus of the paper. We feel that remaining focused on tropical forests would be more effective than discussing here the different conclusions of recent studies inferring the contribution of semi-arid systems to the land C balance.

Page 5 – Line 5-6. It is a quite trivial statement that the most meaningful variables to compare with are the ones directly observed. At the same time, it is also true that there is some value in comparing model simulations with variables, which are somehow inferred from ïňĄeld observations, even though not directly observed. I would make this a bit more nuanced.

We agree that partially-extrapolated or otherwise inferred indirect estimates can have heuristic value, and we will attempt to express this more explicitly; however, the focus of this paper is benchmark-level field data, those providing the most solid possible standards for guiding the models.

Page 8. Line 14-15. I completely agree on the importance of capturing interannual variability and long-term trends, ultimately this is what we are really interested in, at the same time, it is important to understand the mechanisms leading to these trends/variability, otherwise we risk that models are forced to reproduce something for the wrong reason or through the wrong process.

We agree.

Page 9. Line 1. “high-resolution local meteorological data” are simply fundamental. For instance, many or almost all the RAINFOR plots will be impossible to simulate properly with models because meteorological data are not available or are not properly released.

We agree (and, as noted earlier, we would be happy to reinforce this point in the conclusions section).

Table 1. While there are not estimates for tropical forest, plant-C export to mycorrhizal and root exudates are typically thought to be at the maximum 10-15%. Maybe calling it a “large fraction” is a bit excessive and subjective statement.

We would be happy to change the comment for each of these C fluxes in Table 1 from "Unquantified in tropical forest; possibly a large and increasing fraction of NPP" to "Unquantified; possibly a non-trivial and/or increasing NPP fraction".

Table 7. The estimate for VOC: 10-90 gC/m-2 yr-1 seem to me too large (almost one order of magnitude), when compared to other estimates in the tropics (Kuhm et al 2007) and generally to the expected mass contribution of VOC (Keenan et al 2009)

Per Guenther et al.’s 1995 (JGR-Atmospheres, 100:8873-8892) model of tropical rain forest total VOC emissions (isoprene, monoterpenes, other reactive VOC [ORVOC], and other VOC [OVOC]), annual emissions of all these classes of VOC combined are estimated at 31 g C m-2 (from their Table 1). They state that "...estimates of annual VOC fluxes from tropical forests are as high as 75 g C m-2." They also conclude the uncertainties exceed a factor of 3. We base our guesstimates in that table on these numbers, which include additional VOC’s (beyond isoprenes and monoterpenes).

Page 21. Leaf – litterfall. One point, I would made explicit is that litterfall estimates should be coherent with the leaf turnover rates and the product of average “leaf mass area” [gC/m2 LAI] and LAI [m2 LAI/m2 ground] observed in a given site. My experience, from published observations, not only on the tropics, is that this is rarely the case. Probably, this is the result of the problems you mentioned.

Such a cross check would indeed be excellent in any system where the two datatypes are available. Unfortunately, we know of no tropical forest with direct observations for estimating the whole-forest leaf turnover rate (an appropriately-weighted composite of the mean leaf turnover rates for the understory, mid-storey, and canopy). Leaf longevities in the understory of tropical moist/wet forests have been found to exceed several years, while leaf longevities in the canopy can vary from more than 1 yr to several months, depending on the tree/liana species.

Page 21. Line 26-27, see also Wu et al 2016, for the link between leaf-production and litterfall, even though not completely synchronous.

That very interesting study used ground-based observations through time of the "% of individuals with leaves < 4 mo old", which varied between ca. 25% and 50%, as a surrogate for leaf production. Although providing a useful seasonal index related to leaf phenology, this metric would not enable quantification of forest-level leaf-production or its exact phenology through time.

Page 24. Line 2-10. I would still mention that some observations of ïňĄne root production even though sub-optimal is very important, if it is not used blindly in models.

We agree. As we state at the outset of the fine-root production section, when observations are highly replicated at the landscape scale, they provide a useful lower bound (lower bound, because they are confined to the surface soil and also involved low-bias from the methods issues). We will reinforce this point in the text.

Page 25. Line 12-28. See also Gloor et al 2009.

We agree this reference is a good addition to the citations here.

Page 29. Line 21-22. I do not want to downplay the importance of C-Exudation and export to mycorrhizal, but with uncertainties in NPP and GPP estimates in the order of 20-30%, I would emphasize this aspect in the conclusions not only the missing components, which is likely smaller. In the conclusions, I would also suggest to emphasize more the temporal dynamics of pools and ïňĆuxes in line with the Section 3.3 and 4.5.

We agree that adding both these points would strengthen the final conclusions.

(end of author responses to comments by Referee #1)

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