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Biogeosciences Discussions

Interactive comment on "Terrestrial carbon sinks in the Brazilian Amazon and Cerrado region predicted from MODIS satellite data and ecosystem modeling" *by* C. Potter et al.

C. Potter et al.

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Referee #1 - Page 955 As the model is constructed (or at least explained in the text), long term Rh is equal to NPP (what goes in eventually goes out). Therefore it is the estimation of NPP that is ultimately the measure to be evaluated; it makes little sense to focus on the close agreement to Rh that happens to exist over the measurement period.

Author Reply ... This assumption by the Referee is incorrect. The only long-term pools that are initialized in the CASA model are (1) down coarse woody debris and (2) mineral soil carbon; both of these pools have mean residence times for carbon of about a decade in the humid tropical zones. However, the major fine leaf litter, microbial

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turnover, and fine root decomposition pools in the model are largely controlling monthly Rh-CO2 emission fluxes, and hence, total soil CO2 fluxes from these CASA pools vary substantially from year-to-year, and, of course, from month to month. It does make sense then to focus on both NPP and Rh measurements to evaluate the model in the Amazon region.

Referee #1 - As explained in the paper, it appears there is no scope for CO2 fertilisation of NPP in the model formulation, i.e. an long term shifts in NPP can only be driven by climate shifts. This is fine if correct, but should be explicity stated,

Author Reply ... This is true, but we consider future CO2 fertilisation effects to be inconsequential because the NPP algorithms in CASA are calibrated to current global estimates and we do not run the model into future years with elevated CO2 fluxes. We have now stated this in a revised version of the paper.

Referee #1 - A recent review of multiple field data from three LBA sites, including the data mentioned in this paper, (Malhi et al, published early online in Global Change Biology) suggests that the NPP at Tapajos is 1440+/-130 g C m 2 and at Manaus is 1010+/-140 g C m 2. This would suggest that CASA actually underestimates NPP. Model-data agreement on NPP should not be overstated.

Author Reply ... This statement by the Referee that our paper should have considered the review by Malhi et al. is not a defensible comment, because this review paper was not published until after we submitted our paper to BGD. Nevertheless, we stand firmly by our CASA model comparisons to the Tapajos tower flux measurements to refute the contention that CASA actually underestimates NPP at such sites.

Referee #1 - The most useful contribution of this paper is the exploration and spatial mapping if interannual variation in sources and sinks. Yet there is little mechanistic exploration of what is driving these spatial and temporal patterns in the results

Author Reply ... As recommended by the Referee, we have now added a systematic

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exploration of the monthly and interannual climate controls on NPP and NEP to the Results section of the revised paper. The overall conclusions were that both precipitation and latent heat flux strongly control carbon source (Rh) and sink (NPP) fluxes in zones south of 7 degrees S latitude, whereas latent heat fluxes were far more important controllers of NPP than was precipitation in zones north of 7degrees S latitude.

Referee #1 - Figure 2 is NEP, not NPP as the caption states

Author Reply ... We have corrected this typographical error in a revised version of the paper.

Referee #2 - The authors begin discussing the modeling approach on page 950 and describe how NPP is estimated using time-varying stress terms (lines 20-25), but it is unclear what the time-scale is for these terms. Since the model runs on a monthly time-scale, the reader is left to assume that these stress terms also vary on a monthly time-scale as well. If so, is a monthly time-scale sufficient to capture changes in C, H2O, and nutrient cycling processes to changes in temperature and precipitation? For example, there is some indication from more seasonal tropical forests of the Amazon Basin that litter decomposition can increase rapidly (ca. over 1-2 weeks) after the onset of the rainy season. Thus, a monthly time-scale may not capture some of these dynamics.

Author Reply ... The Referee makes a valid point, but this is one that we had already addressed in the original draft of the paper, specifically in association with the description of results presented in Figure 2.

Referee #2 - On page 954 the comparison between the model-estimated NEE and the tower NEE is presented (lines 1-20). First, the authors suggest that the tower data have relatively large uncertainties; and while this may be true, the authors point out that these data have been validated using a variety of methods including tracer studies and more traditional measurements of woody growth and biomass production. Thus, what are these uncertainties; and how might they affect the evaluation of model

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performance?

Author Reply ... Many uncertainties in the tower flux data are simply unknown to even the investigators of those original studies. Hence, we have altered the wording on our revised text to reflect that situation.

Referee #2 - Secondly, it is intriguing that the NPP increased during the dry season when reports from the Tapajos forest suggest that gross primary production (GPP) is not particularly sensitive to seasonal drought but tree growth actually declines in response to seasonal drought (Saleska et al. 2003). What is the mechanism for the increase in NPP during the dry season?

Author Reply ... The mechanism for an increase in NPP during the dry season is commonly thought to be the increase in daily solar irradiance (see Potter et al., 1998, for example). GPP may well be insensitive to these seasonal dynamics, because autotrophic respiration must be more constant to support large pools of standing woody biomass in Amazon forests.

Referee #2 - On page 956, line 10 change latitudes to longitudes

Author Reply ... This correction has been made in our revised version of the paper.

Referee #2 - The figures were very hard to read and could be substantially improved. For example, Fig. 2-4 could be larger?

Author Reply ... These several comments by Referee #2 about the size and clarity of our Figures seem to have misunderstood the intent of the journal. The format of this paper (as for all papers in Biogeosciences) is digital, so if a figure initially appears somewhat on the small side, the reader can simply zoom in and enlarge it to any size they desire. We have checked all the Figures in our paper again and found that they have all the detail necessary to zoom in many times to enlarge.

Interactive comment on Biogeosciences Discuss., 6, 947, 2009.