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Interactive comment on "The influence of soils on heterotrophic respiration exerts a strong control on net ecosystem productivity in seasonally dry Amazonian forests" *by* J. R. Melton et al.

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Melton et al. present a well-written paper describing an important element of tropical ecophysiology-heterotrophic respiration. This paper is worthy of publication, albeit with some modification. I'm not sure I agree with their characterization of K83 and RJA as having 'similar climate', and the authors are dismissive of several components of ecosystem function and comparison of model to observations in several areas where I would like to see some clarification.

The authors do a good job of acknowledging all the elements that can come into play, to varying degree, to determine cycles of carbon flux and surface-atmosphere

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energy/water/momentum exchange in Amazonian forests. There are a lot of moving parts. I agree that respiration (total ecosystem respiration, not just heterotrophic respiration) is critical to a demonstrably accurate representation of ecosystem function in tropical Amazonia. I also strongly believe that an holistic approach is necessary-we can't just focus on one element of the system and ignore everything else. Heterotrophic respiration is certainly an important element, but not the only one.

That being said, some specific comments:

Page 12488, line 3: I'm not sure I agree that K83 and RJA have similar climate or precipitation patterns. From the 3 years of tower data from LBA-MIP, it appears that RJA has mean annual precipitation of ~2350 mm and K83 has mean annual total of ~1650. This is an almost 50% difference. Furthermore, the dry season at RJA is very distinct-JJA are months with little or no precipitation. At K83, July-December qualify as 'dry' (less than 100mm precip) months, although the possibility of a month with >100 mm of rain is more probable. As data coverage expands, it is becoming more and more apparent that Amazonian forests are extremely heterogeneous; seasonality is most strongly expressed in precipitation, in terms of both annual total and seasonal cycles of wet and dry periods. If I am to believe that RJA and K83 are similar in climate, I will need more than an unjustified statement to convince me.

Page 12491, lines 3-11: Do the authors contend that the mechanisms listed in this paragraph (deep roots, HR, deep soils, leaf age) are unimportant? If so, why? This touches on an important, and hard to resolve, aspect of simulations of these complex ecosystems. As the authors note, many of these model mechanism are not invoked as a 'modelers fancy', but in response to observations of natural ecosystems. Is it required to incorporate every single one of these mechanisms into a model? If not, which ones can we ignore, and why? Admittedly, this is a difficult question to answer; we can't re-write our model when a new paper comes out. On the other hand, I think it is important to acknowledge limitations in our description of the system, as reflected by the equations in our model, when attempting a paper such as this. This is where

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I wonder if the differences between the simple linear parameterization and the more realistic Rhet characterization might come into play. By incorporating a more realistic description of a particular process (Rhet) into the model, real improvement can be seen and quantified.

Page 12494, line 7: How are the maximum Rhet values (0.0208, 0.6339 kg C (kg C-1) day-1) determined? Is that an empirical number to balance GPP over the simulation interval?

Page 12495, equation 7: It appears that there is a typo: the entire soilwater ratio should be taken the the Clapp and Hornberger B exponent, not just the numerator.

Page 12496, lines 4 and 8-10. I agree with the authors' decision to invert the normal moisture potential convention. It might be helpful to say so at the outset of this section, with perhaps a sentence describing how saturated soil has a potential at or near zero, increasingly negative with drying.

Figure 1a: It is a little confusing to have wetter soil at opposite sides of the x-axis in the two panels. It might be more clear to plot moisture potential from high (dry) to low (wet) to correspond to the volumetric soil axis in panel b.

Page 12500, line 22: Do the authors mean 67K instead of 63K?

Variability explained: Throughout the paper qualititative descriptions are used when describing R-squared values. What delineates a 'good' comparison from a 'poor' one?

Figure 3, panel C: The MODIS-derived annual GPP cycle suggests a 'light-limited' environment, in which GPP increases when cloudiness decreases during the dry season, in support of Saleska(2007) and Huete (2006). The tower-based GPP indicates a more 'water-limited' environment, where GPP decreases during the dry season, in support of Semanta (2010) and perhaps Morton (2014). The authors claim that "it is not apparent which of the two estimates is correct", which seems convenient since CLASS-CTEM shows no annual variability in GPP at K83. Does the MODIS estimate suffer from the

artifact in sun-sensor geometry reported by Morton? And what about tower-based estimates of respiration? These agree quite well with CLASS-CTEM Rhet, which invites further discussion about how the tower-based Rhet estimates are formulated. Wouldn't this also imply that the tower-based GPP estimate is perhaps more robust? I think the authors have an obligation to support one or the other of these divergent GPP estimates, even if their only justification is the CLASS-CTEM simulations.

Page 12503, lines 22-24: Using one publication (from a tower in Guiana) to make a blanket statement that Rauto is invariant seems like a bit of a reach. Malhi (2009) describes Rauto as a 'challenge to measure', and the leaf component especially suggests that variability might be an issue. If the authors have multiple sources to defend this claim I would be more likely to believe that Rauto, across the Amazon basin, is invariant. I am not disputing Rowland's results; however, heterogeneity across the basin is seen in almost all observational datasets.

Figure 5, Net Radiation: Rnet observations are available from the LBA-MIP dataset. As this metric is a critical measure of the energy input into the system, these observations should be compared against CLASS-CTEM. From a rough comparison, it appears to me that CLASS-CTEM does a reasonable job with Rnet at RJA, but simulated Rnet at K83 is much lower than observed. The authors should discuss this. Figure 5, Latent Heat: Simulated LE at K83 follows the same annual cycle as observed, albeit with an offset. At RJA there is more variability in the annual cycle than observed, and simulated LE again exceeds observed. The authors cite energy budget closure as a likely reason for this overestimation. If this is the case, then results should be similar with sensible heat.

Figure 5, Sensible Heat: In this case, simulated H at RJA follows a similar annual cycle to observed, with a positive bias; this is consistent with the picture painted for LE, where closure of tower observations imposes a negative bias in the observations. But what about K83? Simulated H there is very small, and in fact is negative during December. This is not observed. In fact, if we claim that observational closure is an issue, then

if the simulation matches the observation exactly then we know the simulated value is too low. What does it mean when the observed H is essentially zero? Does this come back to Rnet, and what does it mean?

Page 12506, line 14: 'leave' should be 'leaf'

CTEM litter generation: As is frequently the case, models formulated by midlatitude researchers frequently have mechanistic processes that are inappropriate for the tropics. This is not a criticism of the authors-almost all models have this bias. The litter triggers in CTEM (cold, persistent drought) are inconsistent with observed triggers in the tropics; did the authors attempt a simple change in the litter generation (increases at the start of the dry season, for example)? Would such a change make a difference in annual cycles of Rhet?

Why do the authors take pains to state that the climate at K83 and RJA is similar? Wouldn't the model results be more robust if it could be shown that the model performs across climatic gradients?

In general, I like the paper. It addresses a subject that is of interest to those who study tropical ecophysiology, and I believe they demonstrate that Rhet is important to annual cycles of carbon flux. I like how the authors bring H/LE into the discussion: respiration is dependent upon GPP, and GPP is tied to the Bowen ratio through transpiration and canopy status. I think there are several points in this part of the analysis that need clarification and/or further discussion.

I'm not sure about the 'Alternative parameterization' or Rhet. If the authors want to demonstrate that they have improved model performance by moving from the alternative to a new formulation for Rhet, then its inclusion is justified. Otherwise, I wonder if this section might be removed altogether.

Increased availability of observations has increased our understanding of ecosystem behavior across vegetation and moisture gradients in tropical South America. This

paper adds to that body of work. I recommend that, with appropriate revisions, this paper be accepted for publication.

Ian Baker

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