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Biogeosciences
Discussions

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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We wish to thank the anonymous referees, the editor and Ian Baker for their positive and helpful comments. We answer each comment below.

Our comments are preceded by 'AU»'.

——————————- Ian Baker:

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 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:

AU» We thank Ian for his carefully considered comments and understanding of the complexity of the system we are attempting to simulate. «AU

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.

AU» We agree with Ian that the sites are by no means identical. The two sites are, however, more similar to each other than to a site like Manaus (with much less seasonality in precipitation) or Pe deGigante (with longer dry season and even less precipitation than K83), which was our intended meaning. In our paper, the similarity of the climate of the two sites is important when we look at their observed NEP dynamics. That is, given how functionally similar the two climates are at K83 and RJA (in terms of timing, duration, and intensity of seasonal drought), we would expect a reasonably similar NEP seasonal cycle. Instead the two sites show out of phase behaviour in their NEP seasonal cycles. This surprising difference is the principle reason we chose these sites for our analysis. To address Ian's concerns, we will add in an explanation expanding on the basis of our view of the sites as 'functionally' similar. «AU

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 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.

AU» The ability of CTEM to reasonably simulate the seasonal cycle of NEP would argue that these processes are of secondary importance to carbon dynamics at these two sites. That does not imply that the deep roots, HR, deeps soils, or leaf aging are wholly unimportant, but it does offer comment on their relative importance for the seasonal cycle of NEP. Our study cannot comment on which of those processes should be

incorporated into a model and which can be ignored. In our context, where we are using a terrestrial ecosystem model that forms part of an earth system modelling framework, we are interested in capturing the broad scale behaviour of a system in a parsimonious manner, with as few poorly constrained processes as possible. Each new process incorporated into a model brings with it a raft of imperfect parameterizations and 'tunable' parameters. Increased model complexity by incorporating uncertain processes such as these can easily result in problems with equifinality (Tang and Zhuang, 2008) where any model response can be achieved by simply tuning parameters. From Ian's comment, it appears he is also well aware of these issues. The revised manuscript will address this issue in the discussion section.«AU

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?

AU» The litter and soil respiration parameters are meant to yield average turnover times of around 1-2 years for litter and 10-40 years for soil carbon depending on the climate, consistent with observation-based estimates from litter bag experiments and similar experiments for soil carbon. In addition, the model parameters are fine tuned on a global scale to account for differences in litter chemistry of different PFTs, e.g. litter from needleleaf trees generally has lower decomposability compare to that from broadleaf trees, and to obtain reasonable global scale geographical distribution of litter and soil carbon when the model is driven with observation-based climate. We will make a note of this in the revised manuscript. «AU

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.

AU» Yes, thanks for catching that. Fixed. «AU

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.

AU»Ok, we have done so. «AU

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.

AU»Agreed, changed. «AU

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

AU» Yes, we haven't physically moved the field site 4 km down the road. Fixed. «AU

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

AU » This is a difficult question. We can expect numerous sources of error and uncertainty in the observations that we compare our model against. This is not meant as a criticism of the data as certain errors are extremely difficult to prevent. These include errors in the measurements themselves, the representativeness of the measurements, and errors in the aggregation from the actual measurements up to monthly values. The exact meteorological and site-level conditions at the flux-towers will differ from what we use to drive the model (even though we have taken pains to use site-level soil data and meteorological drivers, differences will exist). It is also possible that the vegetation themselves will be responding to historical events with recovery timescales long enough to influence the vegetation within the observed period. Quantifying these errors is very difficult. We also are comparing mean monthly values across multiple years. Therefore with these uncertainties in the observations, we cannot expect even a 'perfect' model to achieve exact correlation. This then leaves us to set a 'good' vs. 'bad' criteria that is only based on reasonable judgment of what constitutes acceptable model performance. Setting a criteria of $R2 > 0.7$ is good while $R2 < 0.7$ is bad is just

as subjective as our use of qualitative descriptions of the model performance.

That said, while R2 values are widely used, the RMSE is likely the best measure of model performance as it is more sensitive to the occasional large error due to the squaring process and thus will highlight when the model performance differs greatly from observations (which will most likely be due to real divergence in model behaviour from observations). There is no criteria for RMSE values to be good or bad since they are in the units of the variable being investigated and will thus differ between variables. However, since they are in the units of the variables themselves, readers can easily compare them against the observation values themselves. We will include RMSE values for all observation-model comparisons in the revised manuscript.«AU

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.

AU» We agree that the divergence between the tower-based and MODIS data is interesting. However, we do not agree that we are in a position to provide support to either dataset. If our model result was similar to either dataset, we could suggest that the other was in error, but we fall pretty much in the middle. Yet, our model Rhet similarity to the tower estimate does seem to indicate, as Ian suggests, that the tower-based

GPP estimate might be considered more reasonable. There is a caveat to that since the observed quantity, NEP, can be decomposed to GPP - Reco (or GPP - Rauto - Rhet). Thus we have the additional uncertainty of needing an appropriate estimate of Rauto to ensure a reasonable estimate of GPP (if we take the position the derived Rhet seems to be backed up by our simulations). While our model does seem to indicate that Rauto changes little through the season at K83 (Figure 3d), we have difficulty accurately evaluating that result (indeed Ian makes the same comment below). Consequently we prefer to not pass judgment on these datasets without a more robust platform on which to base our opinion. «AU

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.

AU» This comment ties in nicely to our reply to the previous comment. We share Ian's concern about how well validated Rauto results can be. That is a principle reason why we do not attempt to suggest which GPP estimate at K83 is better. For our use of Rauto to estimate the Rhet component of Reco, we are following the protocol set out by Rowland et al. (2014). The assumption that Rauto changes little seasonally at our two sites is reasonable as the principle control on Rauto is temperature changes, and these field sites have little temperature seasonality. We do grant that it is conceivable that changes in Rauto could be larger than anticipated, but our model results do support the notion that there is little intra-annual dynamics in Rauto at these sites. «AU

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.

AU» Thank you for bringing these to our attention. We obtained our data from a project member, not the LBA-MIP site thus we weren't aware of these. The observation-based Rnet for both sites will now be incorporated into Figure 5a. The simulated Rnet matches very well the observation-based estimate at RJA ($R2 = 0.891$, RMSE = 3.965 W m-2 month-1) and we reasonably capture the K83 Rnet ($R2 = 0.668$, RMSE = 9.776 W m-2 month-1). We also find that the RJA observations do not have energy balance closure while K83 does. We will add in discussion around energy closure and how the model results compare to these Rnet observations.«AU

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?

AU» The two questions above are linked so we will address them together. As we discussed in Ian's question around Rnet observations, K83 observations have energy balance closure while RJA does not. This implies that we are able to assess the model against K83 observations, but this is not possible at RJA. From this we see that the model's monthly latent heat flux estimate is too high at K83 while its sensible heat flux is too low, thus we are not partitioning correctly. The seasonal cycle at K83 appears to be reasonable. Now that we are able to comment on the energy balance closure, we will add some discussion about this in the text.

The modelled K83 sensible heat flux does go to negative values in Nov - Dec. Our best explanation for this is that the K83 forest has a dense canopy (LAI ∼5m2/m2) which is capable of intercepting a lot of rainfall. While the rainfall in December is not high, the canopy may receive enough rainfall to be wet for much of the time; and since temperatures are warm, the atmospheric demand is high. Since in December the net radiation is low, practically all of that energy goes into evaporation, leading to surface cooling and therefore a surfaceward sensible heat flux in December. «AU

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

AU» Fixed.

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?

AU» We are actually not entirely mid-latitude researchers although, of course, more prevalent literature and better understanding of mid- to high-latitude ecosystems means all models are biased to some extent with a better representation of processes for mid- to high latitude ecosystems compared to tropical ecosystems. For triggers of litter generation, we did not attempt to change litter dynamics in CTEM. We cannot judge if this would make a difference in Rhet seasonal dynamics although our results suggest that it would be of second order importance (as discussed above where we

addressed Ian's question about the importance of the other processes like deep roots, HR, deep soil, etc.).«AU

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?

AU» This relates to the NEP behaviour, as already discussed above. «AU

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.

AU» We introduced the section related to the 'Alternative parameterization' following the suggestion by Editor Matthew Williams prior to its acceptance as a discussion paper. «AU

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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——————— Anonymous Referee #1

This article compares model simulations with the CLASS-CTEM model to data from two seasonally dry forests in Amazonia. The research is focused on how changing the simulated soil moisture response function alters the ability of the model to replicate the seasonal pattern of the net ecosystem productivity. The topic of research within this article is very important and greater focus is needed on how to accurately capture the response of heterotrophic respiration to moisture, and its influence on ecosystem level fluxes within tropical forests. This article is well written and demonstrates the importance of accurately simulating soil responses to moisture to improving the simulation of the seasonality of NEP in the study sites. However, it is a shame that this model has not been more comprehensively tested across many sites in Amazonia as this would allow a true assessment of if this heterotrophic respiration model can be used more widely. Previous papers, including the cited paper by Rowland et al 2014 have come to the same ultimate conclusion as this paper that “the role of soil moisture in controlling heterotrophic respiration deserves attention as well” and that accurately modelling soil responses is key to this. Indeed what is needed is for a way to incorporate a more universal model of the response of soils to moisture into vegetation models. This research does provide a model which improves the simulation of NEP at two sites with contrasting soils and soil moisture responses; however one site, RJA , has limited data and no

soil respiration data. The limitation of this research to two sites restricts the capability of this study to really test this models validity and consequently restricted the scope of its conclusions. Despite this limitation I suggest that this this work should be published as this topic is very important, and limited work is done to improve the simulation of the heterotrophic respiration in tropical systems. However I suggest that the authors try to highlight more clearly the unique conclusions that this work adds to the literature and I suggest the authors consider and address the following comments:

AU» We thank the referee for their positive comments. We absolutely agree that a universal model for modelling the response of soils to moisture is needed in vegetation models. That is a principle reason why we do not attempt to tune our model for these specific locations (see discussion of equifinality in Tang and Zhuang (2008)). To do so may improve the performance of the model at these sites, but it would not help us understand the broad applicability of the model. We chose the two sites based on their unexpected contrasting NEP patterns (as discussed in our reply to Ian Baker above). We also very much agree that it would have been desirable to have had more observations available to test our model against, but we are limited by what is publicly available. «AU

1) I would suggest that the research article needs to quantify numerically how much of an improvement the more detailed soil moisture response model gives over the simple one, as this is not clear in the paper and in the Figures it would seem that the simulated K83 heterotrophic respiration of the simple model is similar to the more complex model and the observed data. Perhaps the RMSE of model and data can be compared, across models.

AU» Please see Section 3.5 in the paper (p. 12507). In this section we compare the parameterizations and include the R2 and RMSE values between the standard and alternative parameterizations at both sites. We copy below a relevant part of that section: 'The RMSE and R2 values when using the alternative parameterization (K83 R 2 = 0.10, RMSE = 35.70 g C m-2 month-1 ; RJA R2 = 0.20, RMSE = 28.75 g C m-2 month-1) are also poorer at both sites compared to the standard parameterization (K83 R^2 = 0.81, RMSE = 11.99 g C m-2 month-1 ; RJA R^2 = 0.51, RMSE = 15.34 g C m-2 month-1). ' The difference in RMSE values likely best captures the improvements due to the standard parameterization. «AU

2) I find it concerning that the author does not discuss in more detail the problems associated with the differences between the simulated and observed soil moisture in Figure 7d. Its seems that the author is not overly concerned with the difference in values of the soil moisture for the 20 and 40cm layers (Lines 4-12, p12507) and the author does not discuss the fact that the model seems to have a much steeper decline in soil moisture in these layers in the dry season than is observed. It would seem that the absolute values of soil moisture and the seasonal response should have a significant impact on the soil matric potential and therefore the simulated values and seasonal response of heterotrophic respiration. Therefore I suggest that these discrepancies be discussed in more detail.

AU» As we discuss in the paper, land surface schemes are known to simulate different soil moisture states when driven with the same meteorological forcings. Thus a bias in the model mean state is not a surprise, nor is it concerning. The response to changing moisture conditions is where a model can more ably demonstrate it's accuracy. In our simulations (Fig 7), the model timing of drying and wetting compare reasonably well with the observations. It should also be kept in mind that the depth of observations does not match exactly our soil layer depths since they are the mean soil moisture across a depth of soil while the observations are point measurements. «AU

3) The abstract does not really represent the true outcome of this paper. I believe that the key message of this paper is that NEP can be better simulated by using a soil moisture response function which reduced heterotrophic respiration when soil matric potential is either too high or too low, which requires information of soil texture and depth. This point should be made clearer in the abstract. Also I find it strange that the author highlights as a positive point in the abstract and also in the discussion that

the model can achieve this without "deep soils or roots, hydraulic redistribution of soil moisture or increased dry season litter generation" as the author has not assessed whether these factors could improve the model further. I feel this is particularly the case for litterfall, which is not particularly well simulated in the study and could if improved alter the results of this study.

AU» We do mention in the abstract that the influence of soil texture and depth, through soil moisture that influences GPP and heterotrophic respiration is fundamental to appropriately capturing NEP dynamics. We do not feel it is necessary to explicitly specify soil matric potential in the abstract. Please see our discussion of inclusion of processes such as deep roots, HR, etc. in our response to Ian Baker's comments. «AU

4) Why is the Bowen ratio so much more poorly simulated on K83 than RJA (Line: 28,p12504)

AU» The Bowen ratio has a low RMSE at RJA of 0.054 but is much poorer at K83 with an RMSE of 0.2. This is due to model's overestimation of latent heat flux at K83. We, however, do not know why the model overestimates latent heat flux. We now have observation-based estimates of net radiation for both sites as mentioned above in reply to one of Ian's questions. Those results show that there is no energy balance closure at RJA, so we are unable to compare model simulated latent and sensible heat flux at RJA. This will be an issue that will follow in future studies. «AU

5) I wonder whether comparing to MODIS data is beneficial for this study. Clearly in Figure 3c it is providing an opposite signal to the flux tower data used as the basis for comparison in this study so on what basis should we believe it is giving the correct response in Figure 3b?

AU» Please see our response to Ian Baker's comments around the MODIS vs. flux-tower GPP estimates. As to whether MODIS is giving the correct response in Figure 3b we can only note that the MODIS estimate is supported by our model simulation. As outlined in our response to Ian Baker, we have no way of either categorically refuting, or supporting, the MODIS estimates. «AU

——————— Anonymous Referee #2:

Melton et al. investigate the role of soil respiration on the seasonal cycle of net ecosystem production (NEP) in two tropical wet forest sites contrasted by only soil texture and depth. In the context of recent studies that have concentrated on optimizing the seasonal cycle of gross primary production, Melton et al find instead that soil respiration and its seasonality is equally important. The main findings are that for the CLASS_CTEM model, seasonal GPP is relatively well simulated and that soil respiration is sensitive to whether microbial activity is related to soil matrix potential or soil moisture and the respective shape of each response function, and the paper is well written and clear to follow.

The paper provides an important perspective on the seasonality of tropical forest carbon cycling. Main comment is 1) why are the component fluxes from the site eddy flux towers not used, substituting MODIS GPP for GPP derived from the tower measurements introduces a lot of uncertainty, and 2) more discussion on the difference between the tested Rh methods and how this is related to microbial processes at a more detailed level would be appreciated. For example, are there experiments (i.e., drought experiments) where soil chamber measurements have come to similar conclusions about how soil respiration and microbial processes are controlled?

AU» We are glad the referee #2 likes the paper. We use the MODIS GPP at RJA because, to the best of our knowledge, GPP estimates from the flux tower are not publicly available. We agree that lacking the flux tower data is not ideal and would have preferred it being available, but we are constrained by what data exists for our use.

Ian Baker has suggested to remove the sections of the manuscript related to the Alternative parameterization that models the soil moisture response to Rh and referee #2 has suggested “more discussion on the difference between the tested Rh methods”.

We feel that the current discussion about the two approaches is a balance between the two comments. The basic premise of the standard parameterization used in CTEM, that heterotrophic respiration is constrained both when the soils are dry and when they are wet, is explained at the bottom of page 12495 and top of page 12496. The Griffin (1981) paper that we refer to is in fact based on how microbiological processes are affected by soil moisture. There are other references as well that are mentioned in the original Arora (2003) paper that describes the heterotrophic respiration parameterization for CTEM. For example, Davidson et al. (2000) and Orchard and Cook (1983) show that soil respiration rates are linearly correlated with the logarithm of soil matric potential. We will include these additional reference in our revised manuscript. «AU

Some minor comments: Intro: The amazon doesn’t experience seasonal “drought”, please check that “dry season” is specific. Drought is considered an anomalous event.

AU» While we agree with referee #2’s assessment of drought being an anomalous event, the term “seasonal drought” has been used in the context of Amazonian forests in a number of publications. A simple search for “seasonal drought Amazon” on the Internet will yield several such publications. We therefore prefer to retain our original use. «AU

Methods: I appreciate the clarity in explaining the physical equations for soil respiration, but where do the empirical values come from? For example on page 12494, where do the soil respiration parameters come from (i.e., 0.0208 kgC(kgC)ôĂĂĂ1 dayôĂĂĂ1 and 0.6339 kgC(kgC)ôĂĂĂ1 dayôĂĂĂ1)?

AU» The litter and soil respiration parameters are meant to yield average turnover times of around 1-2 years for litter and 10-40 years for soil carbon depending on the climate, consistent with observation-based estimates from litter bag experiments and similar experiments for soil carbon. In addition, the model parameters are fine tuned on a global scale to account for differences in litter chemistry of different PFTs, e.g. litter from needleleaf trees generally has lower decomposability compare to that from broadleaf trees, and to obtain reasonable global scale geographical distribution of litter and soil carbon when the model is driven with observation-based climate. «AU

Methods: Need to describe processing of MODIS and which quality flags were used – this can have effects on the seasonal cycle and introduce bias (see Morton et al. 2014).

AU» We used the Numerical Terradynamic Simulation Group's (NTSG) improved MOD17A2 data set which contains an 8-day summation of GPP. These data are freely available from the NTSG (http://www.ntsg.umt.edu) and are corrected for cloud contamination and spatial smoothing of meteorological forcing data. Data gaps in the 8-day MODIS FPAR/LAI that are labeled as cloud-contaminations are filled with linearly interpolated data based on reliable FPAR/LAI. More details can be found in Zhao et al. (2005), Zhao et al. (2006), Zhao and Running (2010) and Heinsch et al. (2003). The quality flag in this case simply informs which 8-day GPP uses filled FPAR (bad quality due to cloudiness) and which doesn't (reliable due to clear sky). Filling gaps in FPAR has been shown to overall improve the quality of the GPP estimates (Fig. 5, Zhao et al. (2005)). We will include information about this dataset in the revised manuscript. «AU

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