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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.

Anonymous Referee #1

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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

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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:

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.

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

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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.

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.

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

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?

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