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Discussions**

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Interactive comment on "After trees die: quantities and determinants of necromass across Amazonia" *by* K.-J. Chao et al.

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Final response to Referee2

We appreciate that Referee2 considers our manuscript as an important paper for the scientific community. However, Referee2 raised some concerns about the analysis and results in the paper, especially references and results. We will respond to them in turn.

1. Not citing enough literature in the Amazonian forests. As a literature review there are many papers missing. I understand that this paper might not be a comprehensive paper on necromass in Amazonia, but all available data should be presented.

RE: We agree that to be a comprehensive paper, it is necessary to cite all the relevant studies about necromass in the Amazon, and thank Referee2 for helping to find more

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related papers. However, some are not appropriate with our study criteria (e.g., Carey et al., 1994; Cochrane, 2003; Wilcke et al., 2005) and some papers have been listed in our original papers (e.g., Gerwing, 2002); the rest will be listed in our manuscript (e.g., Palace et al., 2008; Scott et al., 1992; Klinge, 1973). Our responses for each paper are as follows:

a. Carey et al. 1994 has biomass and necromass production (Carey, E.V., Brown, S., Gillespie, A.J.R., Lugo, A.E., 1994. Tree Mortality in Mature Lowland Tropical Moist and Tropical Lower Montane Moist Forests of Venezuela. Biotropica, 26(3), 225-265.)

RE: This study is about stem mortality rates, mass mortality rates, and biomass. There is no necromass measurement.

b. Cochrane et al. 1999 has biomass and necromass stock (Cochrane, M. A., Alencar, A., Schulze, M. D., Souza, C. M., Nepstad, D. C., Lefebvre, P. Davidson, E. 1999. Positive feedbacks in the fire dynamics of closed canopy tropical forests. Science 284, 1832-1835.)

RE: The studies are conducted in the areas where logging practice and fire disturbances are frequent. The paper has studies in unburned plots which are defined as: previously unburned in December 1997 (page 1832). However, in its Note 15 (page 1999), it appears that some of these were burned in 1995. Therefore, it is not clear whether these forests are intact or not.

c. Gerwing, 2002 (you cite this paper), but the paper also has necromass estimates for a non-disturbed forest. (Gerwing, J.J., 2002. Degradation of forests through logging and fire in the eastern Brazilian Amazon. Forest Ecology and Management 157, 131-141.)

RE: We have listed this reference in the original manuscript (page 2001).

d. Klinge 1973 (Klinge, H., 1973. Biomassa y materia orgánica del suelo in el ecosistema de la pluviselva centro-amazonica. Acta Cientifica Venezolana 24, 174-181.)

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RE: This paper reported necromass value in Manaus as 25.8 t ha-1. We will list this reference in Table 2.

e. Palace et al. 2008 - necromass production, stocks and use of a steady state model (Palace, M., M. Keller, H. Silva, (2008). Necromass production: studies in undisturbed and logged Amazon forests. Ecological Applications: 18, 873-884.)

RE: We will list this paper (Palace et al., 2008) in our reference list on page 1985 Section 2.3 CWD determinants: CWD input and decay rate, due to this reference having used the same steady state model as we used. As for the necromass stocks in Palace et al. (2008), they are reported from the same place (Tapajos National Forest) and the same period of time (2002-2004) as in Palace et al. (2007). To avoid possible duplication and over-emphasizing this site we will only use the results from Palace et al. (2007) which have been listed in page 2001 Table1 in our original manuscript.

f. Scott et al. 1992 (Scott, D.A., Proctor, J., Thompson, J., 1992. Ecological studies on a lowland evergreen rain forest on Maracá Island, Roraima, Brazil. II. Litter and nutrient cycling. Journal of Ecology 80, 705-717.)

RE: This study was conducted on a riverine island on top of sandy soils which is not the same as the terra firma forest defined in our study (page 2000). We will list results in this study in the white sand forest type section of our Table 2.

g. Wilcke et al. 2005 (Wilcke, W., Hess, T., Bengel, C., Homeier, J., Valarezo, C., Zech W., 2005. Coarse woody debris in a montane forest in Ecuador: mass, C and nutrient stock, and turnover. Forest Ecology and Management 205, 139-147.)

RE: Not appropriate. The study area is located at 1900-2180 m above sea level. Not the lowland forests which are the focus of our manuscript.

2. Not citing relevant literature in other tropical forests. It might good to reference other tropical CWD work. I know this is not the goal of this paper, but it might be helpful for other researchers in tropical forests. It also might bring the paper to a wider audience.

RE: As the Referee2 states, the goal of our study is not to review all the tropical forest studies about necromass in the world. We agree that it is important to have a review of necromass across all the tropical forests. Our paper will hopefully contribute toward that goal.

3. Not citing the steady state models applied in other published papers.

RE: We will list these examples (Keller et al., 2004; Palace et al., 2007; Palace et al., 2008) which have used the same steady state model on page1985 section2.3.

4. Biomass estimates. Of minor note, in Asner et al. 2002 there is a biomass estimate for Cauaxi. Stand data is available on LBA-ECOs Beija-Flor, so you could calculate new biomass estimates using Chambers and Chaves equations. (Asner, G. P., Palace, M., Keller, M., Pereira R., Silva, J. N. M., Zweede, C.,2002. Estimation canopy structure in an Amazon forest from laser range finder and IKONOS satellite observations. Biotropica 34, 483-492.)

RE: Not appropriate. The Asner et al. (2002) paper is about canopy structure, including the relationships between DBH and both tree height and crown diameter. Although the data (documented in LBA-ECOs Beija-Flor (http://www.lbaeco.org/lbaeco/data/data_poldoc.htm)) can be used for biomass calculation, it records only trees larger than 20 cm diameter and did not have wood density values for most species. All of the new biomass values presented in our study are calculated from all trees larger than 10 cm and with species wood density information. Only those from published results are based on varied methods.

5. Clarify the difference between stem mortality (as a percent) and mass of mortality (as mass of total necromass created).

RE: This is the same point as made by Referee1. We should explain that forests stem mortality rate means number of dead trees and mass mortality rate means amount of dead mass in page 1988, lines 18-20. We will rephrase the sentences.

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6. Address necromass in smaller diameter branches and necromass production in branchfall. Rice et al. 2004 found that close to 10 percent of the necromass were in classes with diameters 2-10 cm. Chave has stressed that smaller trees, shrubs, and vines are components of forest productivity that are important and might often be over looked. Keller et al. 2001 estimated that 21 percent of the total aboveground biomass at Tapajos is found in smaller trees and vines. Another aspect of necromass dynamics not addressed in your study is branchfall. I question whether just using mortality estimates from plot data might be not accounting for a fair amount of necromass productions. Chambers et al. (2001) estimated branch-fall to be 0.9 Mg ha-1 y-1. At Barro Colorado, Panama, Chave et al. (2003) estimated that branch falls may contributed 0.5 Mg ha-1 y-1 to aboveground biomass loss. Clark et al. (2001) noted the potential importance of branch fall to estimation of net primary productivity, which in turn would influence necromass production. Palace et al. 2008 stress that using a mortality rate to estimate necromass production may lead to a substantial underestimation from 30-50 percent.

RE: We did not address the contribution of small diameter branches in necromass pool and branchfall in necromass production, as we focus on results for necromass larger than 10 cm diameter. We will replace the paragraph in page1992 line18-25 by the following paragraph.

Some other factors which can influence necromass pool estimation have not been considered in our study. These include: (1) small branch ($<$10 cm diameter) contributions in necromass pool, (2) branchfall contributions in necromass production, and (3) temporal variation in mortality rates.

Small branches (2-10 cm diameter) may contribute a significant portion to total necromass pool (e.g., 8-18% in Keller et al., 2004; 10% in Rice et al., 2004), but we did not take this portion of dead wood into consideration as there are few appropriate data.

Necromass production in our study is estimated by mortality rate, but Palace et al. (2008) stress that this method can underestimate necromass production up to 30-50

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%. This is mainly due to overlook the fact that branchfall can contribute considerably to the pool (e.g., Clark et al., 2001). Better quantification of the necromass production in branchfall would reduce the uncertainties of the dynamic relationship between necromass production and pool.

Our calculations are based on the assumption that the studied forests are in steady state (dynamic equilibrium). Our studied plots are located in forests free of cyclones, but wind-storms or droughts sometimes affect forests. For example, the extreme value of CWD reported from Tapajos (Rice et al., 2004), is likely to reflect an earlier large disturbance. Long-term studies of CWD decomposition and dynamics across sites would provide a valuable extension to this study.

7. Why three decay classes and not five like many studies? I believe this was addressed in Chao et al. 2008 or Baker et al. 2007, but should be mentioned here, since many necromass studies from Harmon et al. 1995 to Rice et al. 2004 have used five decay classes.

RE: Density of dead wood in the highly-diverse tropical forests is typically sampled from a range of unknown species from the field (e.g., Keller et al., 2004; Palace et al., 2007; Rice et al., 2004). However, when using the five class classification, is there really a significant difference in densities between class 1 and 2 or density between class 3 and 4 for tropical dead wood (e.g., Juruena in Palace et al., 2008)? Some studies actually report higher density in decay class 2 than decay class 1 (e.g., Keller et al., 2004). On the other hand, most temperate forest studies focus in single or tens of species, so it is easier to develop a detailed classification of decay classes (Harmon et al., 1986). The reason why we use the three decay classes are due to it revealing comparable patterns to the five decay classes but being less susceptible to potential problems of small sample sizes and the high diversity of tropical trees (Chao et al., 2008). We will address this in our manuscript.

8. R-square values in regression equations. Though significant the r-square values are

not high. I doubt you can draw any conclusions from these results, especially trying to relate biomass to necromass. You conduct three regressions and decide that the best of three with low r-square values is enough to draw a conclusion. How about some error estimates or confidence intervals on these graphs? You state in the paper that biomass is a poor predictor of necromass, but then you also say that necromass stocks are related to biomass, and especially mortality mass input and living wood density. None of these in my opinion are proven from your regression analyses. Figure 1a, the residuals appears to not be normally distributed around the regression line. There is a bias in middle biomass numbers.

RE: We need to emphasise that we did not reject biomass as a predictor of necromass, but concluded that necromass is BETTER explained and logically sound by both wood density and mortality mass input than it is by biomass (page 1993, lines 8-9). The statistical meaning of r-square is: the percentage of variations that can be explained by the regression (Dytham, 2003). Therefore, among the three regression lines, living wood density has a relatively better explanation in data variation while the p values are all smaller than 0.05. We re-drew the regression graphs with confidence intervals for mean (figures can be submitted when necessary). Figure 1a is certainly biased toward middle biomass plots, Fig1b and 1c have better scattered patterns, so the confidence interval is narrower. Figure 1a showed that among Amazonian forests with similar quantities of biomass there is a wide range of necromass consistent with the interpretation that using biomass to predict necromass is less useful than using wood density or mortality mass input.

When analysing residuals, we find that residuals do not perfectly, randomly distribute against the biomass independent variable, but are better distributed for the mortality mass input and living wood density variables (figures can be submitted when necessary). One study plot in the eastern Amazonia (i.e. Rice et al. (2004)), always with the highest positive residuals, is a distinct outlier. Excluding the Rice et al. (2004) data point, the r-square changes to 0.155 (p = 0.019), 0.406 (p < 0.001), and 0.396

(p $<$ 0.001) for biomass, mortality mass input and living wood density, respectively. These still show higher r-square values for both mortality and living wood density than biomass.

In our revised manuscript, we will use the confidence interval figures to help the explanation and explain the bias distribution of Figure 1a.

9. Why a Mann-Whitney U test and not a t-test? I understand that a Mann-Whitney U test is much like a t-test once items have been ranked, but use of non-parametric statistics is often used when parametric statistical test are not finding a significant difference.

RE: We used non-parametric statistics when the data are not normally distributed, not to search for a significant result. As addressed in Dytham (2003, page 101): the Mann-Whitney U test is less powerful than a t-test or one-way ANOVA, but is less likely to report a significant result when there is no real difference.

10. Problematic Figure 2. Not sure how to fix this. Circles obscure each other, triangles are difficult to tell size. Still some spatial extrapolation might be beneficial to your paper.

RE: Figure 2 is to give readers a visual aid about the distribution patterns of necromass in the Amazon. All the actual values are listed in Table 2 and Appendix A1, so the readers can check these values when necessary. We are confident that the major patterns would stay the same when using spatial extrapolation. However, spatial extrapolation will encounter more problems due to spatial data having complex, multi-dimensional properties that require special handling and processing (Zhang and Goodchild, 2003); we therefore propose to stick with our simpler approach to scaling.

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