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Biogeosciences Discussions

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

Anonymous Referee #2

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Review

This paper is an important for the scientific community because necromass in tropical systems is poorly understood and estimates on local and regional scales are necessary for understanding global carbon budgets and forest dynamics. This paper is also interesting because it is both a partial literature review and a new analyses of existing literature values coupled with new data. The paper presents complicated data from multiple researchers and studies very well and does an adequate job relating very different techniques. The writing is crisp and I found no need for grammatical corrections.

Despite these good merits of the paper I do find many things lacking in the paper and have major problems with their analysis and results. These break down into two groups, not citing relevant literature and poor results in the regression analysis and problems with random sites in statistical averaging.

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. In addition 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.

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.

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.

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.

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.

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

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and logged Amazon forests. Ecological Applications: 18, 873-884.

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.

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.

You state one paper in reference to the steady state model (Olson 1963). Though this concept is not new, there are papers that have used this steady state model in Amazonia for necromass dynamics. You use the data from these papers but do not cite them in the text in reference to a steady state model These paper should be cited in the section where you present your steady state model. These papers are Keller et al. 2004 and Palace et al. 2007, Palace et al. 2008.

Of minor note, in Asner et al. 2002 there is a biomass estimate for Cauaxi. Stand data is available on LBA-ECO's Beja-Flor, so you could calculate new biomass estimates using Chambers and Chaves equations.

As the other reviewer mentioned there is a need to clarify the difference between stem mortality (as a percent) and mass of mortality (as mass of total necromass created). I what the author is getting at and it and feel that it is an interesting distinction in your paper and one that might be discussed about a little more.

I also feel that you need to address smaller diameter necromass. Most of the literature values used in your study only have CWD greater than 10 cm. Rice et al. 2004 found that close to 10 percent of the necromass were in classes with diameters 2-10 cm. This is a fair amount and should be considered in your analysis. I believe some other studies

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have suggested even a larger contribution from smaller CWD. 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 percet 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. If dynamics of necromass are more important to estimating a necromass stock, then not accounting for branch-fall may cause greater errors in your comparison with other parameters. In addition, if forests mortality of stems in the NW involves smaller trees, then you might be missing some necromass production that results in smaller diameter necromass.

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.

It is in the statistical analysis using regressions that I have the biggest problem with this study. Though significant the r-squre 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

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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.

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.

Figure 2 is problematic. 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.

By using equation 12 and density to estimate necromass, is this strengthening or promoting your idea of the difference between eastern and western Amazonia. I am not doubting a difference, but I think it might be using one concept where you have shown wood density differences across Amazonia, and then using that different to promote a necromass gradient. Here is the idea. To estimate necromass you estimate necromass input and decay using wood density. Then you state that necromass is related to AGB, which is determined using wood density.

How about a comparison of standing dead to fallen as a graph? You have a lot of nice data to look at this. It might even merit a statistical test.

Average of sites is not representative of necromass spatially. There is a bias in plots, more work done in Manaus and Tapajos. Need a better method of estimating necromass across regional forests and forest types. This is a major issue. Your sites are not randomly selected so comparison between regions is problematic.

Are plots large enough? Are RAINFOR plots missing some aspects of disturbance due to scale? Do 1 ha plots miss a biomass and necromass relationship? An example is a term called the Chablis effect. Plots with high necromass might not have high biomass. Figure 1a might be showing this.

Because of a lack of strong relations in the regression analysis and problems with averaging non-random site locations for regional estimates (which I did not dwell on in this review), I feel that this paper needs significant work to address these problems. I find fault with the use of wood density estimates to determine necromass stocks and then the general conclusion that there is a difference between eastern and western Amazonia forests. I believe that this paper could have provided a valuable need in tropical forest carbon dynamics, but it needs to address these short comings.

Interactive comment on Biogeosciences Discuss., 6, 1979, 2009.

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