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Interactive comment on "Validation of demographic equilibrium theory against tree-size distributions and biomass density in Amazonia" *by* Jonathan R. Moore et al.

Anonymous Referee #1

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General comment I have mixed impressions of this manuscript. On the positive side, I thought that the exercise of fitting different models to the RAINFOR plot data was a worthwhile idea. It was informative to see the biases associated with the different models. However, the manuscript fell short in two general ways. First, the manuscript does not provide a deep explanation of what the results meant. In particular, the Discussion seemed superficial and primarily summarized the results. Second, by focusing on curve-fitting, the manuscript did not seem very creative. There were, perhaps, some missed opportunities for analysis. Several further suggestions follow.

Specific comments I think that this manuscript would be more impactful if it were or-

ganized around an explicit scientific question or hypothesis. In its current form, the manuscript is focused on the implicit question of whether DET or MST better fits the Amazon plot data. This implicit question strikes me as too technical. I would like to challenge the authors to develop a question that is more focused on the fundamental biology rather than a close-ended question of which model is better.

At the end of the paper, the authors discuss the work of Zhou and Lin (2018), who discuss a "fundament flaw" in the MST model. If the authors knew this, why did they bother with MST model at all in their own analysis? The way that the text is currently framed, one is left with the impression that the MST model was a straw man.

Artificial imposition of a maximum tree size seems unsatisfying to me. That it is needed suggests that there is a problem with the size-dependence of the mortality and/or growth rates. How might mortality (and/or growth) rates be modified so that maximum tree size would be a predictive outcome of the model?

Page 1, line 5: Here and elsewhere in the manuscript, it is stated that one model is "better" than another. But "better" in what sense? Blanket assertions that one model is better than another seem unwarranted to me.

Page 1, line 16: I did not see any whole-continent analysis.

Page 5, line 25: Please explicitly describe your algorithm.

Page 8, line 13: What is a "data point"? A stem? A size class? Something else?

Equation 14: I do not see how the second equality follows from the first. Please be more explicit.

Equation 15: What is S?

Page 9, lines 9-11: from the text, it looks like the two parameters were not estimated jointly: the parameter mu1 was estimated first, and then the estimate of mu1 was used to estimate phi. The problem with this procedure is that the estimation of mu1 itself depends on phi, which is initially unknown. I am left confused about exactly what the authors did.

Page 9, line 19: The inequality is not sufficient to justify the assumption. Rather, the entire argument of the exponential must be small. For example, what if Dmax » DL, but c » mu1? I know it did not turn out that way, but it could have.

Page 10, line 24 through Page 11, line 1: It would help to justify this statement.

Page 27, lines 3-4: The text is misleading because the biomass was not actually observed.

Most of the Discussion is a re-statement of the Results. This is a major weakness of the manuscript because the significance of the results is left unexplained.

I found numerous typos (to list a few: page 3 line 9; Fig 1 x-axis label; page 10 line 22). The manuscript would benefit from a careful proofreading.

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