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Interactive comment on "Sensitivity of woody carbon stocks to bark investment strategy in Neotropical savannas and forests" *by* Anna T. Trugman et al.

Anonymous Referee #1

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General comments

In the age of anthropogenically forced climate change, developing a comprehensively predictive Earth System Model is an extremely high scientific priority. Trugman et al is a highly relevant study that aims to assess hypotheses regarding the trade-off between fire adaptation in the form of bark thickness and growth and survivorship. The hypotheses evaluated in this study are clearly articulated in the Introduction and described in the Methods such that replicating this experiment would be fairly straightforward. ED2 is an appropriate tool and the Methods are, in general, appropriately designed for exploring the hypotheses of this study. Importantly, the source code for the model, which

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documents the exact model structure and parameter values used in this study, is included with the Supplement Information, thus making the model itself transparent and the simulations reproducible. This study makes two important contributions: 1) (model development) it demonstrates the importance of including a fire-adapted trait in terrestrial biosphere models and 2) (ecological understanding) it illustrates the potential role bark thickness has in modulating coexistence of different life forms (grasses vs. trees) and aboveground C-stocks in fire-regulated ecosystems. I do, however, recommend that, before this manuscript is accepted for publication, the authors address the concerns listed below and make clarifications in the text where necessary.

Specific comments

1. The Methods need to be clearer (refer to lines 96-100, 144-146) about the two new tropical PFTs not being simulated simultaneously. As written, the Methods left me with the impression that both PFTs were being run simultaneously and that direct selection for one strategy over the other was part of the experiment (line 144-146). I had to read the namelist (ex. ED2IN_fire_control_MAP1450_FR3_pen1) to verify that only the savanna tree or forest tree (apparently PFT #26 for both?) was being run with the C4 grass (PFT #1?) in each simulation. This needs to be clear so that the reader can fully understand how to interpret the ecological significance of the results.

2. I am not strongly convinced by the ecological importance of the results presented in Figures 2 and 3. In several panels it is difficult to visually discern that the “bark” performed better than the “no bark” or what the differences, where present, actually mean. For example, in all of the panels the IQR of the “no-bark” is tighter than the “bark”, and hence, is in better agreement with the IQR of the observations. Or, for Fig 3a, I would not necessarily consider the “bark” to do a better job “capturing” the observations relative to the “no-bark” just because the observations fell within the broad error range of the bark-model predictions (line 224). Benchmarking new model capability is important, and I think these are obvious choices as benchmarks. However, I am not sure what these benchmarks add to the central argument (and scientific contribution) of this

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paper (e.g. as I take it, the central argument is: Abstract lines 23-25, Introduction line 6, Figures 5 and 6, Results lines 275-276, Discussion line 280, 316-317, Conclusions lines 343-346). Consider moving Figures 2 and 3 to the Supplemental. Even if model performance against the benchmarks in Figures 2 and 3 is weak, as I would contend, I do not view this as a major problem for this paper because the emergent pattern in Figure 6 supports the "bark" hypothesis that this study seeks to evaluate.

3. In replacement of Figures 2 and 3 in the main text, it would be nice to see a figure of how the demographic rates that give rise to Figures 5 and 6 differ between "bark" and "no-bark". Demography is the fundamental feature of the ED2 hypothesis, as explained in Moorcroft et al. 2001. Therefore, it would be informative to see how the internal dynamics of the model that are central to its hypothesis (i.e. predictions of demographic ratesâĂŤgrowth, recruitment, mortality) are modified by inclusion of the bark strategy. In essence, this is the interesting ecology that ED2 and this study have great potential to inform. Figure S1 is the only figure of demographic rates and it is an informative starting point, but by itself, it does not close-the-loop for explaining the patterns in Figures 5 and 6. It seems to me that a mortality figure would also be useful for helping to explain the interaction between the survivorship hypothesis (Eqn2) and the different growth rates.

4. The meaning of the variation in Figures 2 and 3 is unclear. I do not understand how time factors into these distributions. Presumably, the observations are based on census data that was collected at one point in time, July 2012 (line 182). How was the model output sampled to generate the error estimate? If the errors are differentâĂŤone spatial (or across individuals), one temporalâĂŤthen how do we compare the errors? Please clarify in the caption, text or both.

5. I suggest a stylistic revision regarding verb tense for sentences describing model predictions. Since the model is a hypothesis, the verb tense should be in the present when speaking about model predictions. For example, at line 240 it says "predicted", but it should read "predicts". "The hypothesis predicts. . ." = "The model predicts. . ."

6. I suggest that the final sentence of the Abstract focus on the scientific contribution to ecology and not on model development. Model development speaks to a subset of ecologists. Since the Abstract may be the only thing many people read, the concluding sentence should be about the most important scientific contribution and speak to the broadest community you are trying to reach.

7. Line 46. “comes at a growth cost: thicker-bark. . .grows more slowly. . .”. This sentence is written as if this the carbon-cost trade-off is conclusively known, but it is not; it is a hypothesis. Indeed it is a hypothesis evaluated in this study with the no-cost bark PFT. Consider rephrasing.

8. Line 72. “We then tested whether two hypotheses were consistent with model simulations.” This does not read correctly to me. The model is the hypothesis and the simulations reflect the hypothesis. So, it is hard for me to rationalize how a hypothesis is consistent with itself. Consider revising.

9. Section 2.1. The source of the model parameter values needs to be cited. The only obvious reference for parameter values is for the brevideciduous PFT at line 97. For example, it is not clear what version of ED2 is the basis for the photosynthesis parameterization used in this study.

10. Line 168. Please provide the definition of the dry season used in this study (<100 mm/month?). It would also be helpful to know what fraction of annual precipitation falls during the dry season.

11. Line 210. Could tuning model parameters unrelated to bark thickness correct the overestimation of the forest species?

12. Line 235. “likely”. This is speculative. This can be known with this model. Addressing item 3 above should make this known.

13. In the model code, the namelist needs to have a description of what PFT 26 actually is. There are several PFTs listed up to number 17 in the namelist; but after that, it is not

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clear what 26 is or what the other tropical PFTs (numbers 2-4) are. The PFTs above 17 are given some notation in the source code (e.g. in ed_params.f90), but it is still not clear what exactly that notation means. A little more clarification about the notation throughout the code would be helpful.

Technical comments

1. Line 18. “ED2” Define acronym.

2. Line 215. “Fig1a”. Wrong reference?

3. Line 248. “comprised a substantial fraction of AGB were prevalent. . .” This does not read smoothly to me. Consider revising.

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