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Comment on gmd-2021-410

Anonymous Referee #1

Referee comment on "The impact of hurricane disturbances on a tropical forest: implementing a palm plant functional type and hurricane disturbance module in ED2-HuDi V1.0" by Jiaying Zhang et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-410-RC1>, 2022

This paper uses a forest dynamics model (ED2) to assess how hurricane disturbance effects composition and structure of a tropical forest. Key innovations in the work are (1) the development of a plant functional type specifically to represent palms, and (2) the implementation of hurricane disturbance in the ED2 model.

Overall, the paper is heavily focused on development of the model (warranted given the venue). However, I did think the biological context and implications could be more thoroughly presented, especially in the intro and discussion. I also felt there could be more reference to previous work on forest dynamics in the Luquillo Mountains (see detailed comments).

Although the manuscript is mainly well-written, there is room for improvement with respect to grammar / language. Please see detailed comments for some of the most important parts. There are numerous references on key points to papers that are in review. I do not know the policy of this journal but some things of the things being referenced are really critical to understand this paper properly (see detailed comments but, e.g., which species were classified into which PFTs, and on what basis?). I would think that some of these details should be included here in supplementary tables. At minimum, the papers in review should be posted to a pre-print server so the information cited is actually available.

Please note that many of the details on ED2 implementation / testing / calibration are outside of my expertise. Some of my detailed comments below do address these points, especially parts that I found could be more clear. Overall, however, I focus my comments on the general presentation of information in the paper and the links to the biology of the study system.

Thanks for the opportunity to review this manuscript, I hope the authors find my comments useful.

INTRODUCTION

General: Overall, I think this section could be improved by making a more clear introduction to the fact that this study uses a vegetation model to understand hurricane effects on a tropical forest. As it stands, much of the text focuses on effects of hurricanes and then it jumps into PFTs and there is little/no text giving the context / background / rationale / etc. for using vegetation models, including using them to study hurricane effects. This is particularly surprising because there has been other work (including in wet forests of Puerto Rico) on modeling hurricane effects (see, e.g., Uriarte et al. 2009; Natural disturbance and human land use as determinants of tropical forest dynamics: results from a forest simulator). Although not investigating hurricane impacts, another study using ED2 for wet forests in Puerto Rico did include palms as a 'late' PFT (Feng et al. 2017; Improving predictions of tropical forest response to climate change through integration of field studies and ecosystem modeling). It seems strange that this literature is not reviewed in the introduction.

L 35-42: Exposure (to hurricane winds) seems to be an important missing factor here?

L 45: It is not clear what is meant by "faster resprouting" - does it refer to sprouts being generated vs. time since disturbance? It seems to mean something different because the next sentence mentions time since disturbance as a separate point. Please clarify.

L 47: Not clear what is meant by "higher recovery equilibrium". Equilibrium of what? Equilibrium in what sense?

L 51-54: It is a bit odd that this last part of the paragraph highlights a single article (Wang and Eltahir 2000) rather than providing some kind of summary / conclusion point about the preceding paragraph. If the Wang and Eltahir paper is very important to mention specifically then please provide more context.

L 55: The start to this paragraph is abrupt and could be more smoothly linked to the preceding text. I think the jump is mainly in the fact that all of a sudden you are talking about terrestrial biosphere models and PFTs but none of this has been introduced. A better link would help the overall flow here. The introduction is pretty short as it stands so there is space to develop this more.

L 57-63: More citations for the justification of the 3 PFTs mentioned would be useful.

L 63: I would advocate to start a new paragraph when introducing the palm. This is a focal point of the paper but it is kind of buried in this paragraph.

L 64-65: Also reference:

Uriarte, María, Jill Thompson, and Jess K. Zimmerman. 2019. "Hurricane María Tripled Stem Breaks and Doubled Tree Mortality Relative to Other Major Storms." *Nature Communications* 10 (1): 1362. <https://www.nature.com/articles/s41467-019-09319-2>

L 69-70: "To account for these unique characteristics [in what? for what reason?], we define a Palm PFT." As other comments above, I think there needs to be an expansion of the introduction about vegetation modeling - building up to the overall aims of this study.

L 74: Seems strange to say "The results indicate that a single hurricane disturbance has little impact on forest structure" when much of the introduction was spent discussing the various impacts hurricanes have on forest structure / composition. Does this really mean "long-term" forest structure? Please clarify.

METHODS

L 92: "Since there is little knowledge about the traits of Palm." This is not a complete sentence and seems like it should be merged with the previous statement or otherwise revised.

L 93: Not all palms have this low "wood density" and I think, in general, you should couch the statements about palms with something like "many palms" since it is such a diverse group and we do not actually know the degree to which these statements might be true or contradicted by some palms. In fact, the range of wood density for palms (Arecaceae) in the global wood density database (Chave et al. 2009, Zanne et al. 2009) is 0.180 - 0.883 (median = 0.54). (checked with the 'wdData' in the 'BIOMASS' R package, v. 2.1.5).

L94-96: It is difficult to assess this decision because there are no details on the other traits used in the model. If palms "grow fast in open canopies like early tropical trees" then what is the reason to assume they "have the same probability distributions as those of late tropical trees"? I am guessing that wood density is strongly related to growth rate in high light conditions in the model. But isn't it also related to mortality rates (including in shade)? Since the introduction of this palm PFT is such a big part of this paper I think it should be explained in more detail here.

L 104-105: Palms can be shorter than other trees, given the same DBH but I am a bit skeptical of these allometric relationships. For one thing, when I plot them, palms can never reach more than about 13 m height at the maximum diameter (20 cm), which is too

short but other trees are predicted to reach unreasonably tall heights for this forest (~60 m for early PFT at the maximum 90 cm diameter). The only justification for these fitted parameters is from a paper by the authors 'in revision'.

L 107: This sentence requires a citation.

L 108: This sentence needs revision for clarity / grammar.

L 112-114: I am questioning the ramifications of these 'tricks' implemented in the model to help allow palms to survive despite their allometry.

L 115-117: Here the authors use default allometry of Early PFT for Palms but this seems inconsistent with the statement in L 94-96 about "...we assume that the traits of Palm have the same probability distributions as those of late tropical trees..." Please clarify.

L 124: A bit confused by "(sc) is the ratio of the cohort density that survived to the cohort density *before* the disturbance,"... should this not be the proportion that survives after the disturbance? Since $sc=1-\lambda c$ (L 127), and that λc "varies with hurricane strength, ...", it makes sense that sc would be post-hurricane survival... Please clarify / revise.

L 135: It seems like "Given mortality, the rate of each cohort (λc)" should be revised to "Given the mortality rate of each cohort (λc)"?

L 138-145: It is really not clear what is showing on the x-axis of these figures (x: proportion of large stems). How can this be the same during a given hurricane event for all PFTs? And why is mortality lower for all PFTs from hurricane Maria compared to Hugo? This section needs clarification.

L 146: Again some very relevant references seem to be missing. In particular Uriarte et al. 2009 (Natural disturbance and human land use as determinants of tropical forest dynamics: results from a forest simulator) and Uriarte et al. 2012 (Multidimensional trade-offs in species responses to disturbance: implications for diversity in a subtropical forest).

L 150-160: Please clarify the data upon which these functions are based.

L 170: I am missing details on the basis by which species were assigned to PFTS... this

seems too important to have only as cited in a work 'in review'.

L 161-173: It seems that perhaps this description of the census data should go earlier in the text? But more importantly, it is a bit problematic to have such important references to work that is 'in review' (not to mention there are two Zhang et al. 'in review' papers so we don't know for sure which one is being cited here). Perhaps now this paper is published? What is the plan if this manuscript is accepted before the outcome of the one 'in review'? It seems like posting a pre-print of the other work could be at least a partial solution.

L 172: Does the Scatena et al. (1993) biomass allometry apply to palms? Should a caveat be included here?

L 193-194: Clarify specifically the reason why the dark respiration factor from Feng et al. 2018 has "too wide a range". I am not familiar with the paper cited, which seems relevant but from a completely different study system.

L 197: Please clarify: you say, "clumping factor is defined as the projected area of leaves per unit ground area" but then the following details about ranging from 0-1 is more about the relative clumping of leaves over a given unit area

L 237: Why do background mortality rate for large stems? Treefall disturbance rate between small and large stems seems very similar - what data is this based on?

L 250-260: I find this part on palm recruitment to be confusing, in part, because there seems to be a disconnect between the model and data/biology of the system regarding palm "seedlings" and recruitment. Indeed, these palms produce abundant seeds and seedling density could perhaps be considered similar to early successional species. However, the decay of palm seedling abundance with time since disturbance is less dramatic than for early successional species, to my knowledge (there is relevant data available on this at least from the LFDP and prior studies). But how does this really relate to the 'observed recruitment of palms' in the data? The text is not clear about what "recruitment" of palms actually means in the data (at what height / diameter do they enter the census?). These are typically not at all "seedlings" since palms produce robust diameter stems prior to growing taller. If the model considers newly recruited individuals as those represented in the data, then it may not really be reasonable to assume that the "seedling" density of palms is similar to that of the early successional PFT. I think some additional details and work revising this section would be valuable.

L 314-317: It is somewhat difficult to assess this with knowledge of the study system without knowing which species are included in each PFT. I am missing a table showing this. L 170 says this information is in Zhang et al. (in review) but seems to important to

simple be cited in another paper, especially when that paper is not published yet.

L 319: Is 25% underestimation and 38% overestimation considered "consistent with observations"?

RESULTS

L 327: It would be good to include units and more informative labels on the figure itself. The legend does not seem to define the red line, which it should.

L 350: Instead of referring to wood density of *Prestoea decurrens*, the authors could cite measurements of wood density for the study species directly (0.31 g cm^3), which is available here:

<https://datadryad.org/stash/dataset/doi:10.5061/dryad.j2r53>

L 359-362: Is it also possible that the posterior PDFs do not change much from the priors because of some characteristics / amount of data going into the models? Attributing this fact to some reason seems like more of a discussion point than a result.

L 449: 2 cm yr⁻¹ increment in DBH is extremely high and I don't know where this number comes from? The abstract for the paper cited (Brandeis 2009) says, "...growth rate averaged... 0.36 cm/year in subtropical wet/rain forests, and 0.20 cm/year in lower montane forests." (<https://www.fs.usda.gov/treearch/pubs/34208>). This value is also more than 2x higher than the maximum DBH growth rate shown in Figure 10. Something here seems to need clarification.

L 455: Change "the ones" to "the experiments"

L 465-467: The growth rates shown here for late PFT trees are quite a bit higher than what is typical in these forests (see comment about L 449).

DISCUSSION

In general, this section is very short and it feels like there is a lot of work to be done in terms of putting the pieces together for a robust interpretation of the study results. Also

the discussion focuses almost entirely on the modeling exercise but extremely little points back to the biology of the system.

L 486: I would like to see a brief introduction to the discussion section that quickly summarizes the key findings and provides a structure to what we can expect to read in the rest of the section.

L 510-517: RE: clumping factor: It is not clear what are the implications for the clumping factor. The value controls LAI but what does that mean for the simulated dynamics?

L 529: "...vegetation dynamics."