Associate Editor's comments:

Dear authors,

Thank you for submitting your manuscript to Journal of Ecology. I have received reports from two referees, and, although we all agree that the data are impressive, the reviewers pointed out several shortcomings, that I’m afraid are no easy to address.

Overall, the conceptual framework is weak, the threat of the Introduction is fussy which affects the structure and clarity of the whole manuscript, and, as reviewer 2 highlights, the paper is poorly referenced. The definition of the intermediate disturbance hypothesis based on the experimental treatments is weak, as the entire disturbance gradient is unknown and thus deciding which level of disturbance is intermediate is arbitrary. To me, it’s not clear how the three treatments describe a gradient in succession and allow to test the IDH hypothesis. I agree with reviewer 1 that the study is rather inconclusive regarding the main hypothesis. Also, many statements lack of support. For instance, why evenness reveals underlying ecological processes? Line 66-67

Below, I refer to you the reviews, which I hope will be useful during the revision process.

Best wishes,

Natalia Norden

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Reviewers’ comments:

Reviewer: 1

COMMENTS FOR THE AUTHOR

Based on a 30-year monitoring in 12 tropical rain forest plots where three treatments representing a disturbance gradient were applied, you examine taxonomic and functional trajectories to analyse community resilience and test the intermediate disturbance hypothesis. Your study is interesting and represents a great effort, but in my view it lacks proper theoretical depth given the nature of the questions asked. A particularly worrisome problem is the definition of intermediate disturbance based on your experimental treatments, as the entire disturbance gradient is unknown (or at least not disclosed) and thus deciding which level of disturbance is intermediate is arbitrary. Your study is conclusive about the resilience of the community attributes analysed, but it is rather inconclusive regarding the intermediate disturbance hypothesis. I also found many problems with the language; not being a native speaker of English myself, I feel sympathetic and thus I present below many suggestions for improvement. Yet, I remain concerned for other conceptual problems, for example the interpretation of the second order true diversity index employed by you, and the idea of niche availability in the habitat, which is inconsistent with the widely accepted notion of the Hutchisonian niche, prevalent in current ecological theory. I think that solving all these issues adequately would require a very big effort.

Here I share with you numerous comments that I hope may be useful in preparing a revised version of your manuscript.

Key words:

L34. The phrase “Determinants of plant community diversity and structure” is too long to work properly as a search term. Could you perhaps provide a more concise term?

Ok, “community diversity determinants”

INTRODUCTION

L41. Change to “in forests products” to “of forest products” ??? “Non-timber forest product” = dedicated term.

L44. Change to “and time of community structure, composition and function” Ok

L49. Change to “cornerstones” Ok

L59. Change to “community diversity” Ok

L60. Change “ stating a maximized species diversity” to “, which predicts maximum species diversity” Ok

L65. Change to “forest recovery” Ok

L67 and L68. Change “that” to “which” Ok

L71. Change to “community diversity” Ok

L82. Add a comma after “Eventually”; change to “community response” Ok

L95. Why “questioned”? Unless you are challenging some previous assertion about the speed of recovery time, I believe that you here you have “examined” or “analysed” recovery time, rather than “question” it. Ok

I check all ”communities” -> community

MATERIAL AND METHODS

L98. Do these figures represent correct geographical coordinates? The symbol for degrees is missing, and please insert a space between the seconds symbol and the bearing. Ok, to check coordinates

L99. Again, to comply with SI norms and rules, insert a space to separate the symbol from the number and use the complete symbol for degrees Celsius (26 °C). Ok

L100 and L101. Again, please comply with SI (Système International d’Unités) rules and separate all SI symbols from the units. Also, please do not use dots (periods) to joint two units (symbols). ???

L102. “5 and 50 m a.s.l.” Ok

L104. Change “generating” to “, which produces. Ok

L105 Insert a space between 625 and ha

L109-L110. From Table 1, it is clear that the criteria to remove trees were equal or greater certain diameter. In these lines you say that trees were removed above a given diameter. This is inconsistent; please make sure that there is no ambiguity regarding these criteria. Ok, equal or above is the right criteria

L111-L112. Delete the article and start the sentence with “Disturbance intensity...” Ok

L116, Table 1. It is the norm to indicate a given diameter by putting DBH on the left side of the equation, not the other way around. In this manner, this quantitative information is easier to grasp. Also, please delete all dots between “trees” and “ha^-1” and replace them by a space. If dots are replaced by space: it is still a unit!?

L105-111. The table suggests that disturbance intensity was applied in a nested fashion, and on Line 105 you talk about a disturbance gradient. The text and the table do not match well, and therefore all this section needs to be rewritten to achieve a better explanation of he experimental treatments. Ok

L118. Should this section not be presented before the experimental treatments? It looks kind of odd here. I don’t know: first the experiment and second its botanical survey

L120. Family names need not be written in italics. Also, replace the word twelve by the number 12. Ok for twelve, botanical names are in italic

L125. Add a comma after “species” Ok

L126. Change “palm trees” to “palms” (or “arborescent palms” if desired). Ok

L130. Change to “This resulted in...” Ok

L131. Change to “uncertainty” (singular) L139. Change to “leaf thickness, “ L142. Please provide a reference or more information about this project. Ok: weblink has been added

L154. Change to “To account for taxonomic uncertainty”, or use a different word if this in not what you mean (I do not understand well what you mean). Ok: rephrased

L155. Having monitored these plots for 30 years, it is hard to understand why the taxonomic composition needs to be analysed at the genus level. This is undoubtedly an important drawback of the study, given the amount of taxonomic precision lost with this decision. Yes, but we had no choice

L159. Change “later” to “latter”. Also, spell out the number 7. weblink has been added

L164. The sentence “Species seed mass corresponded to 5 classes of increasing mass” is totally unclear to me. What do you exactly mean? That you created five seed mass classes? Or what sort of correspondence are you referring to? Rephrased: Species seed mass were given in 5 mass classes

L167. In the description of the meaning of the true diversity indices based on Hill’s numbers, the order 2 index (2q) represents the number of dominant species, not evenness. Unfortunately, this interpretation is pervasive throughout the text. In comparison with the richness, it is the evenness: “The comparison between richness and Simpson diversity assessed the variations in taxonomic evenness”-> is this sufficient?

L169. Add a comma after respectively Ok

L171. Add the definite article “The” before Rao index L174. Please capitalise “Spearman” Ok

L175. Change “extrema” to “extremes” Ok

L177. Add a comma after “evenness” Ok

L179. Delete the article and start the sentence with “Functional redundancy” Ok

L181. What do you mean by a 2-dimensional plan? A plot? A plane? More importantly, however, PCA assumes linear relations among variables, thus this analysis is not the most appropriate to your purpose.

‘in a plane with the two first axis from a PCA analysis’ Why is it not approriate?

L181-186. I realise that this is explained with more detail in Supplemantary Material, but I still find this paragraph very difficult to follow and I wonder if it should not be expanded a little. Ok: better developed now.

L185. I guess you mean “removed from the community” Ok

RESULTS

L188. Change to COMMUNITY COMPOSITION (i.e., not in plural) L190. The plural of genus is genera. Please correct accordingly. Ok

L196. Change “average” to “mean”, both for WSG and SLA L198. What do you mean by unachieved recovery? Incomplete recovery? Unsuccessful recovery? I am not familiar with this expression but its meaning is not intuitive. Ok, incomplete kept

L199. Change to “correlated with” Ok

L200. Capitalise “Spearman” at all times; also, please add a comma before “respectively” Ok

L201. I am not sure this is correct. On Figure 1c I see higher values at an age of about 13 years. Am I wrong? Ok, rephrased: “The maximum dissimilarity with the initial was reached for taxonomic composition between 15 to 25 years after disturbance, for most of the plots, and around 22 years for functional composition »

L204. Change “flora composition” to “species composition” or “floristic composition”. Also, try to simplify this figure legend: “...floristic composition (a, c) and functional composition (b, d)... Lower panels (c, d) represent...” Ok: I took”taxonomic composition” to keep same appellation as previsouly

L211-213. I assume you are talking about CWMs, but this is not clear. Of course, the maximum height at adult stage of any species (which is a recognised functional trait) does not change along time. Ok: “Community CWM average value of”

L215. Change to “recovered to its initial value... maximum difference from their initial values” Ok

L216. Delete the article “the” (disturbance intensity) L219. Add a comma before “while” Ok

L216. I think you should stress the fact that some of the correlations were negative. Ok: “Positive correlations were observed for Leaf thickness, chlorophyll content, SLA and bark thickness (Negative correlation was observed for Leaf toughness, WSG and Hmax. ().”

L221. This figure (and most others) have very poor quality.

L222. Change to “community weighted means”. Also, spell out number four. Ok

L224. Add a comma after “thickness)” Ok

L226. Change to “COMMUNITY” Ok

L227. Change to “richness and evenness”, but more importantly, see my comment above on the interpretation of the 2q true diversity index. => In the results: evenness was replaced by “Simpson diversity” or “Functional Rao diversity”. The evenness interpretation was kept for the discussion only. **ALSO CHANGE GRAPHS IF AGREED.**

L227-228. Change to “30 years of monitoring” Ok

L232. Change to “correlated with” Ok

L239. Change to “difference from”

L240. Change tro “taxonomic richness (a)” Ok

L243. Add a comma after evenness (and see may comment above). More importantly though, did you not remove this outlier from the analysis? To what extent may this outlier be responsible fro your findings and conclusions? Could this be formally assessed?

L244. Delete “30” Ok

L246. Add a comma after “intensity” and change to “maximum being positively” Ok

L259. Add a comma before “respectively” Ok

L268. The wording in the final part of this sentence is awkward. Do you mean that recovery had not yet attained initial values? Yes: ok I changed

L270. Poor quality figure. Also, delete the upper title and explain the units on the Y axis. The X label should be ‘Time since disturbance (years)’, i.e., variable name and units, as usual.

L278. The phrase “marked before disturbance” is cryptic. Do you mean that such differences were already apparent before disturbance? Yes: reworded

L281. Change “that partly determined” to “which partly determined” Ok

L283. Does initial state recovery define resilience? Whose definition of resilience have you adopted for your work?

As indicated “Resilience, , in the sense of the recovery of initial state,“

L84. “hardly”? What about “seldom”? “rarely”? ok: rarely

L287. Why “mirror”? Surviving trees are the only representatives of initial community, but I am not sure that this makes them mirror it. « the composition of pre-disturbance surviving trees is representative of … »

L290. Here I see another major conceptual flaw. Since the mid-20th century, the large majority of community ecologists adopted Hitchinson’s multidimensional concept of niche. According to this concept, niches represent axes of requirements and environmental tolerances, thus niche is a concept linked to species and no niches can thus be “available” in the habitat. In this context, the phrase “environmental niches” is totally lacking in meaning. Ok: “benefitting from light, space and nutrient resources made available by”

L295. Add a comma after “Thereafter” Ok

L299-L300. Is this you working hypothesis? If so, why did you present it before?

L305. What about “A NEW PERSPECTIVE” Ok

L306. The word “well” is repeated. Delete it once. Ok

L306 and elsewhere. I am really concerned about you work being a study aimed as supporting or discarding the Intermediate Disturbance Hypothesis. For your analysis and interpretation, your treatment 2 represents an intermediate condition of disturbance between treatments 1 and 3. You had no other choice. Yet, what can guarantee that for the community under study such treatment is really intermediate? For one, I can imagine that total obliteration of the forest would represent the most intense scenario for disturbance, and then your treatment 3 could be intermediate or even mild. Of course, this is a problem faced by many studies attempting to provide evidence for or against IDH, but like most of them, I do not think that you solved this issue satisfactorily.

L309. Delete “The” and begin the sentence with “Disturbance intensity” Ok

L313. The abbreviation spp. needs not be written in italics. Ok

L316. Change to “Beyond the intensity threshold, disturbance decreased the taxonomic...” Ok

L320. Change “slighter” to “milder”, and see my comments on evenness (also on L325, L329, L342, L372). Ok

L327. Add a comma after “intensity” and delete the article before “recruitment”. Ok

L334. I find that the expression “long term” for your study period is a bit forced. Perhaps “mid-term” would better match this period, particularly considering the life span of the organisms involved. Ok, changed in the keywords

L343. Long-lived pioneers? Yes

L348. The term “commensurately” is awkward. Ok: significantly adopted

L349. Change to “was not compensated for in the first place” Ok

L357. Change to “so that the time to full recovery” Ok

L360. I suggest changing to “mid-term impact” Ok

L364. Better use the term “keystone species” Ok

L373. Change to “, which were enhanced” Ok

L376. Change to “prevented in the first years after disturbance” or “the initial stages of recovery” Ok

L378. Change to “of tropical forests, defined in terms of recovery to pre-disturbance state” Ok

REFERENCES

I think that you missed this important reference for your study:

Bongers, F., Poorter, L., Hawthorne, W. D. & Sheil, D. (2009). The intermediate disturbance hypothesis applies to tropical forests, but disturbance contributes little to tree diversity. Ecology Letters, 12(8): 798-805.

Ok, added and introduced (discussion)

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Reviewer: 2

COMMENTS FOR THE AUTHOR

This paper presents an impressive dataset that spans 30 years of succession following selective logging in a tropical forest in Paracou, French Guiana. The paper presents both species and functional data, and compares observed patterns what would be predicted from the Intermediate Disturbance Hypothesis. This is a substantial dataset and there are some nice results, however I don’t think the results are particularly novel (see Bonnell et al. 2011, Baraloto et al., 2012; Carreño-Rocabado et al., 2012 etc) even when set against the IDH. In addition, the writing and the statistical analysis could be improved. I present some more detailed comments and suggestions below:

1. The writing in this manuscript needs to be edited by a native English speaker. It is incorrect in places but more frequently the phrasing is clumsy such that it limits the interpretation. I initially corrected these errors but stopped after line 75. There are also some rather sloppy errors in referencing and formatting.

2. The phrasing of the first part of the introduction suggests that there will be a broad introduction to tropical forest recovery from disturbance (including, clearance for agriculture, clear-felling, selective logging, fire etc) however the subsequent references are limited to those of the authors and their collaborators from a subset of selectively logged sites. This is a narrow view and studies from different sites that have experienced different disturbance regimes are needed. There are many meta-analyses and broad analyses that present data from multiple secondary forest sites – see Poorter et al. 2016, Norden et al. 2015, Chazdon et al. 2007. I also disagree with the point that we have little information about species richness and compositional change, and again here references are lacking – see Carreño-Rocabado et al. 2012. The evidence suggests that species richness recovers relatively rapidly but composition is very unpredictable – again see Norden et al. 2015, and also Martin et al. 2014. Other studies have investigated both species and functional composition in selectively logged forests – see Carreño-Rocabado et al. 2012, Mouillot et al. 2013 among others.

3. Can the aims/ achievements at the end of the introduction be rephrased to hypotheses? The current statements are rather vague.

4. The statistical approach did not conform with my expectations for this type of data set – see Laliberte & Legendre (2010, Ecology) for details on analyzing functional diversity metrics. I think mixed-effects models could be used to excellent effect here to quantitatively assess differences among different disturbance regimes.

5. Generally in the figures – can T1, T2 and T3 be changed to low, intermediate and high disturbance intensity? This would make interpretation easier.

6. Some of the description of results seems somewhat misleading, or over stated. See line 195-196 – “In disturbed communities, these compositional changes corresponded to shifts towards species with more acquisitive functional strategies, from communities with high average WSG to high average SLA and chlorophyll content (see appendix I)” - Surely the trajectories in Fig 2. suggest that a proportion of the original community remains but light-demanding species are selected for initially after wood removal and then when these species grow up and canopy closes again, there is selection for more shade-tolerant species. I think this is what you suggest later in the discussion, I think the results could be better represented here. And is the reference to Appendix 1 correct here in line 195-196?

Minor and editorial comments:

Line 14: Change to “tropical forest biodiversity” Ok

Line 15: Change to “in the context of global change” Ok

Line 30-32: Change to present tense Ok

Line 40: diverse – not diversified Ok

Line 40: delete “the” Ok

Line 41-42: “for forest products” – not in forests products Ok

Line 42: Define current global changes – in what? Ok: climatic changes

Line 44: Use spatial and temporal dynamics – not dynamics in space and time Ok

Line 44: Replace communities to community Ok

Line 50: Change ecosystems response to ecosystem responses Ok

Line 75: Odd wording here “was

successfully adopted

”… rephrase Ok: “largely”

Line 75-84: This section needs rephrasing and some more detailed explanation

Line: 281 – not only “determined the pool of recruited species” but also made up a large proportion of the community (60 - 90%) due to residual stems

Line 301-303: This pattern is often seen in tropical forest recovery, see Dent et al. 2013; Arroyo-Rodriguez et al 2017 among others.

Line 306: Repetition of “well” Ok

Lines 306 – 309: Rewrite this long and confusing sentence Ok

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