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Montpellier, 28th of July 2019

Subject: Answer to Round #1 after major revisions

Dear Editor,

We are pleased to send back our manuscript (<https://doi.org/10.1101/351197>) after major revisions on the text. We acknowledge that both reviewers have provided an in-depth and remarkable review of our work, with numerous comments and suggestions. We believe the reviewing process has strongly improved the manuscript. Following the advice of both reviewers, we have removed references to eco-evolutionary processes in the manuscript, and we have shortened the description of statistical analyses by been more specific on the overarching goals of our analysis. Following the advice of the second reviewer, we redesigned our introduction into the broader perspective of the role of intraspecific variability (ITV) in ecology. The question of the ecological mechanisms underlying the patterns of variations is more specifically addressed. We also largely used the framework proposed by the second reviewer relative to resource acquisition vs resource use strategies to better discuss the variation of functional traits vs growth trajectories. By following the suggestions of both reviewers, the discussion part and the overall manuscript have been shortened (previous version = 7,376 words; current version = 6,842 words).

The two questions addressed in this study have been reformulated and are explained at the end of the introduction. Therefore, the introduction, the discussion, and the abstract has been largely rewritten, according to the major suggestions of the reviewers.

In the text with all the listed reviewers' comments we address each point of the two reviewers and include further explanations of the modified text where necessary. As the number of modifications in the text is larger, we also provided the word file with all comments, and the history of modifications in comparison with the past version. The comments of the first reviewer are in blue, those of the second reviewer are in red.

On behalf of all the authors, I state that all the material is original and that no part has been submitted for publication elsewhere. All authors have agreed to submission. We declare to not have any source of conflict of interest.

We thank both reviewers and the editing staff for the suggestions and ideas that have improved our manuscript and we look forward to hearing from you.

Yours sincerely,

Sébastien LEVIONNOIS

Number of words (main body)	6842
Number of references	74
Number of figures	3
Number of tables	3
Supporting information	4 appendices and 4 figures

Major revision needed

The manuscript has now been reviewed by an external referee and me. It has been very difficult to find reviewers who accept to evaluate the manuscript (28 have been invited!), and I apologize for the delay resulting from this issue. Several of the contacted people mentioned that they did not feel competent enough to understand and evaluate the relevance of the analyses on architecture. I think it reflects the need to make the manuscript more accessible to a broad audience of ecologists.

Therefore, I recommend substantially revising the manuscript following these general guidelines:

- to better underline the general context, questions and ecological mechanisms addressed in the study. **Specifically, the beginning of the introduction is much focused on the specific environmental context of Amazon forests, while you should first introduce your questions on the role of ITV in terms of traits and growth trajectories in a more general context.**

We agree. We now give less importance to the specific environmental context of Amazon forest, and recast the beginning of the introduction to the broader perspective of the role of ITV in community assemblages.

- to shorten the presentation of results on architectural analysis, and to keep the main aspects that are most relevant to understand the ecological value of the architectural patterns related to environment,

Results on architectural analysis are now significantly reduced, as we have transferred autocorrelation analyses and annual-shoot-related analyses to the appendices.

- to discuss more specifically the ecological mechanisms underlying the fact that environment influences or not growth patterns and functional traits. The logic of Discussion section should be improved, in order to make clearer your conclusions on the nature and role of ITV.

We agree and tried to take into account this remark. The discussion part was thoroughly re-written, with a dedicated part to the ecological mechanisms. As the discussion was rewritten, its logic was improved, by keeping the perspective of the nature and role of ITV.

My review below further includes more specific comments and suggestions. They are coherent with and complement the comments of the second reviewer.

If you wish to submit a revised version of your manuscript, please provide point-by-point answer to the comments and detailed explanation of the changes made in the revised manuscript.

Reviews

Reviewed by Georges Kunstler, 2018-09-04 10:10

This manuscript explores the intraspecific variability of both functional traits and growth and architecture trajectories between two soil types of contrasted fertility in a generalist tree species in French Guiana. The contrast between poor white sand soils and more fertile soils (such as Ferralitic soil) is a very important contrast structuring the diversity of tropical forests, with a substantial species turnover between these two soil types. These turnovers largely result from specialist species that are restricted to one soil type. There are large differences in traits composition between these two soil types, with a trade-off between growth vs. defense against herbivore (Fine et al. 2004 Science). There is a small number of with-sand endemic species that represent a high proportion of the forest in these poor soils, but there are also several generalist species that can be found at low abundance in these communities (Fine and Baraloto 2016 Biotropica). **This manuscript explores what are the mechanisms that allow these generalist species to establish in the harsh condition of the white-sand communities.** The hypothesis tested with *Cecropia obtusa* is that intraspecific traits variability within these species mirror the interspecific traits variability and thus allow the species to establish in both high and low fertility soils. The authors analyze both the leaf and wood traits and reconstruct growth and architectural trajectories of the trees to test their response to soil type. They **show that soil types have little effect on leaf and wood traits but impact the growth and architectural trajectories. This is a nice example of the analysis of tree architecture to reconstruct individual growth trajectories.** Overall I find the study interesting and based on solid analysis, but the text is sometimes unclear and need improvement. Below I give some general comments:

- I'm not a native English speaker, but I think the English need to be improved.
[Before any potential publication, we will rely on a professional translator for English improvement.](#)
- The presentation of the statistical methods is sometimes unclear. I think it would help to give an introduction sentence for each method explaining the overall goal of the analysis. The appendix S2 provides a better explanation than the main text. For instance, the section Page 11 lines 9 to 18 is unclear and need to be clarified. If I understand well you try to find regular pattern showing the impact of the dry season to date the internode, is it right?
[We focused on this point and gave on introduction sentence for each methods. For the methodology associated to the architectural analysis, which is certainly the most difficult or less known, we gave an introduction paragraph summarising the main steps of the analysis and the overall goal.](#)
- Given that you have soil information for each individual (or cluster of individuals) it would be interesting to include soil effect as a continuous variable based on the key fertility variables (or PCA of soil data) rather than just using the contrast WS vs FS.
[We took this remark into account: the ANOVA analysis was abandoned in favour of linear models by using the first axis of the soil PCA as the environmental variable.](#)
- In regard to the discussion on whether the intraspecific variability of generalist is larger than specialist, Sides et al. (2014) American Journal of Botany could be an interesting reference to add.
[We thank M. Kunstler for sharing this reference, and we have now included the reference in our manuscript.](#)
- Several sections of the introduction and the discussion emphasize the role of genetic adaptation whereas you have no data on this topic in your MS. I would downscale these sections.

With hindsight now, we agree that the role of genetic adaptation is out of the scope, and totally abandoned references to this point in both introduction and discussion.

Minor comments

- Page 1 line 15: “UMR LerFoB” change to “UMR Silva”, no?

Yes, the correction has been added. The writing of our manuscript started before this change.

- Page 2 line 6: “.measured “ delete the point.

Done.

- Page 3 line 9: delete “and on” and replace by “over”?

Done.

- Page 3 line 12: “temporal-scaled retrospective analysis” “temporal-scaled” is unclear delete?

Done.

- Page 3 lines 13: “soil phenotypic variability” unclear.

The all sentence has been re-written.

- Page 4 line 6: the references are variable in structure along the manuscript you need to correct that. In addition on the line, you use both “Steege” and “ter Steege” you need to be consistent.

This variation has been corrected all along the manuscript.

- Page 4 line 18: Delete “across-community” and replace by interspecific.

This point has been abandoned for the new version of the manuscript.

- Page 5 line 14: “a mosaic of the genetic and functional divergences of populations” corrects this sentence.

We totally abandoned references to genetics.

- Page 5 line 33: correct for “are INTRASPECIFIC functional traits variability shaped by ...”

This part has been thoroughly rewritten. But we are now more specific.

- Page 6 lines 12-14: Several papers have explored the ontogenetic plasticity of traits (for instance LUSK, C. H. (2004), Leaf area and growth of juvenile temperate evergreens in low light: species of contrasting shade tolerance change rank during ontogeny. *Functional Ecology*, 18: 820-828.).

This part has been partially rewritten page 6 lines 9-31 (cleaned.doc version of the manuscript).

- Page 6 line 21: “caveats” is it really the good word here?

We changed by the first sentence of the paragraph: “Here, we aim to elucidate the role of ITV in functional traits and growth patterns in allowing species to thrive in different environments by studying...”

- Page 6 line 28: “functional ecology-based approach” unclear.

This part has been partially rewritten, but this term is no longer used.

- Page 6 lines 30-31: Sentence “The ITV On genotype” sentence unclear.

This sentence is no longer relevant since we abandoned references to genetics in this paper.

- Page 8 lines 33- 34: “a single colonization pulse” for both soil types?

“on both soil types” has been added.

- Page 10 lines 22-24. Sentence unclear.

The sentence has been made clearer accordingly.

- Page 10 lines 27-31: The way you computed leaf lifespan is unclear please clarify.

We detailed the methodology and re-wrote the sentence accordingly.

- Page 11 line 27: Why is it pertinent for old trees?

Indeed this sentence is not pertinent, we removed.

- Page 11 line 31: change “in (Davis, 1970)” to “in Davis (1970)”.

Done.

- Page 12 lines 14-16. The sentence “As for classical k-means ... of partitions choice.” sentence unclear clarify.

The presentation of the method has been simplified accordingly.

- Page 12 line 20. PCA of which variables, traits only or soil as well?

Precisions have been added accordingly.

- Page 12 lines 21-25: you have two sentences repeating the same idea.

They were not, but I acknowledge that this was not clear : the first one indicated that an ANOVA was conducted on the factorial coordinates of each individual of the PCA, whereas the second one indicated that an ANOVA was conducted on trait values in relationship with soil type.

However, the second one is no longer relevant since we changed the ANOVA analysis on trait values by a LMER analysis to test the effect of soil on trait variation.

- Page 13 line 19: Appendix S5 or S2?

The correction has been added all along the manuscript.

- Page 15 line 34: “bi- or an annual periodicity” correct.

This sentence is no longer present in the new version of the manuscript.

- Page 18 line 34: “is allowed” change to “could result from two non-exclusive...” ?

This has been corrected for the new version: page 14 lines 33-34 (cleaned.doc version of the manuscript)

- Page 19 line 30: Delete the first name from the reference.

The format of cited references has been reviewed all along this new manuscript.

- Page 29 line 1: “dimension architectural traits” only “architectural traits”?

This has been corrected accordingly. “Dimension architectural traits” are now named “tree-level trait” too avoid ambiguities.

Reviewed by François Munoz, 2018-10-29 07:53

The structure of the Introduction does not best underline the general scope of the paper. The first paragraphs on P3 are about specific environmental factors likely to most constrain community assembly in Amazon forests. When starting reading the manuscript, it gives a feeling that the manuscript is very focused on a specific context of these forests. **But I think the manuscript is about more general questions on the nature and role of ITV in species and community dynamics.** Therefore, I would suggest **first introducing your general questions on the issue, and then come to present how the Amazon forests provide a relevant context to test the hypotheses.**

We followed this remark and tried to recast the introduction to the broader perspective of the role of ITV in community ecology. The Amazon context comes after.

There is much focus on the evolutionary background and genetic variation potentially related to ITV (especially on P5 of Introduction). **But it is clearly outside the scope of the paper.**

We have now removed references to genetics.

You should rather focus on the ecological mechanisms related to ITV irrespective from the genetic basis. See for instance the conceptual framework proposed in Cyrille Violle et al., « The return of the variance: intraspecific variability in community ecology », Trends Ecol Evol 27 (2012): 244-52. The main point that should better be underlined in the Introduction is **that classical functional traits do not necessarily represent well the intraspecific variation in ecological responses, and that development and growth trajectories should be also acknowledged.** The issue is even more general and could also concern the analysis of BTV (a point that could be mentioned in Discussion).

We followed this remark and explained how classical functional traits, generally more representative of the resource acquisition strategy, fail to capture other dimensions of tree functioning such as the resource use strategy.

There is lack of explanation in Introduction on why and how functional traits (basically leaf and wood) and growth trajectories are influenced by soil variation. Some more precise explanation of the underlying mechanisms and some related references would be needed.

We followed this remark and tried to be more explicit on the potential underlying mechanisms of soil effects.

It would help discuss whether and why growth trajectories are more sensitive to environmental variation in the context of the present study. In addition, it is important to note that leaf/wood functional traits and growth trajectories represent different biological properties. Functional leaf and wood attributes can be related to the ability to get and store resources **at a given time, while the growth trajectory represents how the resources are used over time.** Therefore, the absence of functional trait variation across environments is not necessarily inconsistent with a variation in growth trajectories. A variation in growth trajectory and biomass storage can reflect changing metabolic activity over time irrespective from a difference in leaf and wood attributes. You should consider mentioning some works who specifically addressed the relationship between functional

traits and growth, such as Anaïs Gibert et al., « On the link between functional traits and growth rate: meta-analysis shows effects change with plant size, as predicted », *Journal of Ecology* 104, 5 (2016): 1488-1503.

We acknowledge that this framework is pertinent. We largely recast our study within this framework.

One of the questions asked in the paper is whether intraspecific trait variation (ITV) is consistent with between-species trait variation (BTV) across environmental contexts. However, the study is focused on analyzing ITV in a single species, and we lack information on BTV in the communities where *Cecropia* individuals are sampled. The authors mention results from other studies ("usually found at the interspecific level", L21), but there is no quantitative information on it.

We think there is a misunderstanding, probably because of a lack of clearness in our first draft. Our primary question was not about a quantitative comparison between ITV and BTV across environmental context, but rather to ask if the direction of variation was homologous between intra- and interspecific levels. More specifically, at the community level, WS trees generally have high LMA and wood density, contrary to FS. We expected finding the same pattern at the intraspecific level for *Cecropia obtusa* trees. We tried to be more explicit in this new draft to avoid this issue in the future. This point is underlined by the second question addressed in this paper, at the end of the introduction: "Does the *C. obtusa* functional divergence between soil types parallel functional divergence between soil types at the community level?"

Because the authors state that ITV is comparatively weaker than BTV, it is important to provide some more quantitative comparison. Because there is not study on BTV in the same sampling plots (as I understand it), **the limits of the comparison should also be discussed.** Another option is to tune down the ITV and BTV comparison, and to focus on the idea that ITV of functional traits is far lower than ITV on growth trajectory.

According to the reviewer suggestion, we preferred to tune down any ITV vs BTV comparison on a quantitative basis. The main focus of this paper is that ITV of functional traits is lower than ITV of growth trajectories.

Detailed comments:

- **Title:** I would suggest changing the title to better underline the take-home message. For instance, "Growth trajectories better reflect the influence of soil variation than functional traits in a widespread neotropical tree"

We keep it. Thanks!

Abstract:

- P3L2: "environment-driven" is not very clear. The sentence concerns trait variation across species,

which indeed represents adaptation to different contexts, but the term "environment-driven" also suggests environmental filtering driving trait composition of communities.

We change for the more appropriate "trait-environment relationship"

- P3L6: "are retained" is not very clear. I would suggest an alternative wording such as "we assessed whether intraspecific variation was consistent with interspecific variation across contrasted environments".

This first part of the abstract has been completely re-written.

- P3L11: here it is not clear why you "also examined the architectural development". You should introduce a specific hypothesis motivating the analysis of architectural development. It should allow better characterizing and understanding intraspecific variation in ecological response to changing environment.

The sentence has been re-written to incorporate this suggestion : « To understand how soil types impact resource-use through the processes of growth and branching, we examined the architectural development with a retrospective analysis of growth trajectories »

- P3L13: "soil-related phenotypic variability" would sound better.

According to the previous comment of the reviewer, this sentence has been re-written.

- P3L13-15 seems unnecessary in the abstract.

According to the suggestion of the reviewer, we delete this sentence.

- P3L22: not sure that "retrospective analysis of architectural development" is easily understandable for most people. I would suggest an alternative such as "an analysis of growth trajectory based on architecture analysis".

This has re-written accordingly thanks to the reviewer, in the related part of the abstract (4th point).

- P3L24-25: this sentence is rather vague (what "can be captured" means by the way?), and it is a pity that the last sentence of the Abstract does not convey a more striking insight.

The sentence has been re-written to take into account this suggestion : « The architectural analysis improved our ability to detect the effects of soil types compared to measured traits, by better taking into account how resources are used across the tree's lifespan. »

Introduction:

- P4L3-7: you should include less references here.

Even if this part of the text is no longer kept in our revised manuscript, I took cautions to do not include too many references.

- P4L12: there is deeper soil on hilltops?

This point about hilltop vs bottomland has been removed for the new version.

- P4L18: not clear what "directional" means here. It seems that you simply talk about a contrast here (without specific direction).

This point has been abandoned for the new version of the manuscript.

- P4L19: "accruing" sounds weird.

As this part of the introduction has been rewritten this term is no longer present.

- P5L5: **there is an important idea that could be better introduced**: it is that BTV should basically be compared to the ITV of more generalist species. You should briefly explain why ecological generalism should be related to higher ITV, and to ITV likely to be congruent with BTV. Some references can be found on this issue.

We developed this point: page 5 lines 1-12, and page 5 lines 33-34 and page 6 lines 1-8 (cleaned.doc version of the manuscript).

- P5L8-13: although the question of evolutionary processes and genetic diversity is per se interesting, it is outside the scope of the present study (you do not investigate these aspects).

We totally abandoned references to genetics.

- P5L32: I don't think that the present study can address "eco-evolutionary dynamics", it is focused on ecological processes.

We totally abandoned references to genetics.

- P6L21: "bridge these caveats" sound weird.

We changed by the first sentence of the paragraph: "Here, we aim to elucidate the role of ITV in functional traits and growth patterns in allowing species to thrive in different environments by studying..."

- P6L31-32: "eco-evolutionary dynamics" is again outside the scope here.

We totally abandoned references to genetics.

- P7L1-3: the way you will capture soil-related variation in architectural development is elusive here, although it is a key point. The Introduction should formulate hypotheses on how architectural development should reflect the influence of soil variation.

For how variations of architectural development will be captured I tried to be more specific page 6 lines 32-44, page 7 lines 1-7 (cleaned.doc version of the manuscript)

For the hypotheses I tried to be more specific page 6 lines 9-31 (cleaned.doc version of the manuscript).

- P7L3: "variation" instead of "patterns"?

As the questions have been recast and rewritten, this point no longer exist. However, we abandoned the term "pattern" in this new version.

- P7L7: unclear what "phenotypic variance strategy" means.

We have remove this term in the new version.

Material and Methods:

- P8L10: the fact that there is rainfall difference among sites can be connected to the different results found among sites. The point should be tackled in Discussion.

We tackled this point in Discussion page 15 lines 33-34, page 16 lines 1-4 (cleaned.doc version of the manuscript)

- P8L17: "had grown... formed..."

Modification added.

- P8L21: "at the basis"

Done.

- P9L15: you mention here $11+18=29$ individuals while earlier you mentioned 32 individuals (P8L19).

Corrections added.

- P9L16: although the seasonal variation does not fall of the scope of the study, the fact to sample different individuals at different season can affect the assessment of ITV and should be discussed.

As stated in the M&M, we controlled for any seasonal effect on trait variation : page 9 lines 15-18 (cleaned.doc version of the manuscript)

« No seasonal effects on leaf traits were detected, and ontogenetic effects on traits were standardised, as presented in the *Statistical analyses part.*»

- P9L21: acidity = pH?

Done.

- P9L22: "content"

Done.

- P9L24: "correlations" instead of "auto-correlations"?

Done.

- P10L6-21: you should be more explicit on whether and how the measured architectural traits allow characterizing different growth trajectories and strategies. This is more clearly explained later in the manuscript: for instance, there are variations in annual shoot length but different possible contributions of internode length and node number. You should explain whether and how these measured variation represent ecologically different strategies.

We followed this suggestion and we added an explicative paragraph for this purpose. Page 10 lines 20-31 (cleaned.doc version of the manuscript).

- P11: this part is quite long and technical and gives the feeling a main objective of the paper is to provide a detailed architectural analysis (but it is not the case). I suggest to shorten it, and to include additional methodological information in Appendices. The elements kept in main text should synthesize the assessment of growth strategies and trajectories.

The methodology about auto-correlation coefficients has been included in appendix. Some elements unessential have been removed.

- P12L1-2: "higher organizational level..." is not clear.

This part has been re-written accordingly.

- P12L11-18: I don't understand why you use clustering here. Why not performing analyses of ITV across soil types based on the raw quantitative indexes or on the axes of the PCA performed on them? It is unclear why there should necessarily be well-defined clusters.

We are now more explicit in the text on why we rely on clustering. Page 12 lines 7-16 (cleaned.doc version of the manuscript).

Our goal was to directly appreciate the effect of soil on growth trajectory in themselves, which are longitudinal data by nature. Most of trajectories exhibit a non linear behaviour, with different phases (decreasing, increasing). In our point of view, there is no quantitative index able to take into account these variations within and across trajectories for a given trait (phyllochron, internode length, cumulated height). Performing kml is the only way we found for testing potential effects of soil types directly on growth trajectories.

- P12L25: why "proper" soil comparison?

This part of the sentence has been removed accordingly.

- P12L26: I am not sure to understand what "ontogeny-related effects on leaf trait variation" exactly means here.

We are now more explicit in the beginning of this paragraph: "Potential effects of seasonality and ontogeny on leaf and dimensional trait variation for Couнами trees were tested with a multiple regression analysis"

Results:

- P12L34: what "regardless" means? Do you mean "globally"?

We changed for "independently", now in Appendix S4.

- P13: as in M&M, the presentation of architectural analysis seems to be too much detailed.

You should synthesize more the basic information reflecting the variation in growth trajectories across soil types and sites. Additional results can be moved in Appendix.

We followed this suggestion. Results relative autocorrelation coefficient, as also to annual shoot length or the number of internodes per AS, have been removed in appendix for this purpose.

- P13L28: what do you mean by "based on confidence intervals"?

In order to compare mean trajectories of architectural traits between FS and WS, we do not know comparison tests applicable to longitudinal data. We thus relied on the calculated confidence intervals, plotted on the figure, to compare mean growth trajectories and decide of significant (or not) differences.

We are more specific about it in M&M page 12 lines 2-6, as also in Results page 12 lines 31-32 (cleaned.doc version of the manuscript)

- P13L31: expand AS here.

This has been done all along the manuscript.

- P13L32: "There was no significant difference"

Done.

- P14L4: "first years"

Done.

- P14L11: what "largest range of trajectories" means?

We are more specific on this sentence.

- P14L14-18: you already talked about the growth phases before. Maybe it is possible to avoid redundancy and present it only once?

We deleted the reference to growth phase in M&M accordingly.

- P14L18: it is unclear for me how the "optimal" number of clusters is defined. More details would be welcome in M&M, but anyway I am not convinced that a cluster analysis is really relevant (see above). The number of clusters is not equal to the number of site x soil combination, the mapping between the two could be formally tested.

More details have been added in M&M Page 12 line 9-16 (cleaned.doc version of the manuscript).

- P14L32: "in terms"

Done.

- P15L1: consider including "However, ..."

This changes since we abandoned ANOVA in favour of LMER linking to the use of the PCA axis for the soil gradient.

- P15L8: why not using the term "PCA" as you did before?

Done.

- P15L9-10: strange to say that an axis "cluster" individuals. An axis is related to a variation of individual scores with or without clustering.

We changed the sentence in this way: "The first axis (28.5 %) is driven by $C:N_{leaf}$, L_{pet} , A_{pet} , A_{leaf} and N_{leaf} ".

Discussion:

- The Discussion section is quite detailed but lacks a clear logic. The connection between subsections and the global message emerging from the different aspects presented here should be made clearer.

The questions asked at the end of the introduction has been reformulated. Thus the discussion has been largely rewritten.

- P15L21-23: the sentence is a bit misleading because you did not compare architectural and functional traits, but analyzed their variation with environment separately.

The beginning of the introduction has been rewritten, and this sentence has been rewritten.

- P15L26: this sentence is problematic because you did not quantify BTV, so you could not formally

compare your quantification of ITV to BTV in the same context. You should be more cautious on this point.

We are now more specific on what we wanted to express.

- P16L15: what is the ecological meaning of "is avoided" here?

We changed the formulation: "Reducing the number of nodes per AS would imply the increase of the phyllochron, thus reducing the number of leaves produced per year. Such mechanism would critically affect tree carbon balance, and is probably unselected as the contribution of a given leaf to the carbon balance is disproportionate in comparison to most of species".

- P16L24: not clear what "hindering their competitive abilities" means.

We changed by "decreasing their competitiveness".

- P16L27: the term "posit" is not appropriate here.

The idea developed in the sentence has been removed. We do not use this term anymore.

- P16L33-P17L2: this sentence clearly shows that eco-evolutionary dynamics cannot be addressed in this study. Therefore, you should not put emphasis on it in the paper (see also comments on Introduction).

We totally abandoned references to genetics.

- P17L14: "accruing" sounds weird.

With the rewriting of the discussion, this part has been removed. We no longer use "accruing".

- P17L16-19: (too) many references here.

With the rewriting of the discussion, this part has been removed.

- P17L21-23: I agree that it is an important perspective here, but you should formulate (here and/or elsewhere) more specific hypotheses on how soil variation should influence developmental trajectories. Otherwise, the point of this paragraph sounds a bit trivial.

With the rewriting of the discussion, this part has been removed.

- P17L27: you mean "community composition"?

Accordingly, I don't use this term in the new version of the manuscript, but rather « community composition ».

- P17L32: "It implies"

Done.

- P17L33: "high water use efficiency" is not explicit enough.

This has been done accordingly.

- P18L1: unclear what "according to species soil specialization" means. Is it that the functional changes in forest community composition across soil types are related to the attributes of specialists occurring in each context? Then we need to clarify that there are still generalists that should have ITV related to these functional changes, which is connected to point (ii) on "species colonizing both FS

and WS" and on the topic of the next subsection. The point on specialization vs. generalism needs clarification.

This section has been rewritten as: "These findings (*op. cit.*) highlighted how WS is a constraint for plant development, related to resource scarcity, and how it is an abiotic factor selecting for trait optima diverging from those found on FS."

Page 17 lines 20-22 (cleaned.doc version of the manuscript)

- P18L2: unclear what is your point about the bounded phenotypic range, isn't it obvious that it is bounded?

With the rewriting of the discussion, this part has been removed.

- P18L6: unclear what a "strong enough driver" would be. There is lack of some quantitative reference of the expected functional variation (based on BTV?). Same need of a reference on L9-10, "does not mirror the interspecific functional level composition".

With the rewriting of the discussion, this part has been removed.

- P18L7: maybe mention "leaf and wood trait", as it does not concern architectural features here.

With the rewriting of the discussion, this part has been removed.

- P18L8: "were differentiated"

With the rewriting of the discussion, this part has been removed.

- P18L11: "are generally found" is vague, and there is a single reference. Are there still exceptions?

With the rewriting of the discussion, this part has been removed.

- P18L14: missing year for Gleason et al.

With the rewriting of the discussion, this part has been removed.

- P18L15: unclear what "self-sufficient" means?

With the rewriting of the discussion, this part has been removed.

- P18L15-18: this is a general sentence, and it is not explicit that it explains the patterns observed in present study.

With the rewriting of the discussion, this part has been removed.

- P18L19: "in contrast" or "contrasting"

With the rewriting of the discussion, this part has been removed.

- P18L19-25: what is the point about "maternal habitat", it concerns here genetically inherited trait variation? Note my general point about the fact that distinguishing genotypic or non-genotypic ITV is beyond the scope of the study here.

With the rewriting of the discussion, this part has been removed. Moreover we totally abandoned reference to genetics.

- P18L26-27: this other study still concerned quite different leaf traits.

This reference is now discussed in another perspective page 18 lines 8-10 (cleaned.doc version of the manuscript)

- P18L34: "non-exclusive"

This has been corrected for the new version: page 14 lines 33-34 (cleaned.doc version of the manuscript).

- P19L6-7: "greater influence of hydric stress"

With the rewriting of the discussion, this part has been removed.

- P19L12: "stressed"

With the rewriting of the discussion, this part has been removed.

- P19L17: this section is about architectural variation and growth strategies, so the title should be more explicit on that.

With the rewriting of the discussion, this part has been removed.

- P19L18-19: "did not affect... drove..."

With the rewriting of the discussion, this part has been removed.

- P19L23: unclear what "carbon savings" ecologically means.

With the rewriting of the discussion, this part has been removed.

- P19L27: the idea of "minimizing carbon expenses" is not clear for me, as well as the idea of "optimizing". If there is less opportunity to acquire resources, there is less biomass production, but there is not a specific mechanism minimizing carbon storage. It can be more a constraint than a specific response. The term "expense" is also unclear here. The idea of these sentences should be clarified.

With the rewriting of the discussion, this part has been removed.

- P19L28: "heliophilous"

With the rewriting of the discussion, this part has been removed.

- P19L32: I guess it is rather "is not optimized".

With the rewriting of the discussion, this part has been removed.

- P19L34: "smaller stem/shoot increments"

With the rewriting of the discussion, this part has been removed.

- P20L1-9: these sentences are rather long and unclear. You could only state that greater variation in both functional traits and growth strategies were found in the site where the contrast of FS and WS were greater.

With the rewriting of the discussion, this part has been removed.

- Conclusions: as previously mentioned, I would put less emphasis on the eco-evolutionary aspects, and underline the need to better understand how ITV in terms of both functional traits and growth trajectories determine species dynamics and coexistence in different environmental contexts.

The conclusion has been shorten, and focus on the perspective of ecological processes.