Dear Editor,

Please find herewith our revised ms 'Identifying climatic drivers of tropical forest dynamics' for re-submission to Biogeosciences. We first would like to thank both reviewers for the high quality and seriousness of their review. We acknowledge them for that and we recognize that their works have contributed to improve the quality of our manuscript. Responses to their comments are in red in the following as well as are changes in the main ms.

We remain at your disposal for any further enquiries.

Bruno Hérault,

Reviewer 1 -

Scientific significance

This ms addresses the important question as to what climatic factors govern tropical tree growth and mortality in the tropical rainforest of French Guyana using a large dataset containing measurements on more than 20 thousand trees over a period of 20 years. This is a significant effort using one of the larger datasets available for growth and mortality of tropical rainforest trees.

Scientific quality

The analysis presented makes use of various statistical techniques to assess to how tree growth and mortality are shaped by various climate factors. It includes the use of PCA to unravel correlation between climate variables, and the development of a growth and mortality model, with maximum diameter, wood density, height and d13C as explanatory variables. This seems a valid approach. Then they proceed to apply a MCMC method (Markov chain, mote carlo) for estimating the climate influence on growth and mortality.

-> Thank you for your interest in our work.

Presentation quality

While the efforts of the authors to elucidate the growth and mortality dependence on climate variation can be applauded, the description of the methods, results and the following discussion can be much improved. While I think the performed statistics are sound, the authors need to explain better step by step what they did and why. The statistical methods, especially the MCMC-method should be explained more clearly and authors need to justify their choice of use of this particular set of statistical methods versus other methods, like multiple regression techniques.

-> We have tried to improve the way we present the chosen methodology. The main reason behind the choice of MCMC techniques (versus multiple regression methods) is related to the non-linearity of our coupled growth-mortality model. Multiple regression techniques are useful inference techniques but limited to linear or generalized linear modeling framework. We were not in this case because (i) of the exponential kernel of the growth model and (ii) the link between growth and mortality through the individual tree vigour. Because we have access to the global model likelihood, we used MCMC technics to find the parameter posterior distribution.

In the beginning of the model section, we have changed

"The model used in this study consists of a model coupling growth and mortality processes at the whole community scale. The likelihood is computed using the distribution probability of mortality (equations 1 and 2) and the computed growth rate (equation 3). A vigour index is added into the mortality process, taking the past growth into account. We added the climate variables into the two processes to highlight the links between some climate drivers and one particular process."

To

"The model used in this study consists of a model coupling growth and mortality processes at the whole community scale. The model is build taking advantage of two preliminary studies where the growth \citep{Herault2011} and the mortality \citep{Aubry-Kientz2013} sub-models were developped. The likelihood is computed using the distribution probability of mortality (equations 3 and 4) and the computed growth rate (equations 5 and 6). A vigour index is added into the mortality process, taking the past growth into account. We added the climate variables into the two processes to highlight the links between some climate drivers and one particular process. Because the final forest dynamic model was not linear, we build a MCMC algorithm under a bayesian framework to infer the parameter posterior distributions."

We changed the model inference section from

"We implemented a MCMC algorithm to estimate the model parameters \citep{Robert2004}. A random walk was used as a proposal distribution to sample new values of parameters that were or were not selected, using the ratio of Metropolis-Hasting. Only standard deviation was sampled in an inverse-gamma posterior distribution with a Gibbs sampler. All the algorithms and statistical treatments were implemented with R software \citep{RCoreTeam2014}."

To

"We implemented a MCMC algorithm to estimate the model parameters \citep{Robert2004}. A random walk was used as a proposal distribution to sample new values of parameters that were or were not selected, using the ratio of Metropolis-Hasting. Only standard deviation was sampled in an inverse-gamma posterior distribution with a Gibbs sampler. The functional traits used as demographical predictors were uncertain because botanical determination was incomplete for the older censuses, and not all values of functional traits were available for all species. We used the method developed in \citet{Aubry-Kientz2013} to handle these uncertainties. All the algorithms and statistical treatments were implemented with R software \citep{RCoreTeam2014}"

We move the Appendix A into the main text for more clarity. We believe that sufficient details are now reported to reproduce the study and we now refer to the 2 initial papers for those interested in more justification of the modeling choices. We are open to any suggestion from the editor to add more materials in the methodological section, if needed.

Why using univariate approach? The authors do not explain very well what the relative influence of the various climate variables is. They say what climate variables are associate with what, but how can the reader infer form the table 3 the relative influence. Thus, is it possible to give more information on what the values in the table 3 mean. How much of the variation in growth do they explain, and how can the reader see what variables most strongly influence growth.

-> We first use an univariate approach to make a first selection of the useful covariates. This is explained here:

"We included all climate variables one by one in each process of the model and computed the partial likelihood for each sub-model of growth or mortality we obtained. This provides a first result about the importance of each climate variable. Depending on their statistical significance and on their degree of collinearity from the PCA, we selected some climate variables and included them in the growth model and in the logit function of mortality."

We then have added two new columns to table 3 to see what variables most strongly influence growth and mortality, the effect size. The later was calculated by multiplying the amplitude of the observed variable to the absolute value of the estimator. The higher the effect size, the higher the importance of the variable in shaping growth and/or mortality.

Can the authors explain why they use this particular model definition? There seems to be no explanation or justification as to why this particular form of the model. Also the justification for the inclusion of the different functional traits can be more detailed.

-> We move the former Appendix A into the main text in order to explain how the model likelihood was build. The mortality model is a classical logit model widely used in forest dynamic modeling. The growth model belongs to the potential-reductor growth model family and is presented and justified in details in Hérault et al. {2011}. We now clearly refer to this paper.

How sensitive are the results for different model choices and for different algorithm setups? This might be crucial but I see no discussion on this. Please provide the readers with some insights on this.

-> We have a high confidence in the developed algorithms. This is due to the fact that the developed modeling framework succeeded in estimating the parameter values of the growth and mortality sub-models formerly developed. Added to the text: "The posterior values for obtained the functional trait parameters are coherent with results of \citet{Herault2011} and \citet{Aubry-Kientz2013}, increasing our confidence in (i) the developped algorythm and (ii) the biological determinisms of the ecological processes we want to model. This confirms that the functional trait-based approach could be successfully used to predict climate-induced tree dynamics in highly diverse tropical forests for which taxonomic data may be lacking but functional trait data are available."

What is the use of the table 4? It is not clearly embedded in the overall results, and reference to the table comes after table 5. The sentence at the start of section 3.3 is unclear. Interaction between WD max –WD and drought is negative (table 4) . . . is this an outcome of this study or of the literature? This part needs to be explained much more clearly. Currently it reads as if the table merely presents the findings of others, but the results are from this study I infer? Please explain this better in the results and give a proper discussion on the findings. Also mention the studies in the introduction and explain why they hypothesize this.

-> We realized that table 4 was poorly introduced. We now introduce it in the introduction and in the M&M part to explain the choice of the investigated interaction terms.

Introduction: " Finally, we include in the model some interactions between functional traits (wood density and tree size) and the climate predictors to test for a potential differentiated response depending on the individual functional identity. First, tree species having high wood density have

been reported to better resist drought events as compared to lower density ones \citep{Phillips2010}. Part of these differences are related to differences in hydraulic failure, as wood density is linked to xylem structure. Second, the current tree size also influence resistance to drought events or other climatic perturbations \citep{Nepstad2007,Condit2004}. Two main hypotheses are debated. First, small, young trees that are not well established and that do not have deep roots may be more sensitive and may suffer under stressful water conditions. Second, large, older trees may feel water stress because they must maintain their photosynthesis activities and carry sap to a higher altitude."

M&M: "Functional traits were introduced in the final model with an interaction term by multiplying a climatic variable with a functional trait. We did not test all possible interactions but, based on results from a literature survey, focused on biological-meaningful interactions (Table 14)." The former 'expected effect' column was based on results from the literature. The former 'result' column was from our modeling study. We made their names more explicit.

These results were already discussed in the subsection "water stress"

Conclusions can generally be more clearly written and put better into context. The importance of the results and findings should be emphasized much more clearly. In the current format I find it all a bit too concise. In the conclusion, three subsequent statements are made that require some references and justification. Please explain which studies say that dry seasons are becoming longer and stronger, which study says that precipitation is expected to decrease (some studies show increasing precipitation over the Amazon) and by how much is temperature expected to increase! Model predictions for climate in the Amazon vary widely.

-> We rewrote the conclusions to replace our findings in the current global change context.

"Global climate models converge to simulate, at least for the Amazonian region, a change in precipitation regime and temperature conditions over the coming decades \citep{Malhi2009}. Drought is expected to become longer and stronger in the future \citep{Joetzjer2013} and the temperature will continue rising drastically during the next century \citep{Stocker2013}. Our modelling framework allows us to study inter-annual variations of climatic variables and identify which of these climatic variables are the key drivers of tropical forest dynamics. Drought, precipitation and temperature were highlighted as strong drivers of tree growth and/or mortality. Drought decreased annual growth and mortality rates, high temperature decreased growth and high precipitation events increased mortality rates. Moreover, we demonstrated best resistance to drought for trees with high wood density and for trees with small current diameters, giving us some possible indications on the future composition of a tropical forest where droughts are becoming more frequent. In light of these results, raising awareness of the potential impacts of climate changes on tropical forest dynamics is urgent."

Specific comments. -The English phrasing can be improved throughout. Sometimes it is simply too concise to be sufficiently clear. -Page 3155, line 24. Diameter at maximum growth is attained for 0.794DHBmax." Please rephrase.

*-> Agreed and changed to: "Maximum growth rate is attained for a tree diameter equal to 0.794*DBHmax."*

-Nd under and Nunder are both used. Please check consistency.

-> Agreed and changed.

Also in general the ms would profit from less use of acronyms.

-> Agreed. We used a lot of functional traits and climate variables that indeed generates a lot of acronyms. We think that using the full trait and climate variable names may be worse for the reader but we are open to suggestions from the editor on this point.

-Page 3149 line 17 word "height" is missing.

-> Agreed and changed.

Page 3146 line 21. What is meant here with demonstrable success.?

-> Changed to "The seasonality of tree growth and tree mortality is consequently highly studied in tropical forests, with a lot of studies having succeeded in linking seasonal tree demography to climate seasonnality {Wagner2012, Grogan2012,Brando2010}"

-Page 3147 line 7 A consensus . . .. What is the consensus? Please be specific on what you want to say.

-> Changed to "Similar major mortality events were observed in Panama \citep{Condit1995}, in chinese rainforests \citep{Tan2013} or in South-East Asia \citep{Slik2004}. Water exclusion experiments in Brazil provide results in line with a deep impact of drought on tree mortality \citep{Nepstad2007, DaCosta2010, Brando2008}"

-A climate graph on the climate in French Guyana would be beneficial.

-> Agreed and added XXXXX

Reviewer 2 –

The manuscript entitled "Identifying climatic drivers of tropical forest dynamics" authored by Aubry-Kientz et al. assess the impact of climatic drivers on tropical forests. The data used by the authors are rather unique for the tropics (growth and mortality data every two years plus functional trait data) and allow asking questions that are very timely for current discussions (namely the effect of inter-annual climate variation). In general, I found the manuscript very interesting and relevant, and adding to our current knowledge. However, I have several comments that need to be addressed properly.

Specific comments

P3146, L1-6: I think that these two sentences need to be better link. In the first sentence you talk about the importance of looking at the impact of climatic drivers on tropical forest dynamics, and in the second one you explain the approach used. But you do not say that the approach used allows you to deal with the issue being raised in the first sentence. Just adding something like "to look at these we used ..."

-> Agreed and changed to:" In the context of climate changes, identifying and then predicting the impacts of climatic drivers on tropical forest dynamics is becoming a matter of urgency. To look at these climate impacts, we used a coupled model of tropical tree growth and mortality, calibrated

with forest dynamic data from the 20-year study site of Paracou, French Guiana, in order to introduce and test a set of climatic variables."

P3146, L15-18: this is a weak sentence for an introduction because the reader is not interested in the specific case of French Guyana, the reader is interested in knowing how common climate seasonality is in the tropics. Then you can argue (somewhere in the introduction) why French Guayana is a good place to look into this.

-> Agreed and changed to "Tropical forests are characterized by high annual precipitation and high evapotranspiration. Nevertheless, strong seasonal variations in rainfall inputs, partly driven by atmospheric movements related to the monsoon or latitudinal changes in the inter-tropical conversion zone, occur in most tropical regions around the world \citep{Feng2013}. Such seasonality…"

P3146, L19-22: I do not agree completely with the statement done in this sentence. Yes, there are studies looking at the effect of seasonality on tree growth rates but I think that most of these studies focus on smaller individuals and not on trees >10 cm in diameter.

-> Agreed and changed to "The seasonality of tree growth and tree mortality is increasingly studied in tropical forests, with some studies having succeeded in linking seasonal tree demography to climate seasonality"

P3147, L9-13: the sentence starting with "Besides exceptional. . .." is too complicated, and needs revision. I also think that the work of the Clarck & Clarck should be cited in here (as they have been working on inter-annual variation of growth).

-> Agreed and changed to "Between the time scale of exceptional events and the time-scale of intra-annual seasonal rhythmicity, there is a gap in our knowledge on the inter-annual scale. This gap is partly due to the weak magnitude of variation of the demographic rates when compared to what is observed from a seasonal point of view or to some spectacular events. This gap is also due to the lack of sites in tropical forests where annual regular inventories of tree growth and death are performed and where precise climatic data on the same time-scale are available. Moreover, the potential links between inter-annual climate variations and tropical forest dynamics should be studied from a multi-decadal long-term perspective in order to be representative of the climatic variability and of the variability of forest dynamic responses \citep{Clark2010}."

P3147, L17 and P3147, L20: new ideas are presented in these sentences that are not well connected with the previous ones. I do not see the need to use the models and their advantages to link functional traits with the effect of seasonality on growth and mortality rates. I think it is better if the authors write a new paragraph focusing on functional traits, and why they are relevant to the issues being raised. The models and their advantages can be included later on in the text; I would say when the questions/hypothesis are presented. Something like "the approach taken is . . ..".

-> Agreed. We wrote a new paragraph focusing on functional traits in the end of the introduction. We now present the advantage of our modeling approach just before presenting the research questions. "In this study, we use a modelling approach in order to mechanistically link climate conditions and functional plant traits to tree growth and survival \citep{Zuidema2013}. Functional traits have been recently used to include functional diversity in models of tree growth

\citep{Herault2011,Ruger2012, Wagner2014} and tree mortality \citep{Aubry-Kientz2013}. We first question the potential relationships existing between climate variables computed on two-year time step and forest dynamics. We identify independent variable responsible for the inter-annual variation of growth and mortality rates. These variables are then included in a coupled growth-mortality model to test their multivariate effects. Finally, we include in the model some interactions between functional traits (wood density and tree size) and climate predictors to test for a potential differentiated response depending on the individual functional identity. First, tree species having high wood density have been reported to better resist drought events as compared to lower density ones \citep{Phillips2010}. Part of these differences are related to differences in hydraulic failure, as wood density is linked to xylem structure. Second, the current tree size also influence resistance to drought events or other climatic perturbations \citep{Nepstad2007,Condit2004}. Two main hypotheses are debated. First, small, young trees that are not well established and that do not have deep roots may be more sensitive and may suffer under stressful water conditions. Second, large, older trees may feel water stress because they must maintain their photosynthesis activities and carry sap to a higher altitude."

P3147, L26 to P3148,L21: here you provide the effect of several climatic variables (several measures of drought, rainfall, temperature) on tree growth and mortality. I would mention these variables in the first sentence, and try to keep this as short as possible. For example, the description of REW and the specific results found when using REW are too much details for me. I would go for the more general picture.

-> Agreed. We tried to simplify the REW discussion. However, we prefer keeping it in the text because we think that it's important to distinguish precipitation (total over 1 year) from extractable water (water available for tree development).

P3148,L29: here you can mention the advantages of having a modelling approach.

-> Agreed and done.

P3149, L18: indicate to how many species the 20340 trees belong. This is important because in section 2.1.2 you indicate that you had functional trait data for 335 species, less than half of the species found in the area (700 species).

-> Agreed and done. Our dataset contained 642 species.

P3150, L2: the 335 species for which you had functional trait data, what percentage of the 20340 individuals do they represent? Or in other words, what did you do with the species for which you did not have functional trait data? How were they included in the analyses?

-> Agreed. The procedure used to assign functional traits to trees with no functional trait values is fully described in Aubry-Kientz et al. (2013). We now refer to this paper and add in the main text: "These 335 species represent 79\% of the total number of individual trees included in this study. We used the procedure described in \citet{Aubry-Kientz2013} to assign functional trait values to trees for which (i) the species is known but trait values were not available, (ii) the species was not determined at the species level and (iii) the tree was dead before being identified."

P3151, L11: can you indicate how the "past growth" is taking into account very briefly? Is it the growth rate of the previous year? Or of the two previous years? How do you define this?

-> Agreed and done.

P3153, section 2.5: did you also check for collinearity among functional traits? For example, height and diameter are very much related to each other.

-> Yes collinearity among traits was a problem. We checked it in our two former paper (Herault & al 2011 J ecol, Aubry-Kientz & al 2013 plos One). Basically, we used the Kuo & Mallick algorithm to select the significant predictors while avoiding multicollinearity.

P3153,L22-25: the relevance of wood density should go in the introduction, not here. You can use the arguments presented here in the paragraph focussing and justifying the importance of incorporating functional traits.

-> Agreed and done.

P3153, L12: what percentage of the inertia is explained by the second and third axes?

-> Agreed and added (46, 20, 12 % respectively)

P3155, L11-13: not clear why Aover was not included in the model and precipitation was included even when it did not have any effect on growth. Please explain the reasoning behind this decision.

-> Agreed. We now clearly explained that but both the effect size (Table 3) and the likelihood of Aover were the worst score obtained among all the tested climate predictors. To avoir overparameterization, we decided to not include this variable in the final model.

P3155,L13-14: here you said that Pre had an effect on growth and mortality but in Table 3 no effect of precipitation is shown. Which of the two is correct?

-> We're sorry for that mistake. We correct it. Pre had an effect on mortality only and was not introduced at all in the growth model. Thank you for spotting that.

P3155,L13: how come maximum growth rate increases with DBHmax and decreases with Hmax when DBHmax and Hmax are highly correlated?

-> Yes, this pattern is highly robust and we first saw and discussed it in 2011 in our J Ecol paper. We think that discussing that is beyond the scope of the current ms. But the idea is the following:

"Maximum growth increased strongly with d.b.h.max. Species that attain large sizes may realize faster growth rates simply because they intercept more light because of their taller stems and larger crowns. In contrast, the maximum growth was negatively related to maximum tree height. This counterintuitive result may be explained because, for a given d.b.h., species that attain great heights invest in rapid vertical growth rather than diameter increment."

P3156,L9: how come WDmax???

-> This is explained here (from M & M) "The term A_{under} multiplied by $(WD_{max}-WD)$ accounts for the effect of drought on trees with low wood density. This term is added in growth and mortality to test this effect" and we report it in Table 3.

P3156,L17: it is only here that I realized that you have used Bayesian algorithms. This is for sure related to lack of experience with the modelling approach that you have taken but it may help to be a bit more explicit about this in the methods.

-> Agreed and done in the model chapter.

"Because the final forest dynamic model was not linear, we build a MCMC algorithm under a bayesian framework to infer the parameter posterior distributions."

P3156, first paragraph of discussion: I think that the main and more general findings are here but I think that this paragraph could be stronger. Start with a sentence indicating the question that you are addressing (not just focussing in French Guyana), then report the main findings, and then finished up with the advantages of the method used and related issues.

-> We completely reworked the first paragraph and follow the suggested logical framework.

"In this study, we questioned the importance of the climate drivers of tropical forest dynamics by using a community growth-mortality modeling framework. First, one can note that few climate variables had an univariate effect when included in the mortality process, while almost all had an univariate effect in the growth process. However, the magnitude of the impact of climate variables is stronger in the mortality process (observed mortality rate varying between 1.6 and 2.5\% of mortality/2 years, while observed growth rates vary between 1.9 and 2.5 mm/2 years, Figure \ref{figure_couplage_clim_data}). Next, we developed bayesian algorithms to infer the multivariate nonlinear model and select the best predictors with a great flexibility. We found that drought decreased annual growth and mortality rates, high precipitation through soil water saturation increased mortality rates and high temperature decreased growth. We confirmed that the vigour index is negatively related to mortality, \textit{i.e.}, trees that grow more than expected have a lower probability of dying, and trees with lower-than-expected growth have a higher probability of dying. \textcolor{red}{Moreover, the posterior values for obtained the functional trait parameters are coherent with results of \citet{Herault2011} and \citet{Aubry-Kientz2013}, increasing our confidence in (i) the developed algorythm and (ii) the biological determinisms of the ecological processes we want to model. This confirms that the functional trait-based approach could be successfully used to predict climate-induced tree dynamics in highly diverse tropical forests for which taxonomic data may be lacking but functional trait data are available.} A limited number of interactions between climate variables and functional traits was tested because of our selection of three climate predictors. One can argue that some climatic variables that were disregarded in the first selection step would increase the likelihood if included in interactions with a functional trait. This pathological case is very improbable \citep{Wagner2014} and will necessitate an impractical amount of computational time to be tested."

P3156, discussion in general: I think that you have covered properly two of the three climatic variables included in your model, namely temperature and water stress (or drought). What about precipitation? Or is this being covered by the section "water saturation"? If this is the case, then make this more explicit.

-> Yes the second axis of the PCA is a water saturation axis very well caught by the Pre variable. We've made it more explicit, in the results and discussion chapters.

P3157,L25-28: please rewrite these sentences, they are not clear because you mixed your results (without providing them explicitly) with the results of the through-fall experiment. Please also indicate what reduction in rainfall you see in the driest season in your database.

-> Yes, we tried to make it clearer

"Experimental trough-fall exclusions conducted in Tapajos and Caxiua\~na indeed demonstrated that 50\% rainfall exclusion led to very high mortality rates \citep{Nepstad2002}. Our results show no positive effect of drought intensity on mortality rates (Table \ref{table_univ}) and look contradictory to \citet{Nepstad2002}. However, the natural variability of the drought intensity (total rainfall from 5486 to 6207 mm) in our dataset is hardly comparable to the experimental 50\% reduction in total rainfall."

P3158,L1-2: I do not understand this statement because you have 20 years of data that include some strong dry events and that should allow you to see long-term effects of drought events. So what other factors explain the positive relation between Aunder and mortality? What about a negative effect of water saturation on mortality rates? What about topography in your study area? See also comments below.

-> Yes, we rephrased this part to make clearer the fact that our modeling framework impeded us to see the effect of repeated droughts in the sense that the values of the drought variable depend only on the last 2-year climate. Changed to "Moreover, our modeling framework prevented us from seeing long-term effects induced by repeated drought events because the drought variable values depend only on the last 2-year climate."

P3158, section on water saturation: I think that this is an interesting paragraph, with convincing arguments. But I wonder why are you putting so much emphasis on water saturation (measured by Aover, I guess but it is never stated in this section) as it is not selected as a climatic variable and therefore, it is not included in the final model of mortality (Table 5). This variable is also orthogonal to Aunder, so you cannot translate the results with Aunder to Aover. So I think that you need to revise this paragraph and check how you can incorporate the main points being made in the previous paragraph. Maybe you can explain in this way the unexpected results of mortality and Aunder.

-> Yes, we now make clearer early in the results "that the second axis of the PCA is a water saturation axis and we select Pre *because of its large effect size in the mortality sub-model.*

"The second axis of the PCA is related to water saturation and is correlated with Pre (C=-0.68) and A_{over} (C=-0.61); only A_{over} had an effect when included in the growth process, \textcolor{red}{but both the effect size (Table \ref{table_univ}) and the likelihood were the worst score obtained}, which is why we did not include this variable in the final model. Concerning mortality, Pre had an effect and \textcolor{red}{is thus included as a proxy of water saturation in the final mortality model.}"

We recall that at the beginning of paragraph.

P3158,L10-13: please rewrite as the way it is written is too complicated. Say first that on average half of the trees die standing and half fall over. And then talk about how this ratio varies with precipitation. In figure 2 the relation between tree-fall mortality proportion and

precipitation is not significant and in the text you say that the relation is significant. Which one is corrent?

-> Yes we've adopted your suggestion.

"In the Paracou forest, about half of tree deaths are due to standing death and half to tree-fall. This ratio looks, albeit non-significantly ($R^{^2}$=0.61, \textit{P}=0.08) because of the low number of observations (n=9)), linked with total precipitation. The highest total precipitation pre 2-yr led to the highest proportion tree-fall deaths (Figure \ref{chablis})"

We did not say that the relationship was significant. In fact, it's hardly significant but this is clearly due to the low number of observations (9). This is clearer now.

P3159, L8-13: I think that this is a very important aspect and contribution of your work. I would make it a separate paragraph and expand it more (to highlight the implications of your results).

-> Yes, agreed and done

"Indeed, as temperature was identified as a strong predictor of growth, all else being equal, averaged community growth and forest productivity may consequently decrease in time. This decline in productivity in time is perhaps what we are starting to see throughout the Amazon \citep{Brienen2015}. As no consensus has been reached yet, additional studies using regular inventories are urgently needed \citep{Reed2012, Corlett2011} to explain the conflicting patterns of the temperature effect found in the extant literature \citep{Dong2012}. Finally, we need to acknowledge that we do not know much about how forest dynamics will behave in the next century under temperature conditions that will be so different from what is actually observed. In this context, manipulative warming experiments are increasingly vital to better predict the future of tropical forest dynamics \citep{Cavaleri2015}.

P3158,L20: what you do mean with the "time dimension"?

-> Changed to "time variation in soil water saturation"

P3159, L20: change to "Current climate predictions suggest (or indicate) that drought will become longer and stronger....." P3159,L22-23: but are we not aware already about the potential impact of climate change on forest dynamics? I would say that we are aware of it, so the concluding sentence of your work should be something else. Need of more information? Other similar studies? Relevance of the approach used?

-> the conclusion was fully reworked following recommendation from R1 and you.

Table 1: can you indicate which of the traits belong to the categories you have provided (leaf and stem ecomomics and life history)?

-> Agreed and done.

Use m as units for Hmax as it is more standard to use m rather than dm.

-> we prefer keeping dm because the values of the model parameters (table 14 & 15) depend on this unit.

Table 2: say explicitly in the table that the values presented in column RANGE are values over two years. Otherwise some of the values do not make sense (e.g., precipitation). There are too many decimals in some of the variables (e.g., cloud cover, daily mean temperature).

-> Yes, agreed and done

Table 3: include "demographic parameter"

-> Yes, agreed and done

Table 4: I would refer to this table earlier in the text as it does include several of the hypothesis that you are discussing.

-> Yes, agreed and done

Table 5: what is "DoptDBHmax", "KWD"? And what is the role of foliar composition on growth rates?

-> We're sorry for this typo. We now precise in the growth model which variable is included in the exponential kernel. The role of foliar composition in growth rates was extensively discussed in Hérault et al. (2011, J Ecology). Overall, leaf traits are of minor importance in shaping growth rates. Stem and life-history traits are of primary importance.

Figure 1. Also mentioned that you are plotting years in the plot. And do the location of the years in the biplot make sense? Are years to the left of the first axis drier years than years on the bottom of the second axis? And by the way, I count here 10 double years but in the other figures I only see 9 points. Why the difference?

-> Agreed. Yes location of the years on the biplot makes sense with the driest 2-years being to the left for instance. Our forest dynamic dataset is from 1991-2011 so that we have 10 climatic periods. But because to calibrate the whole forest dynamic model we need to have the past growth to compute the vigour estimator (estimated from the precedent census), the full model likelihood was computed over the 1993-2011 period. That's why in the following graphs, 9 double years only are reported.

Figure 2: indicate the test being used for the analysis. Is this a correlation or a regression? None of the p-values are significant based on the r2 provided but you only state this for the relation between standing mortality proportion and Aunder.

-> Yes, agreed and corrected.

"In the Paracou forest, about half of tree deaths are due to standing death and half to tree-fall. This ratio looks, albeit non-significantly ($R^{^2}$=0.61, \textit{P}=0.08) because of the low number of observations (n=9)), linked with total precipitation. The highest total precipitation led to the highest proportion tree-fall deaths (Figure \ref{chablis})"

Figure 3: I like these graphs very much. It would help if they would be larger as they are hardly readable at this moment.

-> Yes, we wil provide the vector file (.png) that will allow the publisher to choose the right size.

Technical corrections

P3146, L 1: change to “climate change”

-> Yes, agreed and done

P3146 L 25: change “estimator” to “proxy”

-> Yes, agreed and done

P3147, L1: start a new paragraph with the sentence “At another time scale. . .” because you are here focusing on another type of climate events

-> Yes, agreed and done

P3150,L7: change “playback” for “payback”

-> Yes, agreed and done

P3151, L23: change “are computed” to “were computed”

-> Yes, agreed and done

P3152, L23-27. This sentence is extremely long. You can make 2-3 sentences out of it to increase clarity.

-> We are sorry but we have not understood what sentence you refer. No line 23-27 on P 3152. Lines 23-27 on P 3153 contain 3 sentences.

P3153, L2: write in full MCMC algorithm

-> Yes, agreed and done

P3153,L14-15: change to “Two main hypothesis were tested using interactions effects (Table 4). Therefore, you can erase the last sentence of the paragraph.

-> This part has been moved to the introduction and has been rephrased, following recommendations from Reviewer 1.

P3154: I would not use subsections in section 3.1 (i.e., 3.1.1,3.1.2, 3.1.3). The section is not long for subsections to be needed,. The first sentence can also be removed, the information is repetitive.

-> We respectfully disagree with the reviewer. We think that our procedure to select the final variables (based on a PCA and univariate analyses) may be better understood with explicit subsection titles.

P3154,L17: change to “climate variables”

-> Yes, agreed and done

P3155,L4: erase “together”, not needed

-> Yes, agreed and done

P3155,L7: change to ". . . finally included ONLY temperature, which had a . . .. . . when it was included alone in the growth model".

-> Yes, agreed and done

P3155,L10-13: please divide this long sentence in at least two sentences

-> Yes, agreed and changed to

"A_{over} only had an effect when included in the growth process. However, both the effect size and the likelihood (Table \ref{table_univ}) were the worst score obtained so that we did not include this variable in the final model"

P3155, L22: maybe worth mentioning what Herault et al. 2012 found. Otherwise the sentence does not make sense.

-> Yes, agreed and changed to

"Parameters linking the maximal growth to the functional traits $DBHmax$, WD, $Hmax$ and $\delta13C$ have have similar values and interpretations to \citet{Herault2011}, \textit{i.e.} maximum growth rates increase with increasing $DBHmax$, and decreasing WD, $Hmax$ and $\delta13C$ (Table \ref{table_estim})"

P3157,L24: not clear what "field observations" you are referring here. Please rephrase.

-> This part has been fully re-worked following recommendantions from reviewer 1.

Appendix A and Appendix B. Make sure that these appendices are cited in the text. I did not check it but I thought that Appendix 2 was not really used/described in the text.

-> Appendix A is now included in the main text following recommendations from reviewer 1. Appendix B was cited in the legend of figure 14.

And therefore, it is strange that figure 3 comes before other figures that are referred to in the text.

-> Yes, agreed and changed

Figures and Tables: not sure about the policy of the journal but figures and tables are not numbered in the order in which they are cited in the text (e.g., table is cited before table 2 or 3 have been referred to). The authors always say "(see Figure X/Table X)". Is this required by the journal? I would say that (FigureX) is the standard.

-> Yes, agreed and changed

Figures and Tables: not all tables and figures are cited in the result section, only in the discussion.

-> Yes, agreed and corrected.