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Interactive comment on "Disturbance legacies have a stronger effect on future carbon exchange than climate in a temperate forest landscape" *by* Dominik Thom et al.

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Anonymous Referee #1

REFEREE #1: General comments The manuscript deals with the legacy effects of disturbances (both natural and anthropogenic), and of future climate change, on the C balance of the forest. It is a relevant topic and provides new input to the field. The manuscript is well-written and the work has been done thoroughly.

AUTHORS: We thank the referee for his/her overall positive evaluation of our study.

REFEREE #1: The first part of the study is an analysis of possible interactions between

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two past disturbance events. Although I can appreciate the work that has gone into digging out the old archives, my impression is that the analysis was more exploratory in nature, while writing it up, one reference (Schurman et al. 2018) was used as a quick excuse for a hypothesis and the discussion is more focussed to find references on temporal autocorrelations at different time scales. Perhaps part of the material in the discussion should be transferred to the introduction to provide a more solid hypothesis (like the references in line 442/443), or no hypothesis should be given at all and the patterns found should be discussed against other findings in literature. A weak point here is that there were only two events, and no autocorrelation analysis could be done at different time scales. Furthermore, I'm not always convinced by the arguments the authors bring up in comparing their results to other studies. For example, they state that they find a low probability for the same area to be affected by the two episodes (line 443), which is in contrast to a study that does find correlations between episodes but at very different timescales. I think there is only a contrast if both studies were at the same timescale, and if not, they cannot be compared. Similarly, they state that other studies did find correlations at the plot and stand scale (line 450), but the authors attribute their different finding to the fact that they work at the landscape scale. I do not see why this would yield so different results. If you check a sufficient number of stands and find correlations, I would expect the same would hold true for the landscape. If not, you would expect low correlations at the stand scale as well. Also, lines 457-466 pose some possible reasons why the two events were different. I think they should have enough material to check some of these alternative explanations, or should be able to obtain them with little effort (for example wind direction of both events). Overall, I suggest the authors re-think their hypothesis and discussion for this part of the analysis.

AUTHORS: The referee makes an important point with regard to revising the hypotheses and the part of the discussion pertaining to the first part of our analysis. Given the lack of explicit data on past disturbance episodes comparisons as the one undertaken here are rare, which makes embedding it in the literature challenging. Furthermore, as some important characteristics of the first disturbance episode remain unknown (e.g.,

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exact wind speed, wind direction) some uncertainties about the causes of the difference between the two episodes will necessarily have to remain. Furthermore, we'd like to point out that an analysis of individual drivers of the Central European disturbance regime is beyond the scope of the current contribution, in fact the causes of natural disturbances have been investigated in detail already in prior studies (Marini et al., 2012; Overbeck and Schmidt, 2012; Pasztor et al., 2014, 2015; Thom et al., 2013). Nonetheless, we agree with Reviewer #1 that the correlation between the two disturbance events warrants further attention. Congruent with the suggestions of referee #1 and referee #2, we will add another analysis to investigate the contribution of forest management to the second disturbance episode. Based on our factorial simulation design, we will analyze the effect of four different combinations of previous natural disturbances and management on the second disturbance episode in 320 simulations (those including the second disturbance episode). This analysis will provide further insights into the drivers of the second disturbance episode. Following the referee's advice we will also reformulate our hypothesis and substantiate it with some of the material provided in the discussion section. Based on the results of the new analysis, we will also adapt the discussion in section 4.1.

REFEREE #1: The second part of the study deals with an analysis of the future effect of human and natural disturbances, and future climate change. I think this part of the study is well described and the conclusions are valid. The authors give great care to initialise their model in 1905 using an innovative method, and to simulate the conditions until now, and then project their model into the future. They conclude that the past trajectory is very important to understand the future carbon dynamics. Usually, models would be initialised according to the current state of the forest, and carbon dynamics projected into the future. The current state of the forest would in most cases represent past events, and legacy effects are thus already present. I'm wondering if the 100-year simulation of the past really influences the results, and that this would be a recommended procedure for all models, or that the correct representation of current state and current management is sufficient to include these legacy effects. I could imagine

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the authors use their new initialisation procedure to represent the current state and compare future projections with and without the 100-year historic run. Perhaps this is too much to add to the current paper, but I would encourage the authors to give some indications on this issue. Are the current initialisation procedures sufficient to take care of past legacies or are longer historic runs needed?

AUTHORS: We thank the referee for the positive evaluation of the second part of our study. The Reviewer is in fact correct in stating that usually legacy information is captured via the initialization of a model. This is not different in iLand, the model applied here. However, our point here is a slightly different one, namely: How different would the state of the forest (and hence the initialization of a simulation model) be if it would have had a different disturbance history? We thus quantify the structural effects of different past activities onto the state of the forest in 2013, and investigate how long these differences persist into the future, given everything else is equal. So the Reviewer is correct in assuming that if the initial conditions are known the legacies are adequately captured for modeling. However, in many cases the initial conditions of a forest landscape are incompletely known. This is for instance the case for the state of our landscape in 1905, for which we have information about species composition and growing stock, but not for other important variables (e.g., soil C pools, the spatial composition and configuration of stands). The legacy spin-up approach presented here was designed to address this very issue. In the revision we will revisit our description of legacy effects and reword/ amend it where necessary, in order to make explicitly clear what our contribution here is, and avoid confusions for the reader.

REFEREE #1: The ordering and numbering of the supplement is a bit strange. S2 and S3 are figures connected to text S1, S4 is text, while S5 and onwards are again figures. While reading the main text, the first reference is S4 while earlier supplementary material is referred to later. Perhaps the supplement could be ordered according to the appearance in the text, and a difference could be made between text and figures.

AUTHORS: We agree that the enumeration of the supplement can be improved. This

has also been suggested by referee #2. In our revision, we will restructure the supplement into sections (e.g., Section S1) and provide all figure with a consecutive number. We will take care that sections and figures will be numbered continuously throughout the text.

REFEREE #1: Specific comments

In Figure 1 it would be helpful to add a small map to show where the study area is located within Austria.

AUTHORS: We will complement the figure with an insert showing a map of Austria and the location of the landscape.

REFEREE #1: Line 152: does the model allow for build-up of beetle populations over the years?

AUTHORS: Yes indeed, the process-based bark beetle module implemented in iLand is able to simulate the build-up of bark beetle populations over years. Weather conditions affect the bark beetle population directly (e.g., the number of generations and sister broods per year, as well as winter survival rate). Furthermore, the vitality of trees and thus their defense capacity (simulated via carbon starvation) as well as the amount of windthrown tree (easily colonizable breeding material) influence beetle populations in the model. Seidl and Rammer (2017) found that iLand was well able to reproduce the 2nd bark beetle disturbance episode contained in our analysis here. We will add this information in the revision of the manuscript.

REFEREE #1: Line 285: I assume the weather data was adapted to the elevation gradient in the study area somehow? If yes, could you add one sentence about it?

AUTHORS: Indeed, elevation gradients are captured in the climate data used. The climate data from 1950 – 2099 were all statistically downscaled to a resolution of 100 x 100 m by means of quantile mapping. For the years 1905 – 1949, we had only temperature and precipitation from a nearby weather station. We thus drew a climate

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from the period 1950 – 2099 for each missing year by matching its temperature and precipitation data to that of the weather station record for 1905 – 1949. We will clarify in this approach in the revised version of the manuscript.

REFEREE #1: Line 356: Simulated species shares were compared against "independent" data for the year 1905. I think 1905 data were used to make the spin-ups for the model. If it is the same data, they are not independent. Or is it really another source? If so, please specify here.

AUTHORS: We agree with the referee that the comparison of the simulation with the observed data is not entirely independent, as the observed data was used to guide the spin-up procedure. We will change the text accordingly in the revised version of the manuscript.

REFEREE #1: Line 410: Is "stock" perhaps better than "storage" here?

AUTHORS: We will change to "stock" in the revision.

REFEREE #1: Line 487: You mention here that you only studied wind and bark beetles, while other agents may become more important in future. I think wildfire was included in your simulations as well. Moreover, you conclude that management was far more important than disturbances, i tihnk this needs to be highlighted here as well.

AUTHORS: Our simulations included disturbances from wind and bark beetles only as stated in l. 143 – 146 "As wind and bark beetles are of paramount importance for the past and future disturbance regimes of Central Europe's forests (Seidl et al., 2014a; Thom et al., 2013), we employed only these two process-based disturbance submodules in our simulations". Although it is correct that iLand is able to simulate disturbance from wildfire, we did not include wildfires here as they are not an important component of the disturbance regime in our study system. With regard to management we agree on its importance, and will highlight this more explicitly in the revision (see also our response to similar suggestions by the other Reviewers).

References Marini, L., Ayres, M. P., Battisti, A. and Faccoli, M.: Climate affects severity and altitudinal distribution of outbreaks in an eruptive bark beetle, Clim. Change, 115(2), 327–341, doi:10.1007/s10584-012-0463-z, 2012. Overbeck, M. and Schmidt, M.: Modelling infestation risk of Norway spruce by Ips typographus (L.) in the Lower Saxon Harz Mountains (Germany), For. Ecol. Manage., 266, 115–125, doi:10.1016/j.foreco.2011.11.011, 2012. Pasztor, F., Matulla, C., Rammer, W. and Lexer, M. J.: Drivers of the bark beetle disturbance regime in Alpine forests in Austria, For. Ecol. Manage., 318, 349–358, doi:10.1016/j.foreco.2014.01.044, 2014. Pasztor, F., Matulla, C., Zuvela-Aloise, M., Rammer, W. and Lexer, M. J.: Developing predictive models of wind damage in Austrian forests, Ann. For. Sci., 72(3), 289–301, doi:10.1007/s13595-014-0386-0, 2015. Seidl, R. and Rammer, W.: Climate change amplifies the interactions between wind and bark beetle disturbances in forest landscapes, Landsc. Ecol., 32(7), 1485–1498, doi:10.1007/s10980-016-0396-4, 2017. Thom, D., Seidl, R., Steyrer, G., Krehan, H. and Formayer, H.: Slow and fast drivers of the natural disturbance regime in Central European forest ecosystems, For. Ecol. Manage., 307, 293–302, doi:10.1016/j.foreco.2013.07.017, 2013.

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