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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 #2

REFEREE #2: This study depicts the past and future of a forest landscape in Austria. It aims at evaluating the respective weights of past natural disturbances, past human management, and future climate change on the forest capacity to sequester carbon. For this, the authors reconstructed the landscape history of the federal forest under study using historical data sources. This history is marked by a windstorm in 1905 followed by a bark beetle outbreak, technological evolution of management practices until 1997 when management is ceased, and a second wind and bark beetle event in

2007. The historical reconstruction results show that there is no correlation between the locations impacted by the first and the second natural disturbance events. In a second time, the authors designed a factorial simulations experiment in which the forest landscape under study undergoes all combinations of conditions : 1917 windstorm and bark beetle event or no, evolution of management practices between 1924 and 1997 or no management after 1924, 1997 windstorm and bark beetle event or no, four climate scenarios from 2013 to 2099. The simulations show that the net ecosystem exchange is dominated by past management found to explain 97.7%. The recovery from past management causes an increase in the future carbon storage. The authors find that by 2100 the effect of human and natural disturbances overcome the effect of climate change. The object of this study is interesting and timely as the issue of the response of forests to climate change becomes more pressing. The case study is interesting due to its particular history including two large natural disturbance events and a ceasing of human management that allow the analysis of the legacy of management practices on a forest landscape. The simulation experiment is well designed and the model used (iLand) is appropriate to address the questions raised and introduced in a satisfactory way. However, the results and discussion section are somewhat superficial and do miss some important points. Also, the way the study is presented is often confusing or misleading and impairs the comprehension and interpretation of the results. The display items as well as the presentation of the results should be reconsidered to enhance the impact of the work presented.

AUTHORS: We thank the referee for his/her interest in our study and the very thoughtful review with valuable comments to help us improve our manuscript. In the revision, we will focus on dissolving the confusing interpretation of results that were highlighted by the reviewer. See our responses below on how we plan to achieve this.

REFEREE #2: Detailed comments Terminology : " disturbance " My main concern is about the use of the word disturbance all along the article, from the title on. The use of this term disturbance is misleading. Usually disturbance refers to natural disturbance

(Overpeck et al., 1990; Seidl et al. 2014, 2011). In the present manuscript, it is sometimes used to refer to natural disturbances only (p4 L73 or L395) and sometimes to refer to natural + anthropogenic. It seems that the authors are aware of the confusion this creates, because most times they explicit that disturbances is natural+anthropogenic (ex : p5L86). Aggregating two very different processes such as management and natural disturbances, on top of being very confusing for the reader, impedes the discussion of one very important result which is the extreme dominance of the effects of management compared to natural disturbances on carbon sequestration of forests. To this regard even the title of the article is misleading or even incorrect since it is not the legacy of the natural disturbance events (explaining only 2,8%) but of past management that has a stronger legacy effect than climate change. The manuscript should be revised to account explicitly for this distinction in the processes analyzed which is obvious in the results.

AUTHORS: We thank the Reviewer for pointing this out. Our idea in the initial submission was to first combine natural and human disturbances to quantify the overall disturbance effect on carbon storage, and subsequently disentangle the partial effects of natural and human disturbances. As two of the three referees (referee #2 and referee #3) found the combination of natural and human disturbances into the overall disturbance effect confusing and problematic, we concede that this idea needs to be revised. In the revision we will clearly distinguish between management and natural disturbances throughout our study. We will rephrase the title of the study into “Legacies of forest management have a stronger effect on future carbon exchange than climate and natural disturbances in a temperate forest landscape”.

REFEREE #2: Methods In the description of the simulation experiment it is noted that each scenario is replicated 20 times (p15 L 347) ? The rationale for this should be explained. What changes between the replicates ? Is there a stochastic component in the model ?

AUTHORS: iLand is a process-based model including fully-dynamic submodules for

natural disturbances and forest management, and each of these components contain stochasticity (e.g., the spread of an individual bark beetle cohort from an infested tree is determined by drawing from a distribution of empirically determined dispersal distances, with spread distance drawn randomly between 0° and 360°). To account for this stochasticity, we have replicated every simulation 20 times. This particular number has been proven to be a good middle ground between determining robust results and keeping simulation times reasonable in previous applications of the model (e.g., Seidl et al., 2018; Thom et al., 2017). We will explain the rationale of the replicates more explicitly in the revised version of the manuscript.

REFEREE #2: L212: the sentence describing the 1905 age distribution seems a bit far-reaching from fig S8 as the bimodal distribution is not obvious, and the statement is very qualitative.

AUTHORS: We agree with the referee and will change the text accordingly.

REFEREE #2: Results and discussion The manuscript seems very unbalanced with 13,5 pages of intro and methods (both well written and with relevant content) and only 5,5 pages of results and discussion (2,5 and 3 respectively). As reflected by these numbers, the results and discussion sections are sometimes shallow compared to the information presented and the very large number of display items included both in the main text and the supplementary materials (8 and 12 respectively).

AUTHORS: We only partly agree with the referee on this point. We feel that it is important to include an extensive methods section in highly complex and computational extensive studies in order to ensure the highest possible degree of reproducibility (Scheller et al., 2011; Schwaab et al., 2015; Temperli et al., 2013). As 5 of the 8 figures as well as both tables are anchored in the results section, the manuscript is overall less imbalanced as it may seem based on text pages only. Moreover, besides the 5.5 pages for results and discussion, there is another page of text making up the conclusion section, which should be considered as well. With regard to the supplement we feel that the extensive additional material presented here helps the reader to understand our study and provides additional context on the validation and applicability of the methods used in our study (while not further clogging the main text). Nonetheless, we will further strengthen the results and discussion sections in the revised version of the manuscript through an additional analysis of the impacts of management and the first disturbance episode on the second disturbance episode (see our response below). Additionally, we will improve the clarity of information provided in the results and discussion sections based on the reviewer comments. However, we will refrain from omitting parts of the methods (which the referee agrees are relevant to understand the study) or prolong the results and discussion sections extensively (as our manuscript is already fairly long).

REFEREE #2: Some missing information : - 3.1 Performance of the reconstruction of past events:L377 " a good match" with reference to three supplementary figures, L379 " well able " with reference to one supplementary figure, L381 "small overestimation", L382 "corresponded well" with reference to one supplementary figure etc. all results from section 3.1 are qualitative and based on supplementary figures. An effort should be made to quantify the quality of the reconstruction and to present it in a concise manner in one display item, that, if judged crucial for the validity of the results should be presented in the main text.

AUTHORS: We understand the desire of Referee #2 for a single, concise evaluation result. However, we here follow a patter-oriented modeling approach (Grimm, 2005), which means that a variety of very different indicators are in order to evaluate the model's ability to reproduce the empirically derived historic data (i.e., tree species composition in 1905 and 1999, management, natural disturbances, carbon storage). In our opinion these cannot be combined into a single number/ figure, as such a combination may hide important information regarding model performance (e.g., the model could be doing very well wrt one indicator while performing poorly wrt a second one, which would give on average moderate performance; if the poorly captured indicator is, however, of particular importance for the study, this information would be lost in such an

aggregate evaluation). After careful consideration of the Referees comment we thus have decided to retain the multidimensional nature of our evaluation. In the revision we will explain this in more detail and provide our rationale for this approach for the reader.

REFEREE #2: - 3.2 Temporal interaction of disturbance events: the autocorrelation between natural disturbance events is described and found very low. No link is analyzed between disturbance events and management: is there a correlation between stands affected by natural disturbance and species? And age? And density?

AUTHORS: We thank the Referee for bringing up the issue of management in this context. In fact the possibility of a connection between management and the second disturbance episode has also been pointed out by referee #1, and we agree that this is an important issue here. We thus suggest to add an additional analysis in this regard. Following the advice of referees #1 and #2, we will investigate the contribution of forest management on the second disturbance episode in our revision. We will analyze the effect of all 4 potential combinations of previous natural disturbances and management on the second disturbance episode in 320 simulations (i.e., those including the second disturbance episode). This additional analysis will help us to investigate legacy effects on disturbances, and give further insights into the first disturbance episode on the second episode, as well as the effect of forest management on the second disturbance episode. We feel that such an additional analysis will considerably strengthen our submission further, and thank the Referee for suggesting it!

REFEREE #2: - 4.1 The discussion of the lack of autocorrelation between both natural disturbance events and the link to previously published literature is not always clear. For example, the authors state that their hypothesis was that older stands are more prone to wind and bark beetle damages (L442) and link this statement to the low probability of a same area to be affected twice. The fact that a stand is affected by a disturbance does not make it older hence more susceptible to a second disturbance. Several hypotheses are formulated to explain the lack of autocorrelation between both episodes as found in other studies, but none is backed by data so that the discussion is not convincing.

One hypothesis is that the longer and larger temporal and spatial scales analyzed here weaken the link found in smaller scale studies. I do not see why stands being more prone would not show up at the landscape scale. Similarly, the hypothesis of a dampening effect of a previous disturbance due to the resulting heterogeneity should be backed by minimal tests on the age and species structures of the affected and non affected stands. As well, the suggestion as to the difference in wind directions of both events needs to be investigated. In summary, an analysis of the characteristics of the stands affected by both natural disturbance events would enlighten this part.

AUTHORS: As highlighted by referees #1 and #2 we agree that this part of the discussion needs to be revised. We have tried to find other studies investigating the spatial autocorrelation of two consecutive major disturbance episode, but spatio-temporal autocorrelation of disturbances has been usually either described over very limited time frames (e.g., Pasztor et al., 2014) or the spatial resolution for the comparison of disturbances over longer time frames has been very coarse (e.g., Senf and Seidl, 2018). In this regard, our analysis constitutes a novel contribution, improving our understanding of disturbance dynamics over extended temporal scales. Although we have spatially explicit disturbance data for both events, we cannot conduct a process-based analysis at the level of individual drivers. The reason is that we do not know all the characteristics of the wind event of the 1917-1923 disturbance episode (e.g., wind direction and wind speeds) as these have not been faithfully documented. Moreover, we feel that the analysis of disturbance drivers is beyond the scope of the current contribution, as this has been investigated in more detail in other studies in Central European ecosystems (e.g., Marini et al., 2012; Overbeck and Schmidt, 2012; Pasztor et al., 2014, 2015; Thom et al., 2013). Nonetheless, we will improve the analysis of how past legacies have affected recent disturbances in the revised version of the manuscript. As mentioned above we will add a new analysis investigating the contribution of the first disturbance episode and forest management on the second disturbance episode. This analysis will serve to substantiate our finding of a weak contribution of one disturbance episode on the other, and provides more insights into the effect of forest management

on the Central European disturbance regime. Based on these results we will also improve the discussion in section 4.1 of the revised manuscript.

REFEREE #2: -4.2 disturbance legacies on future C uptake The authors argue that other studies of effect of climate change on carbon sink do not explicitly consider the legacy of past events. It is a bit surprising as past events' legacy in embedded in the initial conditions. The legacy spinup method derived here is interesting and relevant but should be placed in the context of alternative methods to describe forest initial conditions, see for example (Crookston et al., 2010; Garcia-Gonzalo et al., 2007; Hurtt et al., 2002; Karjalainen et al., 2002; Peng et al., 2009). The novelty of this study does not seem to be the inclusion of the disturbances' legacy but their quantification so this section should be rephrased. Several sentences are not backed by any reference and should be justified and developed. For example on L484, the sentence stating that these results may not hold for longer time frames, on L499 the sentence interepreting the simulation results as a change in forest types.

AUTHORS: We agree with the reviewer that the legacy effects are indeed embedded in the initial conditions, if the initialization is based on a comprehensive set of empirical data. It is also correct that the quantification of the legacy effect is the actual novel contribution of our study (see also our response to a similar comment of Referee #1). We will rephrase this section accordingly, and add references as suggested. We will also discuss the legacy spin-up method in the context of other established methods.

REFEREE #2: - effect of climate change It is not explained in many details what response of forest growth to climate change is simulated by iLand (with respect to species or altitude for example). The results shown here on the comparison of climate change and management are highly related to the processes included in the modeling exercise as correctly stated in L501-507 and would deserve a more in-depth explanation. A discussion section on the simulated response of forest growth to climate change only would help put the results in perspective.

AUTHORS: We agree that this is important information for readers in order to understand the results presented here. iLand considers both direct and indirect vegetation responses to climate change. For instance, temperature increases directly affect processes such as leaf phenology and the length of vegetation period, the efficiency of photosynthesis (modeled using a state acclimation approach following Mäkelä et al. 2008), and the availability of water in the soil (via altered evapotranspiration rates). Similarly, rising CO2 levels directly affect net primary production via CO2 fertilization. Thus, climate change might affect growth of one species differently than that of another species (direct effect), leading to a change in forest competition and structure (indirect effect). We will provide more details of the climate change effects on forest vegetation in iLand in the revised methods section. We will also explicitly refer the reader to the more technical iLand papers describing this issue (Seidl et al., 2012b, 2012a; Thom et al., 2017).

REFEREE #2: Display items Some display items do not help the understanding of the text, are redundant, or at the contrary lack information, and so should be rethought as material that supports the claim made in the text. Fig2 aims at summarizing the events included in the historical reconstruction of the forest landscape. Its design is more appropriate for a slideshow than a written article. Fig3 illustrates how the events shown in fig2 are included in the simulation experiments. Its design is confusing, especially with the 'n' that is cumulated from left to right (it takes some time to understand this) and that attempts at expliciting the factorial combination of the events simulated. These 2 figures could be condensed into a single display item where only the information relevant to the study would be presented. For example a table structured as below: Period / Scenarios' options / details 1905-1924 / disturbed / storm+bark beetle+ ... / undisturbed / 1924-1997 / managed / Technological improvements / unmanaged / Forest left to grow 1997-2013 / disturbed / / undisturbed / 2013-2099 / Climate scenario 1 / / Climate scenario 2 / / Climate scenario 3 / / Climate scenario 4 /

AUTHORS: As mentioned by the Referee, Figures 2 and 3 are highlighting two different

aspects of the study: Figure 2 represents the history of events on the landscape while Figure 3 shows the simulation design. We will explore combining these aspects into a single figure as suggested. Another option is combining the information into a single table, as suggested. We will revise our display items for clarity and remove redundancy where possible in the revised version of the manuscript.

REFEREE #2: Other problematic display items are Fig5, Fig6 and FigS14. These three figures are redundant and should be combined into a single figure that shows the time evolution of NEE attributed to climate, event1, event2, and management. Please explain 'cumulative NEE'. From fig5, since the climate driven cumulative NEE decreases it means that the forest becomes a source of carbon between 2035 and 2050? This pattern should be discussed (see comment on 'effect of climate change').

AUTHORS: The referee is right that there is some redundancy between these figures as the endpoints in Fig. 5 and Fig. S14 reflect the effect size in Fig. 6. Also with regard to the previous comment to distinguish between management and natural disturbance throughout the paper, it makes sense to combine these figures, and we will make these changes in the revised version of the manuscript. However, the interpretation of NEE by the Referee is not correct here. As NEE = -NEP an decrease in NEE means an increase of carbon in terrestrial ecosystems, i.e., between 2035 and 2050 there is an uptake of carbon by forests under climate change. We have provided a definition of NEE in l. 363f.: "NEE denotes the net C flux from the ecosystem to the atmosphere, with negative values indicating ecosystem C gain (Chapin et al., 2006)". The effect of climate change on NEE between 2035 and 2050 can be explained by more favorable conditions for tree growth (longer vegetation periods in the higher elevation parts of our mountainous study area) in combination with a CO2 fertilization effect, relative to baseline climate conditions. We will combine Fig. 5, Fig. 6 and Fig. S14 as suggested by the referee in the revised version of the manuscript. We will also improve the text with regard to the interpretation of NEE in order to avoid confusing interpretations by future readers.

REFEREE #2: Supplementary materials The supplementary figures are excessive. Some could be merged into a single figure such as Fig. S11 and S12 that show the same variable (growing stock per species). Some are not even cited in the text such as Fig S13. Fig.S5 is not clear, why showing two sites in the fictitious landscape map with only on stand development below. Letters A to D are shown but not used in the explanation but the outcome of the spinup (letter D I guess?) is not highlighted.

AUTHORS: Figures S11 and S12 show the same variable, but provide different aspects of the simulation. While Fig. S11 compares the simulated with the observed species composition and growing stock in year 1999, Fig. S12 presents the temporal trajectory from 1905 to 2013 of the simulation only. The temporal trajectory cannot be provided for the observed data as there are no records available at annual resolution. Hence, by omitting Fig. S12 we would omit crucial complementary information. Fig. 13 is cited in the text in l. 387f. "At the same time total ecosystem carbon increased by 40.9% (Fig. S13)." Letters A to D have been explained in the supplement in l. 139 – 150 "For instance, the initial planting could plant trees according to the target species shares (A in Fig. S5). During the simulation the defined management steps are executed (e.g., thinnings, B, final cut C). Periodically, the state of the forest is evaluated against the available reference data. A basic evaluation compares, for instance, the growing stock and species shares emerging from the simulation with the respective reference state, and calculates a similarity score (e.g., Bray-Curtis index). When the deviation between the emerging state space from the simulations and the reference state are not satisfactorily, the STP for the next rotation can be altered. In the example in Fig. S5, the simulated share of spruce was lower than the spruce share in the reference state, indicating that spruce was likely favored by past management, either by planting spruce (C) or by favoring spruce via selective thinnings. This information is incorporated in the spin-up run, which henceforth uses a modified STP for the given stand and the next rotation (D)." In our opinion, the supplement figures all provide unique and complementary information, and are important to understand our approach and evaluate model behavior. As these figures will only appear in the online supplement and not the

main paper, we do not see a reason for reducing them, and thus withholding the details of our model evaluation efforts from the interested reader. Regarding Fig. S5 we agree with Referee #2 and will extend the figure caption to facilitate its interpretability.

REFEREE #2: Technical details P3 L49 'Keenan and others' instead of 'et al' the numeration of the supplementary materials is confusing with only one line of numbering for text sections and figures. There should be section S1, section S2, section S3, figure S1, figure S2, figure S3, figure S4

AUTHORS: We agree with the referees #1 and #2 that the structure of the supplement needs to be improved. We also thank the referee for his/her close view on the text, pointing out a mistake in the citation style. We will follow the referee's suggestion to differentiate between sections and figures, and correct the citation style where needed.

REFEREE #2: Crookston, N.L., Rehfeldt, G.E., Dixon, G.E., Weiskittel, A.R., 2010. Addressing climate change in the forest vegetation simulator to assess impacts on landscape forest dynamics. For. Ecol. Manag. 260, 1198–1211. Garcia-Gonzalo, J., Peltola, H., Gerendiain, A.Z., Kellomäki, S., 2007. Impacts of forest landscape structure and management on timber production and carbon stocks in the boreal forest ecosystem under changing climate. For. Ecol. Manag. 241, 243–257. Hurtt,G.C., Pacala, S.W., Moorcroft, P.R., Caspersen, J., Shevliakova, E., Houghton, R.A., Moore, B., 2002. Projecting the future of the US carbon sink. Proc. Natl. Acad. Sci. 99, 1389–1394. Karjalainen, T., Pussinen, A., Liski, J., Nabuurs, G.-J., Erhard, M., Eggers, T., Sonntag, M., Mohren, G.M.J., 2002. An approach towards an estimate of the impact of forest management and climate change on the European forest sector carbon budget: Germany as a case study. For. Ecol. Manag. 162, 87–103. Overpeck, J.T., Rind, D., Goldberg, R., 1990. Climate-induced changes in forest disturbance and vegetation. Nature 343, 51. Peng, C., Zhou, X., Zhao, S., Wang, X., Zhu, B., Piao, S., Fang, J., 2009. Quantifying the response of forest carbon balance to future climate change in Northeastern China: model validation and prediction. Glob. Planet. Change 66, 179–194. Seidl, R., Schelhaas, M.-J., Lexer, M.J., 2011. Unraveling the drivers of

intensifying forest disturbance regimes in Europe. Glob. Change Biol. 17, 2842–2852. Seidl, R., Schelhaas, M.-J., Rammer, W., Verkerk, P.J., 2014. Increasing forest disturbances in Europe and their impact on carbon storage. Nat. Clim. Change 4, 806.

Please also note the supplement to this comment: https://www.biogeosciences-discuss.net/bg-2018-145/bg-2018-145-RC2- supplement.pdf

AUTHORS: Thanks for providing the references and the pdf which has been more convenient to work with than the online version of the text.

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