Biogeosciences

Discussions

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.

Anonymous Referee #1

Received and published: 1 May 2018

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.

The first part of the study is an analysis of possible interactions between 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.

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 sim-

ulation 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 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?

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.

Specific comments In Figure 1 it would be helpful to add a small map to show where the study area is located within Austria. Line 152: does the model allow for build-up of beetle populations over the years? 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? 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. Line 410: Is "stock" perhaps better than "storage" here? 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.

Technical corrections None.