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Interactive comment on "Soil CO_2 efflux from mountainous windthrow areas: dynamics over 12 years post-disturbance" *by* M. Mayer et al.

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Review #2:

The topic of the current manuscript is interesting and also important. Already decades it has been observed that in the conditions of climate change, heavy winds and storms are more frequent in our region. In today's Europe, wind disturbances are the disturbance type that influences the biggest land areas, thus it is important to have an idea/knowledge what consequences it has.

Below are my comments on the manuscript.

Author response:

We thank the reviewer for his/her constructive feedback, which we feel has made a genuine improvement to the manuscript. We have carefully addressed each of the specific comments, with our respective responses given below. Each reviewer comment is repeated, with the corresponding author response written beneath.

General comments:

The authors have tried to present the 12 year dynamics of soil CO2 efflux on mountainous windthrow area, and at the beginning I had an impression that they are really dealing with windthrow areas, but actually they are dealing with managed areas after the windthrow, as the material was removed from the areas - this small detail, that the material was removed, was coming out somewhere in Material and Methods, but it must be clear already when reading the abstract and it must be clear also when stating the objectives and hypothesis in Introduction. I would also consider some change in title, to make it clear already there, that we are dealing with forest areas that are managed after windthrow. I have also some concerns considering study design and how the data collected with such design was analyses and interpreted. It is obvious that these two areas (Rax and Höllengebirge) are so different from each other (soil, stand, climate conditions, etc.) that they must be treated separately and one must be really careful with conclusions like have been drawn out in Fig. 4. But in general the manuscript is interesting, language is good and fluent and the graphical part is also solid.

Author response:

In the common Austrian forest practice, woody debris (mainly the stem fraction) is usually removed subsequently to a windthrow event, mainly in order to prevent insect infestations. We agree with the referee's comment on clarifying the fact that these sites were managed after windthrow. We emphasized this issue throughout the whole revised manuscript. We added a detailed explanation of post-disturbance management to the Materials and Methods section. We nevertheless do not feel that we should change

the title. Both sites were disturbed by windthrow, regardless of post-disturbance management, and a significant amount of woody debris (about 15% stem fraction) was also left on site. Nonetheless, the information on how the sites were managed subsequent to disturbance is now clearly given in the text.

Specific comments:

P6384 L5-9: Like mentioned earlier, it must be clear already in Abstract that we are dealing with forest areas that are managed (material was removed) after windthrow.

Author response:

The suggested changes have been made. We now mention the management situation at the windthrow areas already in the Abstract.

P6384 L13-14: You are using two phases after windthrow (1-6 and 9-12 years after disturbance). How do we know that the soil was the same on these two areas? Maybe on the area 9-12 years after disturbance, the soil CO2 efflux was higher already from the beginning, straight after storm. When comparing the CO2 effluxes from control areas (both sites), we can see that the fluxes from Höllengebirge are much higher, it may affect and probably affects also the post-disturbance fluxes.

Author response:

It is true that we did not measure pre-disturbance soil CO2 efflux. However, tree species composition, stand age, stand structure, exposition, elevation, slope and soil characteristics were similar within the respective adjacent disturbed and un-disturbed areas. Therefore, it can be expected that pre-disturbance soil CO2 efflux rates were similar as well. We point toward this issue in the revised manuscript: "According to forest inventory data from both sites, pre-disturbance stand conditions (tree species composition, stand age, stand structure) of the windthrown areas were similar to those of the respective adjacent control stands. Furthermore, at both sites exposition, slope, soil types and humus forms were similar between respective disturbed and undisturbed

areas." In order to account for the site differences between Rax and Höllengebirge, we did not compare absolute soil CO2 efflux rates but rather looked at the post-disturbance trends in terms of relative contributions of soil CO2 efflux from disturbed areas compared to the respective undisturbed control stands (a more detailed response to that issue is given later).

P6387 L13: Where the areas totally damaged or partially damaged after wind disturbance? If the material was removed after windthrow, were all the trees removed (also the ones that survived the wind disturbance)? What about uprooted trees, how many of these you had in the areas – if the area was cleaned after windthrow with cable yarding operations (that is not damaging the surface so much in my idea), but you had a lot of uprooted trees there, with exposed mineral soil layers, this is affecting a lot soil respiration (specially if you have calculated annual sums later). All these things must be somehow mentioned here and described also in Material and Methods section

Author response:

We addressed and clarified all above mentioned issues in the new Materials and Methods section: "The windthrow areas at both sites were actively managed. Sites were partially cleared of stem wood immediately after the disturbance events in order to prevent insect infestations. About 15 % of the stem fraction was left in place. Branches and stumps were kept on site. Wind snapped trees were cut, and the logs were harvested as well. Only a marginal number of mature trees survived the disturbance events at both sites, which were not harvested after the windthrow. "We also rephrased the sentence (P6387 L13) to ". . .varying temporal stages after disturbance", in order to clarify the context of time.

P6387 L25: As we can see from here these are completely different forests (coniferous dominated and mixed forests) means also different site type and soil chemistry – how you can assume that the initial stage was the same, when combining this data later.

Author response:

It seems that our site description was not accurate enough and therefore was a bit too open to interpretation. Actually, the stands at Rax and Höllengebirge did not differ that much as coniferous tree species (spruce, fir), together with Beech, were dominating the canopy at Höllengebirge site. We clarified this in the new site description. Furthermore, soil characteristics of Rax and Höllengebirge forests were very similar. The soil characteristics of the two sites are now addressed in a new table (Table 1). We are very conscious about the general concerns of space for time substitutions with respect to the initial site conditions (see e.g. Pickett, 1989; Johnson and Miyanishi, 2008). Therefore, we are aware that the combination of two sites (Rax, Höllengebirge) presents some difficulties. However, due to the similar site characteristics and similar behaviour of the two sites post-disturbance (during the initial years after windthrow), we are still confident that combining the two sites provides a sound dataset from which longer-term development of soil CO2 efflux from windthrow areas in relation to undisturbed stands can be investigated. We therefore left Figure 4 in place.

P6388 L5-6: Among the other differences between the sites there is also huge difference in average air temperature – can this be a reason also for different soil temperatures?How this can affect your data interpretation and results? Think it was not mentioned also in Discussion.

Author response:

The mean annual air temperature differed between the sites, which definitely affected the soil temperatures. We agree with the referee that this issue was not addressed in the interpretation/discussion of the results. Nevertheless, for the interpretation of the long-term (12 years) post-disturbance trends in soil CO2 efflux, we did not use respective absolute values. Instead we were using relative effects compared to the undisturbed stand at the site (Rax, Höllengebirge). However, this comment opened our eyes to another potential pitfall in that the air temperature no doubt affects a variety of processes (e.g. vegetation growth and substrate dynamics) influencing post-disturbance of soil CO2 efflux. We therefore highlight this discrepancy in the revised manuscript

(Material and Methods), so as to present the data analysis in the most transparent way possible. Nevertheless, we wish to emphasise again that the level of difference between our sites is typical, rather than atypical, of the majority of published chronosequence studies. We thus maintain our stance that the two sites provide a scientifically solid dataset for studying the post-disturbance development of soil CO2 efflux.

P6388 L19: "blown over or suffered wind-snaps" – means there was windthrow with uprooted trees and broken trees (see my comment already on P6387 L13). Were the pits and mounds of the uprooted trees taken somehow into consideration – the CO2 efflux values from there are completely different compared to undisturbed forest floor (soil not exposed).

Author response:

We have not attempted to stratify into pits and mounds, but randomly select locations for the soil CO2 efflux measurements, and thus "catch" the average conditions of the disturbed sites. For the Höllengebirge site for example, the sampling design was set up on a digital map prior to installations. The coordinates of the sampling plots were subsequently localized in the field by means of a handheld GPS. The area of pits and mounts was generally relatively small ($\sim$ < 5 %) when compared to the whole disturbed stand areas.

P6389 L1: Here you are mentioning first time, that the area was cleaned after windthrow. It must be stated already earlier! Were the areas totally cleaned (also survived trees removed) or some trees were left to the area?

Author response:

We addressed that in the Abstract as well as in the Introduction now (see also response to earlier comments).

P6389 L9: It was stated that the sites were similar regarding bedrock and soil conditions, but we are missing here some basic soil parameters (pH, C stock, fractionation,

etc.) to state that. And obviously if we are dealing with pure coniferous stand and mixed stand, the soil pH and C stock may be different when comparing the sites.

Author response:

A table with soil parameters was added to the revised manuscript (Table 1).

P6390 L21: You mentioned that 65 plots out of 89 in Höllengebirge were used for further analyses. What about these 24, where they then used at all, if not why to mention them at all? Right now there is a lot of talk with 89 plots and then suddenly it was stated that only 65 was used – it makes the things confusing.

Author response:

Only the 65 plots were used in this study. We mentioned the other plots in order to give additional information for choosing such a specific (multi-stage) sampling design. We agree that the switch from 89 to 65 plots for the analysis seemed to come a little bit out of the blue. We therefore rephrased this paragraph to make things more clear.

P6390 L25: What is the definition of the plot in this study? How big it is? I can understand that on the plot there is one collar for soil respiration measurements and one 1x1 quadrat for ground vegetation measurements and somewhere also the soil temp. and moisture was measured and that's it. Is the plot and 1x1 square the same and where then the collar is located?

Author response:

The definition of a plot is a 1 x 1 m quadrat, where one collar was placed in the centre. Soil temperature and soil moisture was accordingly measured within a plot and beside a collar. This was not stated clearly. We rephrased this aspect in the Materials and Methods section of the revised manuscript.

P6391 L4: For how long the concentration increase inside the chamber was measured? 60sec, 120 sec? Why this time was chosen?

Author response:

The temporal CO2 increase inside the chamber headspace was measured, for either a maximum of 120 seconds or a maximum CO2 increase of 50 ppm. The recording interval of CO2 efflux [ppm] was 4.8 to 5 seconds. These were the standard settings from the company (EGM4, PP-Systems) which was shown to produce reliable soil CO2 efflux rates (e.g. Pumpanen et al., 2004, Agric. For. Meteorol. 123). This information has been added to the manuscript as well.

P6391 L11-13: This is one of the biggest problem in this work. If measurement cycles took 8 (14) h, and this was done with one day, then we have huge temp variation in these measurements? The temperature in soil changes a lot within 8 (14) hours. And you have stated that plots were measured in the same order through entire study, means some plots were always with much higher soil respiration then others (and this occurred through entire measuring period). Which ones where with the highest temperature? How the measuring order looked like? I'm concerned that this is strongly affecting your results and conclusions, but cant be sure before can have the description about the measuring order.

Author response:

Due to a comparable high number of sampling locations (plots) and a quiet large and steep study site (total size of the Höllengebirge site was 12 ha, average slope was $\sim$ 25 %) we had to find a compromise between feasibility of the measurements and randomness of the observations (from a statistical point of view). However, in order to guarantee a comparability of the three areas (treatments: HC, HW09, HW07) with respect to the time of the day, we changed between them every seventh plot throughout one measurement cycle. Within the individual areas, we thereby attempted to distribute the time of the measurements as equally as possible over the course of a day. We added this explanation to the manuscript as well: "The long duration of measurements at the Höllengebirge site posed the risk of bias due to changing soil conditions (temperature,

moisture) throughout the day. To account for that and to ensure comparability between undisturbed and disturbed sites, measurements were undertaken in uphill loops. After every seventh plot, the treatment (HC, HW09, HW07) was changed."

P6392 L22: From where this 34 vol% is coming? Is it based on your data? I haven'tseen any explanation for that value (no graph, no explanation).

Author response:

We agree with the reviewer that this topic was not addressed properly within the manuscript. After highlighting this issue by the reviewer, we reanalysed the data and accordingly revised our method of filtering out dates when water content was limiting soil CO2 efflux. Instead of using a threshold per se, we followed the method applied by Ruehr and Buchmann (2010) and removed dates where drought clearly interfered with the temperature response of soil CO2 efflux. To clarify this issue, we now highlighted the dry dates where low moisture limited soil CO2 efflux in Figure 3 as well.

P6393 L3: And now from where this 40 vol% soil moisture is coming. Earlier you were saying, that everything above 34 vol% should be OK, as below it soil respiration decreased sharply. Why not to use 35 vol% for example. I'm not trying to ironize here, just you are not explaining from where the parameters are coming.

Author response:

40 vol% was roughly the overall average in soil moisture of the control stands. This value is not relevant any more, as we removed the normalization approaches (Eq. 3 and Eq. 4) for soil CO2 efflux in the revised manuscript. Instead, we were just using F10 rates from the treatment specific models shown in Figure 2 for the further analysis of temperature independent Fsoil rates (see also response to comment 1 of reviewer #1). This makes the methodical part shorter and the interpretations easier, without losing validity of the results. The issue about the moisture threshold of 34 % is commented above.

P6393 L10-11: If you have used Fsoil through entire text for Soil CO2 efflux, why to jump now back. Use the same terminology through entire text.

Author response:

The suggested changes have been made.

P6394 L24: If you have pointed out the average soil moisture over the whole study period for Höllengebirge, why not to do this also for Rax.

Author response:

The suggested changes have been made. The average soil moisture values for the Rax site have been added now.

P6395 L2: No need to give abbreviations for soil CO2 efflux again. Use only the abbreviation as it is explained already earlier. The same problem continues through entire Results section

Author response:

The suggested changes have been made.

P6395 L7-10: It is clearly seen (from the Fig. 4) that we have the difference between the sites (Rax and Höllengebirge), so In my opinion you cant but these two sites together. If we would use only Rax, as this site covers a lot of the "years since disturbance" can we say clearly, that there is rebound and increase during years 6 to 12 after disturbance. And when calculating the curve (parabolic function) in Fig. 4. You cant use both sites as the sites are clearly different from each other.

Author response:

Our intention was not to compare the two sites with respect to the absolute soil CO2 efflux rates, but rather with respect to the general patterns in post-disturbance efflux dynamics. We therefore calculated the relative efflux rates, where fluxes from the

windthrow areas were related to the fluxes of the respective control stand. We hope it became more clear from the new site description that the two sites (Rax, Höllengebirge) are not clearly different (except annual air temperature) and that a comparison therefore is scientifically sound. We clarified this issue in the methods and materials of the revised manuscript as well. Furthermore, see related responses above.

P6395 L14: Again, I would like to see how this 34 vol% is found?

Author response:

The response to this issue is covered above.

P6396 L8-13: Are these average annual sums of soil CO2 efflux already reduced values (because of rock outcrops)?

Author response:

These values are not reduced by the percentage of rock outcrops. In the revised manuscript we changed these sums just to the reduce values and rephrased the paragraph.

P6396 L 16-23: Why there is no data presented about Rax area when talking about ground vegetation cover, although in Material and Methods section it is stated that the survey was done there also and some of the results are also visible in Fig. 6?

Author response:

The results of the Rax site were now added as well.

Table 1: Why to separate the p values into three different categories? What it gives? In Material and methods section it was stated that the $p < 0.05$ was used.

Author response:

Although a p-value of 0.05 was chosen as a minimum level of significance, the separation into different p-values should emphasize stages of significance within the data. We

further believe that providing this information improves the transparency of our presentation of the results. P value of 0.05 is our level of significance, but we cannot speculate on the level of significance accepted by the potential readers.

Table 2: Why we have only info about Höllengebirge site, but not for Rax site?

Author response:

The results of the Rax site were now added to the table as well.

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