**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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Author response:

We thank the reviewers for their 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 in italics, with the corresponding author response written beneath.

**Review #1:**

*The main finding of this study is that post-windthrow soil respiration was equal to, not lower than, that in undisturbed neighboring forested sites mainly because soil microclimate became more favorable to the decomposer community. The authors come to this conclusion based on: a) measured soil respiration, shown to be equal in disturbed and undisturbed stands; b) warmer soils in undisturbed sites; and c) empirical relationships that show the typical positive response of soil respiration to soil temperature. The authors also attempt to isolate the separate effects of changes in soil temperature, soil moisture, and other windthrow-related impacts using empirical modeling that transforms the data to control for one or more factors and examining remaining variation.*

*Overall this is a great study. The topic is important and appropriate for the journal. The writing is good (introduction is excellent), the graphics and statistics are solid, and the presentation is largely sound. However, I have some concerns about the analytical methods and interpretations used to attribute the steady soil respiration rate to changes in soil microclimate. The use of empirical modelling to tease apart the separate effects of temperature, moisture, and other factors is wise, however the implementation does not seem to be quite right in my opinion, for the following reasons, mainly revolving around the fact that both the microclimate conditions and the functional parameters differ between disturbed and control plots, but also due to other concerns.*

**General comments:**

*First, if you let F10 (the base rate of soil respiration at a temperature of 10 degrees C representing substrate supply) vary at a plot level, its effects on control versus disturbed site respiration is being misattributed to a microclimate effect in your interpretation. More broadly, between-site variation in F10 should reflect windthrow impacts independent of temperature, but it is unused in the presentation/analysis. Looking at Figure 2, I would guess that F10 was generally higher in controls,* *for 3 of 4 contrasts. I believe this would at least partially support your claim that microclimate is the principal cause of the maintenance of Fsoil at the pre-disturbance (or at least the control) rate.*

Author response to comment 1:

We are grateful to the reviewer for this hint. For sure $F_{10}$ (basal rates) are not an indicator for differences in microclimate. In the revised discussion section, we took the reviewers advice to heart, and now use $F_{10}$ rates explicitly to examine windthrow effects independent of temperature (P11, L16,17; P13, L7,8; P25 Table 3). In the revised manuscript, we furthermore skipped the normalization approach (Eq. 3 and Eq. 4) of the daily plot specific $F_{soil}$ rates. As recommended, we now use $F_{10}$ values from the models shown in Figure 2 ($Q_{10}$ functions) instead (P11, L16-17; P13, L7,8). This makes the interpretations easier, without losing validity of the results.

*Second, if you let Q10 vary at a plot level, again its effects on control versus disturbed site respiration is being misattributed to a microclimate effect in your interpretation. A shift in community Q10, and its resultant impacts on soil respiration rate, is not a microclimate effect but rather a change in the physiological response of the decomposer community, autotrophic community, and / or the type of substrate being decomposed. Your analysis and interpretation assumes that the effects of drift or shift in Q10 is either small, or is rolled into a "microclimate" effect. Your normalization of Fsoil for temperature and temperature plus moisture effects includes two moving parts: a) the microclimate conditions, and b) any drift in parameters (F10, Q10, and a).*

Author response to comment 2:

Our response to comment 2 is given together with the response to comment 3.

*Third, if the apparent Q10 is overestimated because of sensitivity to a change in the seasonality of autotrophic supply, this could falsely elevate the role that warmer soils plays in explaining the post-disturbance rate of soil respiration. I fear that there is not much you can do about this aside from restricting your estimation of Q10 to the shortest seasonal window that you can tolerate without loss of statistical power in determining the respiration – temperature relationship, but you might try to deal with this issue somehow.*

Author response to comment 2 and comment 3:

We also agree that a change in $Q_{10}$ at the disturbed areas is likely related to changes in the autotrophic contribution, and/or the decomposed substrate, rather than to a change in microclimatic conditions. As $Q_{10}$ values in the control stands were tendentially higher compared to the windthrow areas, we attributed this mainly to a seasonality in plant phenology and a consequent, proportionally higher autotrophic respiration component during summer. Such seasonal changes in autotrophic supply are consequently influencing the apparent $Q_{10}$ values. To minimize these effects we followed the reviewer's advice in comment 3 and subdivided the periods of measurements into two seasonal windows: 1) mid - season (1$^{st}$ June to 31$^{st}$ August) and 2) early/late – season (1$^{st}$ September to 31$^{st}$ May) (P8, L5-7). A further subdivision could not be done because of a drastic loss in model accuracy. Equation (1) was subsequently fitted to the seasonal data as well. Thereby, it was possible to analyse not only the temperature sensitivity of $F_{soil}$ but also the $F_{10}$ rates for each season separately (P8, L3-7; P25, Table 3). While $F_{10}$ rates did not change considerably, the $Q_{10}$ values increased drastically in the early/late – season (P10, L18-20; P25, Table 3). As mentioned in the response to comment 1, we skipped the normalization approach (Eq. 3, Eq. 4) from the revised manuscript. Thus, location specific $Q_{10}$ parameters for the normalization were not necessary anymore. Accordingly, we rephrased the respective paragraphs within the material and methods as well as in the discussion section (P8, L3-7; P12, L 27-31).

*Fourth, the model results shown in Figure 5 does not appear to have been fully successful because it does not recover the equal rates of Fsoil in the disturbed and control plots. The graphic shows that Fsoil for RW07 > Fsoil RC, when it did not. The graphic also shows that Fsoil HC > Fsoil HW09 and Fsoil 07, when it was not. Does this result from biases in the model fits? Can this be amended somehow?*

<u>Author response to comment 4:</u>
The empirical model approach definitely comes along with model uncertainties. However, the differences between the bars of Figure 5 are in good accordance with the results from Tukey's tests shown in Table 1 (compare: $F_{soil}$ RC < $F_{soil}$ RW07; $F_{soil}$ HC > $F_{soil}$ HW09 and HW07). Also the annual estimates of $CO_2$ emissions showed the same differences between the treatments (first manuscript: P 11, L 25-28). Nonetheless, we agree with the reviewer that the bar charts allow space for a vague interpretation of the results. Therefore, we decided to represent the effect of altered soil climate on $F_{soil}$ in a more elegant, but also a more precise, fashion. Instead of using the three different soil climatic averages together with the models, we now used the continuous soil temperature data. As the effect of altered soil moisture on $F_{soil}$ was negligible (~2%) at the Höllengebirge sites and not existing at the Rax site, we decided to remove this from the analysis. By using the model parameters (Eq. 1) of each windthrow area, we accordingly simulated the annual course of $F_{soil}$ rates (hourly interval) firstly, with the continuous soil temperature at the windthrow areas and then for comparison, with the continuous soil temperature of the respective control stand (P8, L19-23). The difference between the hourly simulations were summed up over the simulated year (2012) and represented the effect of windthrow related changes in soil temperature on $F_{soil}$ (P10, L 27-29; P30, Fig. 4).

*Fifth, the interpretation seems to suggest that autotrophic respiration was largely non-existent at the windthrow sites, however vegetation cover is equal if not higher in the disturbed plots. For example, P13, L30 seems to ignore autotrophic respiration as playing any role at all in the soil respiration at the disturbed plots by comparing the rate of respiration inferred without microclimate alteration to literature values for heterotrophic respiration. This argumentation should be clarified or refined. Furthermore, that section misquotes the range of respiration here, stating 60-70% when the graph (Fig 5) shows 64% to 78%, rising to outside of the literature range I believe.*

<u>Author response to comment 5:</u>

Of course, we cannot assume that autotrophic respiration was non-existent at the disturbed sites. Due to a quite sparse ground vegetation cover (between 6 and 50 %) in the initial phase post-windthrow we however assumed the autotrophic contribution to soil $CO_2$ efflux to be nevertheless comparably low. We agree with the reviewers statement that this issue was ignored in the respective paragraph of the discussion. This was clarified in the discussion of the revised manuscript as follows: " *Williams et al. (2014) reported an autotrophic contribution of ~ 30 % after four years post-clearcut. Their site was however nearly 100 % covered by ground vegetation already after four years, while it took much longer at the sites in our study region."* (P13, L15-17)

The 64 % to 78 % written in Figure 5 are related to the efflux contribution within a respective area (to add up to 100 % efflux). This is not the relation of the $CO_2$ efflux from windthrows to the $CO_2$ efflux of the respective control stands. However, this point is now redundant as Figure 5 (in the first version) was removed from the revised manuscript (see response to comment 4).

*Sixth, P12, L24: the fact that elevated temperature post windthrow boosted Fsoil at disturbed sites (Table 1) only shows that it contributed to sustained Fsoil rate, not that it was the principal factor. It would be entirely possible that other factors contributed even more, while temperature was still a significant contributor. At this stage in the paper the analysis does not yet point to temperature as having been the main factor, something that is explored further later in the paper.*

<u>Author response to comment 6:</u>

We agree with the reviewer that elevated temperature at the disturbed sites cannot be seen as the principal factor of increased $F_{soil}$ rates, as e.g. substrate quality/quantity or the microbial community as well as a delayed decomposition of residues have to at least some extent affected efflux rates. As already explained, we clarified this in the revised discussion section (see response to comment 1) P11, L16-28.

*Seventh, P14, L8: the high rates of CO2 efflux at the oldest windthrow area is assumed to be due to the dense grass vegetation and its effects on elevating autotrophic respiration. While plausible, it could still be that heterotrophic respiration is elevated by windthrow inputs with a lag as roots, litter, and woody debris fragment and decompose, serving as a supply for heterotrophs. It is also possible that exudate supply from the grass to the decomposer community feeds the heterotrophs as much as elevated autotrophic respiration. While autotrophic (root) respiration is likely a contributor, you do not have the data to show that it is the main factor and other processes may contribute as well and should not be dismissed.*

Author response to comment 7:

We agree with the reviewer's comment that a dense grass vegetation (and thus a higher autotrophic respiration) at the oldest windthrow area was probably not the only reason for the higher efflux rates. It is very likely that a delay in the decomposition of dead roots, litter and debris also contributed to an elevated efflux. We added these recommendations to the discussion section of the revised manuscript (P13, L18-19). However, the correlation analysis shown in Table 2 of the first manuscript (Table 4 in the revised version) encouraged us to assume the development of grasses and an assumed consequent increase in autotrophic respiration was nonetheless an important factor explaining these higher $CO_2$ efflux rates.

*Taken together, the main conclusion is not fully supported by the analysis presented. It should be possible to perform further testing, isolating parameter ($F_{10}$, $Q_{10}$) versus microclimate (soil temperature) changes, to dig deeper into the processes and more accurately attribute the observed patterns to drivers. Some of the interpretations should be modified accordingly.*

Author response:

In order to dig deeper into the driving processes of soil $CO_2$ efflux, we modified the analysis. As mentioned earlier, the data were split into two seasonal windows now (mid-season and early/late - season). $F_{10}$ and $Q_{10}$values were thus analysed for the seasons separately (see response to comments 1 – 3) (P8, L3-7, P25, Table 3). Furthermore, the modelling approach to disentangle the effect of altered soil temperature on $F_{soil}$ was modified in the revised manuscript (see response to comment 4). Respective interpretation and conclusions were clarified accordingly (see response to comments 1 to 6).

**Specific comments:**

*Eq 2: why did you adopt an exponential function of soil moisture in your model? Please add a citation to justify this model selection and explain the rationale or even defend it with a graphic and statistics.*

Author response:

We tried to fit other function types as well (e.g. linear function, quadratic function) but we got the best fit for the model results using an exponential function of soil moisture. This type of model function was also used in studies done by Soe and Buchmann (2005) and Knohl et al. (2008) and it was also cited in the book sections of Janssens et al. (2003) and Reichstein and Janssens (2009). As the inclusion of the soil moisture term only marginally improved the model results we anyway decided to remove Eq. 2 from the revised manuscript. We nevertheless still mention the incorporation of the soil moisture term to the model within the methods/materials (P7, L30-32) as well as within the results (P10, L7-10). The renouncement of the soil moisture term did not influence the validity of our results, but rather made the manuscript more understandable and easier to read.

*Table 2: why does Table 2 omit the Rax site? Please add it as well if you can.*

Author response:

The results of the Rax site were now added to the table as well (P26, now Table 4).

*P15, L22: is browsing pressure strong enough to prohibit forest regrowth or does it just delay it? This is an important point, and if forests regrow in the face of the browsing pressure, the risk of soil C stock reduction might be substantially reduced. Furthermore, the litter inputs in whatever community does succeed may still support and sustain soil C stocks, so it should not be assumed that the soil C pool is so vulnerable to release to that atmosphere, particularly if the main C source is the windthrow-killed trees, which should not be described as part of the soil C pre-disturbance.*

Author response:

Browsing pressure can definitely prohibit natural forest regrowth at our sites. Once you get a dense grass layer as at the old windthrow area, it is almost impossible for natural regeneration to establish in a later phase post-disturbance. We agree with the reviewer's statement that windthrow debris act as a C source post-disturbance. A large proportion of the killed trees was nevertheless removed subsequently to the disturbance at our sites. We also agree with the reviewer's statement that a post-disturbance vegetation sustain the soil C stocks and modified the discussion accordingly: "*In* *addition to the effects of ground vegetation cover, a delayed decomposition of woody debris might have contributed to higher $F_{soil}$ rates in a later phase post-disturbance as well."* P13, L18-19

*P3, L11: "forests" to forest's*

Author response:

The suggested change has been made (P2, L11).

*P13, L6: "died back" to dieback*

Author response:

The suggested change has been made (P12, L6).

*P12,20: see also and consider citing: Williams et al. 2013 Global Change Biology, "Post-clearcut dynamics of carbon, water and energy exchanges in a mid-latitude temperate, deciduous broadleaf forest environment", showing Rhetero:Rauto in a postclearcut environment. Could also be cited at P13, L8/9.*

Author response:

The suggested reference has been included in the manuscript (P12, L14; P13, L15; P14, L30). See also response to comment 5.

*P13, L1: see also and consider citing: Vanderhoof et al. 2013 Biogeochemistry, "Controls on the rate of CO2 emission from woody debris in clearcut and coniferous forest environments of central Massachusetts" showing how temperature and moisture affect decomposition in neighboring disturbed and undisturbed environments.*

Author response:

The suggested reference has been included in the manuscript (P12, L1).

**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.*

**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 (P5, L5-10). 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 (P1, L17; P3, L 29-30; P5, L5-10).

**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 (P1, L17).

*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 $CO_2$ 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 $CO_2$ 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.*" P5, L23-26
In order to account for the site differences between Rax and Höllengebirge, we did not compare absolute soil $CO_2$ efflux rates but rather looked at the post-disturbance trends in terms of relative contributions of soil $CO_2$ efflux from disturbed areas compared to the respective undisturbed control stands (a more detailed response to that issue is given later); P8, L24-28.

*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.*" P5, L5 -10. We also rephrased the sentence (BG discussion paper P6387, L13) to *"…varying temporal stages after disturbance"*, in order to clarify the context of time (P3, L30).

*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 (P4, L18-23). 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 (P23, 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 $CO_2$ efflux from windthrow areas in relation to undisturbed stands can be investigated (P8, L24-28). We therefore left Figure 4 in place (Please note: Figure 4 was changed to Figure 5 in the revised manuscript).

*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 $CO_2$ 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 $CO_2$ 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 (P8, L24-28). 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 $CO_2$ 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 $CO_2$ 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 (~ < 5 %) when compared to the whole disturbed stand areas (P5, L 3-4).

*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) P1, L17; P3, L 29-30.

*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 (P23, 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 (P6, L6-12).

*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 (P6, L2).

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

<u>Author response:</u>

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 $CO_2$ efflux rates (e.g. Pumpanen et al., 2004, Agric. For. Meteorol. 123). This information has been added to the manuscript as well (P6, L 25-30).

*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.*

<u>Author response:</u>

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 ~ 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 (P7, L 4-10).

*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 $CO_2$ 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 $CO_2$ efflux (P10, L2-7). To clarify this issue, we now highlighted the dry dates where low moisture limited soil $CO_2$ efflux in Figure 3 as well (P29).

*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 $CO_2$ efflux in the revised manuscript. Instead, we were just using $F_{10}$ rates from the treatment specific models shown in Figure 2 for the further analysis of temperature independent $F_{soil}$ rates (see also response to comment 1 of reviewer #1) (P25, Table3). 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 (P9, L16-17).

*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 (P9, L23).

*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 $CO_2$ 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 (P4, L18-23; P23, Table 1) 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 (P8, L24-28). 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 (P10, L21-26).

*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 (P11, L8-9; P26, Table 4).

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 (P26, now Table 4).