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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 #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 varia-

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tion. 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.

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:

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 F10 (basal rates) are not an indicator for differences in microclimate. In the revised discussion section, we took

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the reviewers advice to heart, and now use F10 rates explicitly to examine windthrow effects independent of temperature. In the revised manuscript, we furthermore skipped the normalization approach (Eq. 3 and Eq. 4) of the daily plot specific Fsoil rates. As recommended, we now use F10 values from the models shown in Figure 2 (Q10 functions) instead. 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 Q10 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 Q10 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 Q10 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 (1st June to 31st August) and 2) early/late – season (1st September to 31st May). 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 Fsoil but also the F10 rates for each season separately. While F10 rates did not change considerably, the Q10 values increased drastically in the early/late – season. As mentioned in the response to comment 1, we skipped the normalization approach (Eq. 3, Eq. 4) from the revised manuscript. Thus, location specific Q10 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.

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?

Author response to comment 4:

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: Fsoil RC < Fsoil RW07; Fsoil HC > Fsoil HW09 and HW07). Also the annual estimates of CO2 emissions showed the same differences between the treatments (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 Fsoil 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 Fsoil was negligible ($\sim$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 Fsoil 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. 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 Fsoil.

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.

Author response to comment 5:

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 (P13 and 14, L25 – L33 and L1 - 2). 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." 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 CO2 efflux from windthrows to the CO2 efflux of the respective control stands. However, this point is now redundant as Figure 5 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.

Author response to comment 6:

We agree with the reviewer that elevated temperature at the disturbed sites cannot be seen as the principal factor of increased Fsoil 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).

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

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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. However, the correlation analysis shown in Table 2 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). 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.

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 as well as within the results. 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.

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

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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 Fsoil rates in a later phase post-disturbance as well."

P3, L11: "forests" to forest's

Author response:

The suggested change has been made.

P13, L6: "died back" to dieback

Author response:

The suggested change has been made.

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 post-clearcut environment. Could also be cited at P13, L8/9.

Author response:

The suggested reference has been included in the manuscript. 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.

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