Biogeosciences Discuss., 11, C2047–C2051, 2014
www.biogeosciences-discuss.net/11/C2047/2014/

Biogeosciences
Discussions
Open Access

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

Anonymous Referee #1

Received and published: 2 June 2014

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.

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.

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).

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.

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?

Fifth, the interpretation seems to suggest that autotrophic respiration was largely nonexistent 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.

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.

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 ithe main factor and other processes may contribute as well and should not be dismissed.

Taken together, the main conclusion is not fully supported by the analysis presented. It should be possible to perform further testing, isolating parameter (F10, Q10) 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.

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.

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

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.

P3, L11: “forests” to forest’s

P13, L6: “died back” to dieback

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.

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.

Interactive comment on Biogeosciences Discuss., 11, 6383, 2014.