

Interactive comment on “Examining moisture and temperature sensitivity of soil organic matter decomposition in a temperate coniferous forest soil” by C. E. Gabriel and L. Kellman

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This manuscript describes a field and laboratory experiment where soil heterotrophic respiration has been measured in connection with monitoring and manipulations of soil temperature and moisture. The paper looks at the response of these variables in shallow and deep soils profiles, and speculates on the causes behind the observed differences. The authors argue that the use of intact soil cores allows for better estimations of actual field-condition responses to the measured variables. I identified a number of corrections to be made that together amount to a major revision. I think the manuscript is good for publishing once these issues have been resolved. I wish good

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luck to the authors on this.

We thank to the reviewer for these insightful comments. Much effort has been put into this manuscript review, and your attention to detail is appreciated. A response to specific concerns will be addressed in the technical comments section below.

Concerning the scientific significance of the paper: The method used, i.e. intact soil cores in lab conditions, is not new (Reichstein 2005 is cited). On the other hand, not many studies of this kind have been made and this type of data is valuable to understand the behaviour of different kinds of soils. That said, the paper lacks a clear scientific question or the study of underlying mechanisms explaining the observations. The value of the paper rests mainly on the relations obtained, which basically involve information on the effects of moisture (including the birch effect after wetting events), temperature and soil profile on respiration for the specific soil under study. Even without a specific question, the data is relevant and valuable for this field of science, which often relies on an accumulation of data for different ecosystem types. Importantly the study adds evidence for: respiration-moisture relations and breakpoints, temperature moisture effect interactions, birch effect influence on Q10 estimations.

Although we agree that there is much value in the documentation aspect of this study, we feel that the manuscript does propose a clear scientific question pertaining to the mechanisms underlying soil respiration. We feel we have clearly outlined the research objectives for this study and how this research addresses the problems inherent in temperature sensitivity estimations with standard laboratory incubations, with effort to study the neglected deep soil horizons. While the approach is not new, the inclusion of deep soil core work is novel, as it adds to the body of emerging knowledge about soil deeper in the profile.

General presentation: The authors discuss mostly relevant issues throughout the paper. However, much of the manuscript is over worded. The phrasing should be in general more succinct and to the point, precisely describing all relevant methods or results

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and avoiding non-relevant information. This is a problem throughout the manuscript. In addition, several sections need rewriting for consistency, or reinterpretation of the results. The discussion of related but not directly relevant issues should be avoided. This mostly involves the introduction and discussion sections. Some sections can be merged (see below). There are nearly 100 citations, showing a good deal of research, but also a disperse focus. The specific corrections below show the need for a more fluent and precise language. I realize the authors may not agree with all remarks, but the number of corrections prevented that I triple check all.

The referee's comments are appreciated. We have made an effort to ensure that the final version of the manuscript is tighter with more precise language. Details are provided below in response to specific points raised by the reviewer.

Data analysis and results:

1. Most of the methods used seem to be ok, with an important exception. Equation 4, given as a ratio raised to a term, is incorrect and should be:

$$Q_{10} = (R_2/R_1)^{(10-(T_2/T_1))} \quad (1)$$

(also, brackets should be used to show the exponent applies to the entire fraction). Before considering for publication, all calculations should be redone using the correct expression.

This was a typographical error in the manuscript. Calculations were performed using the correct function.

2. The discussion of the moisture sensitivity of the deep horizon rests on the assumption/observation that there are no significant differences between respiration at extreme and intermediate water contents. Although the plateau of optimal moisture seems larger than for surface soil, the extremes in figure 2b are evidently significantly lower than other values. The result and discussion should be changed to reflect this.

We have given this some consideration. We will rephrase the discussion to reflect this.

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3. The results and discussion on the temperature relations confuse basal respiration (absolute rates at a given temperature) and the temperature response or sensitivity. Basal rate changes in the study are mostly a result of the changing moisture conditions. The relation with temperature should be analyzed by comparing the sensitivity (Q10) and not the absolute values.

We do agree that the temperature sensitivity is more important than basal respiration. We will revise relevant sections of the discussion to ensure that this idea is clearly communicated in our text.

4. For table 2 and related results/discussion: as shown, respiration during rewetting conditions (RW) is not stable and will either be going up or down during a diurnal period, independently of T changes. If this change is correlated with the temperature change used to determine temperature sensitivity, then the Q10 determined will not be valid (i.e. it will not be an accurate measure of temperature sensitivity). This problem should be addressed in the study and these Q10s not over-interpreted if this confounding effect exists.

The discussion will be revised to reflect more clearly that this is the concern with interpretation of rates in situ, where moisture conditions change rapidly and frequently. Temperature sensitivity for these intact cores is unchanged despite different basal rates that are specifically influenced by changing moisture conditions and the birch effect (short-term rewetting stimulation).

Specific comments:

P 1371 L 19: change "as it" to ", which" L 21: change "may be" to "is" L 22-25: this sentence repeats itself and is not clear

These changes will be made.

P 1372 L 4: "more complex than that of" L 8-10: "By altering solute and oxygen diffusion, substrate supply and decomposition rates are directly affected". The second

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part of this sentence does not relate well. L 12: "the physical" L 13-14: This sentence "Nutrient cycling: : ." is not detailed enough to be understandable. Also should remove "in dynamics" and write "Nutrient cycling dynamics: : :". L 23: you should probably cite Werner Borke and Egbert Matzner, "Reappraisal of drying and wetting effects on C and N mineralization and fluxes in soils," Global Change Biology 15, no. 4 (2009):808-824.

This reference will be inserted and noted changes made.

L 28-29: "they may act as confounding factors for soil respiration" P 1373 L 1: remove "on respiration" L 8-9: "processes such as autotrophic respiration" (this term was already defined) L 15: "to minimize the potential influence of confounding variables" I believe is more correct.

These changes will be made.

P 1374 L 8: change "measured soil temperature are" for "incubation studies is" or else clarify this paragraph. L 12-16: much repeated from before. Reduce. L 16-20: "In this study, we incubated intact soil cores as in intermediate: : ." also removed "employed", then ": : : and to allow measurements of shallow and deep soil responses to changes in environmental conditions." L 21: change "confounding factors" to "effects" L 22: change to "through disturbance. Environmental factors were then manipulated independently in a climate-controlled facility."

These changes will be made.

P 1375 Paragraph 1: not clear what the relation is between the chronosequence and your experimental site and what the ideal opportunities are. Simplify.

The text will be revised to address this.

P 1376 Paragraph 1: Some repetition from introduction (3rd sentence). L 8: change "can then be" to "were". Title of 2.2 I believe should be "In situ measurements" or "flux measurements" L 13: "Field measurements: : :"

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These changes will be made.

P 1377 L19-20: not clear what you did here L 27: Re-phrase the whole sentence starting on this line which is not clear

The sentence will be rephrased.

P 1378 L 7: change "treatment" for "chamber" L 13: delete " setting" L 15-16: delete "imposed technical constraints on temperature control that" L 23: delete "microcosm"

These changes will be made.

P 1379 L 3: change "CO₂ fluxes were observed" to "measurements were made" L 6: "adjusted to either of: : ." L 10: Remove "Soils in wetting treatments" and merge sentences to "Soils in groups (ii): : : were wetted incrementally : : ." L 13-18: "The slow wetting was : : : soil matrix, to allow observations of soil responses to gradual shifts in moisture, and to assess the response of CO₂ flux after each incremental wetting event." The order you used is irrelevant. Or did I miss something? L 21: change "microcosms were" to "was".

We agree this may be unclear. These changes will be made.

P 1380 L 8: allows L 8-10: what is "aggregate dynamics"? what does the undisturbed condition have to do with pore-space estimation from saturation?

The text related to aggregate dynamics will be revised to better explain this. The undisturbed condition maintains natural pore structure so as to allow an estimation with water saturation (known volumetric water content). Disturbed soils experience compaction and therefore an underestimation of in situ pore space volume.

P 1382 L 3: remove "experimental", write "temperature experiment" Last paragraph: g C is related to what unit? Square meters? L 21 -32: what is greater? A noun is missing here.

These changes will be made.

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P 1383 L 9: "as measured by flux" is redundant L 11-12: as stated before, I think this is incorrect.

We will modify the discussion to reflect this change.

Second paragraph: I would argue that the difference between a linear and exponential fit are too small to justify exponential. This can be discussed. L 25-27: these differences are not an effect of temperature but of moisture.

We do agree that the difference is small. Since many authors report the relationship between flux and moisture to be exponential, we identified this model as an important one to discuss. However, we could include both in our discussion.

P 1384 First paragraph: again there is confusion between the effect of temperature (or temperature sensitivity) and the basal rates. Also, as stated above, can the Q10 (parameter b in table 2) from RW be considered a valid measure?

We have revised the discussion of these data, so as to explicitly communicate the confounding effect of determining the temperature sensitivity as moisture is changing.

P 1385 First sentence is redundant L 11: change to "between limitation by solute diffusion : : : on the lower and and rates of gas : : :." L 24: has VWC been defined already?

Yes, it has been defined in the methods.

P 1386 L 9-13: rephrase for clarity L 26-27: it's either WHC or VWC. They are not the same.

This will be fixed.

P 1387 L1 replace "cease" with "be highly limited" Section 4.2: reconsider this discussion. Significantly low values at extremes are obvious. The data is otherwise quite variable and difficult to interpret so the discussion can be made shorter. L 25: replace "would not have" with something more precise. Did not have? Why not? L 28:

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particularly sensitive to moisture conditions contradicts your previous discussion

We do agree that the detail in this paragraph should be clearer (instead of speculative in nature about differential controls on respiration in shallow vs. deep). This section (4.2) will be shortened.

P 1388 Section 4.3 paragraph 1: here again basal respiration magnitudes are discussed as a temperature response, which is wrong. This section and the next (Temperature sensitivity) should be shortened and merged with a focus on the T sensitivity (expressed here as the Q10).

This will be reframed in the discussion to be more precise.

P 1390 L 13-14: Sentence not clear L 15 "would have included" to "include" L 20-21: delete these lines.

These changes will be made.

P 1391 First paragraph coming from previous page is confusing. Should be rewritten. L 9: "noted in our study" L 10-25: I do not agree with the interpretation of the results. The authors observed the same T sensitivity in deep soils compared to surface soils, so the conclusion that the deep soil has low T sensitivity is based on what comparison? To me it seems like there is no difference.

We will attempt to frame our conclusions more directly. The Q10 values are low compared to the results of other studies, but not within this work.

P 1392 L 15-17: this repeats L 17-27: speculations and interpretations here have no supporting data. Should probably be more concise / to the point.

These changes will be made.

P 1393 First paragraph: it is not clear when the remarks are more general or when they refer to this particular site and study. L 19-21: this sentence is awkward. Re-phrase or change. L 24: remove "interesting and valuable"! very funny.

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Point taken. These changes will be made.

Interactive comment on Biogeosciences Discuss., 8, 1369, 2011.

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