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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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Comments on the manuscript “Examining moisture and temperature sensitivity of soil organic matter decomposition in a temperate coniferous forest soil” by Gabriel and Kellman.

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 es-

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timations 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 luck to the authors on this.

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.

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 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.

Data analysis and results: 1. Most of the methods used seem to be ok, with an impor-

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tant exception. Equation 4, given as a ratio raised to a term, is incorrect and should be: $Q10 = (R2 / R1)^{[10-(T2/T1)]}$ (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. 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. 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. 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.

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

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 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 Borken and Egbert Matzner, “Reappraisal of drying and wetting effects on C

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and N mineralization and fluxes in soils," Global Change Biology 15, no. 4 (2009): 808-824. 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

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."

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

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..."

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

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"

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."

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The order you used is irrelevant. Or did I miss something? L 21: change “microcosms were” to “was”

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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?

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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.

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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 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.

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?

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?

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

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: particularly sensitive to moisture conditions contradicts your previous discussion

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 (Tem-

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perature sensitivity) should be shortened and merged with a focus on the T sensitivity (expressed here as the Q10).

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

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.

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.

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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