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Biogeosciences Discussions

Interactive comment on "Amplification and dampening of soil respiration by changes in temperature variability" *by* C. A. Sierra et al.

Anonymous Referee #2

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Review of Seirra et al., Biogeosciences

This study investigates the effects of changes in the mean and variance of soil temperature on the potential amount of carbon respired from soils. The rationale behind this study is that studies that address the mechanisms for soil carbon release via decomposition have focused considerably more on the effects of changes in the mean of soil temperature than the variance of soil temperature. This to a great extent is true, but in my opinion this paper contributes little insight to overcome this problem. The analysis of this paper relies on using different time series of soil temperature into well-known respiration models and on demonstrating analytically and numerically different scenarios of the predicted soil respiration. I agree with the need for this work but at the same time I am critical of the current content of the paper as it does not provide any new

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Interactive Discussion

Discussion Paper

insight or any applicable finding from what it has been already known (for example, the conclusions do not hint at any practical solutions for current models). Exploring Jensen's inequality in the relationship between soil temperature and respired CO2 is not novel. In fact, this inequality is applicable not only to soil temperature, but also to soil moisture and to any non-linear relationship; thus rather than an expansion from Jensen's inequality, this paper is an applied example of the well-known inequality. I suggest the authors specifically state what it is that needs to be changed/implemented in current temperature-based models, and how this implementation is 1) realistic given our current sampling design and sampling limitations; and 2) quantitatively necessary given current predictions of soil respired CO2, which would be much different should these findings be applied.

The paper provides a straightforward analysis, it is easy to follow, and the results are to be expected. The datasets were obtained from websites of three different sites (Alaska, Oregon, Costa Rica) but these datasets themselves are limited. A major criticism is that the authors do not recognize that soil temperature varies with depth (the temperature used comes from three different depths) and the reader is left to assume that either soil temperature is considered constant with depth or that soil respiration occurs at only one depth. In fact, analogous findings would have been reached by looking at soil temperature from one site but three different depths (different probability distributions) or at two contiguous sites with different canopy cover (different probability distributions). This is never discussed. If we followed the logic that I believe is proposed in this study, we would have to apply different probability distributions for each depth in the soil, as these findings are not only relevant across latitudes but across depths at a single site as long as there is decomposition in the soil (5 cm, 10 cm, 15 cm, 20 cm and so on). Really quickly it becomes obvious that the implications of this paper would suggest we need to make our models more complicated to account for different latitudes, vegetation cover, depth, moisture regimes, etc. Is this the direction we need to go? The paper does not elaborate on this and I think it should.

BGD

7, C5223–C5226, 2011

Interactive Comment

To overcome these deficiencies, aside from the comments above I only have one 'major comment.' I suggest the authors make a compelling case as to why these findings matter and how these findings must be implemented into current models to improve quantifications of soil-respired CO2 under changing temperatures. To do this, I believe it is necessary to present quantitative examples (e.g., a case study using respiration data from these sites or from different depths) using current and modified models and more extensive datasets to show numbers that support statements like the last statement of the abstract. My fundamental criticism of this study is that pointing out a problem without providing a practical solution does not advance our field and it only limits the potential impact that this paper may have. I believe the suggested analysis would strengthen the significance of this paper.

Other minor comments:

Introduction: The rationale of the introduction is based on the prediction that mean soil temperature will increase under future climate scenarios and at the same time, the probability distributions of soil temperature will change. The climate connection seems to be overstated but in reality it appears to me that it is unjustified. My point is that the inter-annual variability alone will impose differences in distributions that may or may not be due to climate change, thus the climate change argument could be downplayed and the emphasis should be made on accurate predictions of soil respiration across a wide range of temperatures – anyone reading this would quickly make to connection to climate change.

Page 8985, Line 15. Please delete: "in addition to its familiarity to ecologists." This implies that other scientists do not know this function.

Page 8986: Using RWC=75% seems rather high. At this stage oxygen diffusion may already be limited.

The 'geometric argument' and the 'probabilistic argument' of the results would be better placed in a section called 'Analytical Analysis' that is outside of the Results section.

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7, C5223–C5226, 2011

Interactive Comment

Interactive comment on Biogeosciences Discuss., 7, 8979, 2010.

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7, C5223–C5226, 2011

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