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***Interactive comment on* “Effects of precipitation on soil respiration and its temperature/moisture sensitivity in three subtropical forests in Southern China” by H. Jiang et al.**

Anonymous Referee #2

Received and published: 22 December 2012

General comments

The authors present soil respiration results from a precipitation manipulation experiment in subtropical forests. This is a valuable data set that can provide the scientific community with new insight on the relative importance of moisture and temperature changes for a key C cycle flux with implications for climate change in subtropical forests. There is a clear need for the type of study presented here, and the scientific design and methods are sound. However, significant changes should be made in the justification, presentation and interpretation of the study results before publication.

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Specific comments

The question of changing temperature sensitivity of soil respiration with changes in soil moisture needs more study and I agree with the authors that data are lacking for sub-tropical areas. I also think that the precipitation manipulation approach is necessary to tease apart the confounding influences of temperature and moisture on seasonality of respiration rates. However, the potential impacts of changing precipitation on moisture sensitivity need to be re-considered by the authors. For example, what is the ecological justification for this question? This needs to be adequately examined in the introduction. Upon examining this, the authors might find that their methodological approach to quantifying moisture sensitivity is inappropriate for this type of system. Ecosystems that experience a wide range of moisture levels often display a non-linear response of respiration to moisture changes that is completely consistent with their experimental findings of changing moisture sensitivity to respiration. The way that the authors have presented the moisture sensitivity findings does not constitute a novel contribution to the field. We have known for decades that soil respiration is most sensitive to moisture as moisture increases from dry to moderately wet conditions. At some higher level of moisture content, the soil pores begin to fill and oxygen (the electron acceptor for CO₂ production via respiration) concentrations drop, thus respiration will reach an asymptote or even decline with increasing soil moisture. The authors have simply confirmed this pattern by experimentally manipulating moisture levels. This manuscript could increase its novelty by taking this concept somewhere new - are there important feedbacks with plant productivity (or root biomass) that change the shape of this function?

Related to this, I do not understand the need for using three different model structures to examine temperature and moisture sensitivity of respiration. I suggest using only the model that includes both temperature and moisture parameters in the same model, unless the authors can justify using all three and tie this into their hypotheses. However,

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I think the moisture term is inappropriate for this type of system and the model structure should be reconsidered.

The hypotheses lack adequate justification in the introduction. The introduction should lead into the specific questions addressed by the hypotheses. A major problem occurs in the introduction, found at the end of the third paragraph. The authors state that “the changing precipitation pattern will have a significant impact on the soil carbon stock of subtropical forests in Southern China”. While few would disagree with this statement, it is problematic because the authors do not examine soil carbon stocks in this paper. Soil respiration is not synonymous with “soil carbon stock” and one cannot be inferred by the other. This relates to a concern about the general mood of the introduction. The authors do not make it clear that soil respiration is controlled by both autotrophic processes and heterotrophic processes. Changes in temperature sensitivity of soil respiration may not translate into changes in soil C stocks and therefore, “soil C feedbacks to climate change” cannot serve as a justification for this study. The introduction needs more focus, needs more emphasis on the role of plants in soil respiration, and needs to better develop the need for investigating impacts of changing precipitation on moisture sensitivity of soil respiration. Also, why did the authors decide to study three different forests? It seems like the three sites generate a moisture gradient; if that is the justification, it needs to be specified.

The microbial biomass and fine root data are not integrated into the paper and should be either dropped completely or written into every section of the paper. Currently, the abstract and introduction make no mention of either data types and this is not acceptable. There should be explicit hypotheses involving microbial biomass and fine roots.

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Technical corrections

15669, line 12: Please do not ignore the role of carbon supply. There are many citations supporting the concept that C supply matters for soil respiration, such as

Campbell, J.L., Sun, O.J. and Law, B.E., 2004. Supply-side controls on soil respiration among Oregon forests. *Glob. Change Biol.*, 10: 1857-1869.

Högberg, P. et al., 2001. Large-scale forest girdling shows that photosynthesis drives soil respiration. *Nature*, 411: 749-752.

Curiel Yuste, J. et al., 2007. Microbial soil respiration and its dependency on carbon inputs, soil temperature and moisture. *Global Change Biology*, 13: 2018-2035.

15669, line 17: this isn't necessarily true for moisture. You cited Falloon et al. 2011 who used a variety of soil moisture-respiration functions, some of which account for changing moisture sensitivity with different moisture levels. This relates back to one of my "Specific Comments" above.

15669, line 22: A period is needed after "warming" and before "Several"

15669, line 24: "Curriel" should be spelled "Curiel"

15670, line 23: Re-write this sentence to delete the implication that you studied soil C stocks

15673, line 5: what is the "distance" that you refer to? Is that the distance between the soil surface and the pipes?

15673, line 20: soil respiration was measured 3 times for each collar. Were they then averaged together? That needs to be stated.

15674, lines 14 through 27: Microbial biomass and fine root biomass are not mentioned here. How were they analyzed?

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15676, line 25: Is this paragraph discussing only data from the ambient precipitation plots? If so, that needs to be stated.

15679, line 12: “Curriel” should be “Curiel”

15679, line 13: do you mean to say “predication” here or “prediction”?

15680, line 14: I disagree - I think it also depends on carbon supply. Do you have a reference to support this statement?

15680, line 17: insert “was” after “sensitivity”

15681, line 2: replace “abundant” with “an abundance”

15682, line 12: the statement “moisture sensitivities are often assumed to be constant” should be rephrased because it is not true with many models. I do not think the discovery that precipitation altered moisture sensitivity is a novel one.

15683, line 29: “Curriel” should be replaced with “Curiel”

15684, line 1: “Curriel” should be replaced with “Curiel”

15691: Include the model in the caption so we know what the parameters mean.

15694: Orient the panels for different forests side-by-side instead of up-and-down; it is easier to compare the values among forest types that way. Also, it would improve the figure’s clarity to replace the error bars with error bands.

15697: I am not sure the regression lines are adding anything here. If anything, it draws attention to the nonlinear appearance of the relationship between moisture sensitivity and soil moisture. Consider removing.

Interactive comment on Biogeosciences Discuss., 9, 15667, 2012.

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