Biogeosciences Discuss.,
doi:10.5194/bg-2016-291-RC2, 2016

Biogeosciences
Discussions
Open Access

Interactive comment on "Soil CO_2 efflux from two mountain forests in the Eastern Himalayas Bhutan: components and controls" *by* Norbu Wangdi et al.

Anonymous Referee #2

Received and published: 10 October 2016

The paper "Soil CO2 efflux from two mountain forests in the Eastern Himalayas Bhutan: components and controls" by Wangdi et al. provides further information on a data poor environment, relevant to the Earths carbon budget. In this way the paper is a useful contribution to the cannon. Further lab-based incubations also appear useful for constraining modeled behaviors in the field, and the provided comparisons to in situ outcomes may be informative.

I have a number of concerns about the choice to include some of the provided data, as well as the exact equations and parameters that the model utilized to determine partitioning between respiration components. Overall, in agreement with the first reviewer, I believe that this study has some sound and useful information and analysis that should

be published, but I would expect major revisions would be necessary to clear out the unnecessary components of the article and clarify others.

My broad concerns will be listed first with specific items listed afterwards:

1) The "coniferous forest" as described has a substantial component of broadleaved trees (~29% Quercus sp.), and is later described as, perhaps more appropriately, a "cool temperate mixed coniferous forest" (line 72/73). Perhaps describing as a "mixed forest" throughout the paper would be more helpful. This is likely to have some impact on respiratory fluxes (through litter quality, leaf economics, etc) and this, along with the potential impacts from soil type and understory plant types, density and behavior is not addressed in the text sufficiently in my mind.

2) I question the value of the 2014 field-based Rs results, considering that they are acknowledged by the authors to be influenced by pressure effects from chamber placement.

3) I am uncomfortable with different mathematical functions being used to determine the same biological functionality (in particular the linear versus Gaussian response of soil water content to respiration rates). I would prefer that whichever function is used that there is some biological rationale that can be used to defend this choice.

4) I agree with the first reviewer that it would be better to have the model by which Rh and Ra components were calculated either explained through the primary equations in the text, or by incorporating the model as a supplementary material.

5) The use of the term Q10 to describe the entire soil response to, effectively, seasonal changes is inappropriate to my mind. By definition Q10 refers to the change in reaction rate of an enzyme or system to 10 degree changes in temperature, and on this basis the lab-based incubation Q10s are appropriate and should be retained and used in the models, but calling the whole system response a Q10 when the authors acknowledge (lines 301-303) that it incorporates water content, leaf litter availability and other co-

variable parameters makes this use of the Q10 term meaningless.

———————————————— 1) Line 25/26: see broader point 5 above. This is not in any way a Q10 with the number of conflating variables. Please use different terminology.

2) Lines 64-67: These hypotheses are not all that useful and the final hypothesis is not addressed within the paper, leading to a question of whether these are needed in the paper at all.

3) Line 78: Acer campbelli is listed as a dominant species in the cool, temperate mixed coniferous forest but is not listed in Table 1.

4) Lines 88-92: Climate can vary dramatically in mountainous regions over spatial scales of 1km. Is there evidence that these weather stations were recording appropriate data for these sites?

5) Line 123: By the nature of its close follow on after trenching this seems to refer to volumetric soil water measurements in the trenched plots but instead refers to the broader study plots (as shown in Figure 1). This could be more clear.

6) Lines 145-146: I would be interested in hearing more about the ventilation system used for the incubations. I am uncertain how much water might be lost by the soils during this process (e.g.- the ventilation process during the two-week waiting periods between soil moisture sampling) and how this water loss was addressed during periods between measurements.

7) Lines 143-148: I wonder about the effect of sieving on Rh considering the disruption placed on the soil/fungal community. It seems likely that this has significantly affected this component within this aspect of the study. (Datta et al Int. Agrophys., 2014, 28, 119-124)

8) Agreed with reviewer #1 point 7

9) Lines 188-194: The assumption that the temperature in the soil at 5cm depth is sufficiently predictive of Rs may work within this model but it assumes that the system is sufficiently co-variant that this one data point is essentially all that is needed. This seems to assume that the basal respiration from lower soil depths is effectively constant. Can the authors provide any evidence that this is true?

10) Line 200: I agree that a Gaussian distribution is probably the most appropriate here (and for appropriate biologically relevant reasons) but the linear fits later in figure 2 have no real biological rationale.

11) Lines 205-212: The trenching experiment not only affects water retention in the soil but also provides further litter availability and there are likely non-linear effects that are not well addressed in this section. I am also unconvinced that the correction for soil moisture is precise and accurate based upon the data reported. Perhaps it would be more useful to report a range of possible outcomes instead of the firm values reported here.

12) Line 220: It is unclear if the lack of specific moisture response function is due to a lack of (or no) collected data or a poor linear or Gaussian fit was obtained from the collected data.

13) Line 246-247: The method for assessing fine root biomass is not reported. Either the method should be discussed or a reference to the data would be helpful.

14) Lines 252-254: Given the potentially compromised nature of the Rs data from 2014 I would prefer that it not be reported at all, especially given the successful campaign run through 2015. The nature of pressure pumping and its effects on fluxes is sufficiently well established that this doesn't add much value to the paper.

15) Line 259: This is somewhat self-fulfilling. You measured once every three weeks and find that 3 week sampling density is sufficient. In order to truly test this you would need a higher density sampling rate that you are then able to sub-sample at the 3 week

frequency. I would suggest this comment (and others similar) be removed from the text.

16) Line 277-278: Again, given the nature of the trenched plots in 2015 (errors in strategy that are explainable and understandable) I am uncertain why this is discussed in the methods section and here. If I understand correctly not including this would save space and would not affect your analysis.

17) Lines 280-287: The model should be made more clear, in agreement with point 9 from reviewer #1.

18) Lines 301-319: I find this justification of the "field Q10" values to be unconvincing and suggest that this section be reworked or removed from the text. There are too many other variables that are not addressed beyond the already tenuous soil moisture correction for this to be adequately compared to a true Q10.

I agree that Figure 3 seems to serve little purpose and any lost detail can be described quickly and easily in the text.

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