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

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Anonymous Referee #2

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,

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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.

We highly appreciate all your constructive comments. All suggestions were taken into account and the revised manuscript is shorter and streamlined. 2014 data, which held some methodological bias were removed as suggested by reviewer 2 and 3, Rh-model R-code and all data are added as supplement. While revising the manuscript, we found a minor conversion error for Rh values per kgC-1, which we corrected for in the revised manuscript. Corrections did not affect the overall outcome of the study. Corrections resulted in slightly higher modeled Rh and minimal, insignificant deviations of Q10 and R10 values when compared to the values in the initial manuscript (without any effect on the study results!).

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 (âĹij29% 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.

We consistently use the term “mixed forest” in the revised manuscript. We discuss possible effects of litter dynamics on Rs and Rh (L250-252, L270-275)

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.

We have removed all 2014 data, as suggested. We did not lose important information by doing so, but it made the whole paper much easier to read.

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.

That was a mistake. We use consistent functions in the revised manuscript. We also changed to a more simple mathematical function for the relationship between Rh and soil moisture.

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.

We explained the functions in the text more thoroughly and added the R-code of the Rh model as well as all data as supplement.

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.

We agree that the relationship between seasonal Rs and seasonal soil temperature does not resemble the actual temperature sensitivity of Rs. We also agree that the use ofQ10 actually is not desirable in this regard (although quite commonly done). We used another formulation for the exponential relationship in Eq. (1), avoiding the term Q10 already in the Rs function. We completely reworded the results and discussion

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section and removed the Q10 values for seasonal Rs from the graphs. We did not lose any relevant information by applying these changes, but shortened the ms and made it clearer and easier to read.

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.

No Q10 used for Rs any more – see above.

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.

We removed the hypotheses and defined broader research questions instead.

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.

We removed Acer campelli from the text as it was not dominant.

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?

Especially rainfall can vary within small spatial scales. However, the climate station was at exactly the same altitude in the same valley/same slope/ same aspect, so that there is no indication of differences in climate at such fine scales. Soil moisture data at the site correspondingly fits very well with rainfall events measured at the weather station.

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.

We clarified that. Moisture was measured at all plots, broader study plots, trenched plots, and control plots.

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.

We added (L107-109): "In the meanwhile, disconnected containers were ventilated by means of an air pump in order to prevent internal CO2 enrichment. Wet tissues were put into containers in order to prevent samples from drying out during incubations; moisture loss was thereby negligible (< 2 vol. %)".

We also clarified that soil cores were only placed in the incubation chambers during actual incubation runs and that cores were stored in a storage room (+4°C) in-between the incubation runs.

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)

We are aware that sieving disrupts fungal hyphea and has further unwanted effects, such as a disruption of soil aggregates, which could liberate bound SOM. We nevertheless decided sieving the soil. Incubating undisturbed cores, makes it difficult to be sure that root respiration is really excluded, as intact fine roots in the cores can maintain respiration for relatively long times. Correspondingly, root respiration could add to the core CO2 efflux even after long equilibration times and thereby affect the temperature sensitivity. As we aimed to model Rh, we decided not to use intact soil cores. We added some lines to the discussion:

L 280-282: As a last point, soil sieving could have positively affected Rh rates during incubation by releasing physically protected SOM and/or providing additional C sources via disrupted fungal hyphe and fine root fragments (Datta et al., 2014) .

8) Agreed with reviewer #1 point 7

Changed accordingly throughout the revised manuscript.

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?

Most of Rs will be produced in the topsoil with highest SOM, microbial biomass, and fine root contents. Therefore topsoil temperature usually is a quite good predictor of total Rs. We actually do not assume that the basal respiration from deeper layers is constant, but that respiration rates from deeper layers co-varies with that from topsoil. We are aware that this very likely is not really the case as deeper soil temperature reacts somewhat delayed to topsoil temperature variations and as Rs, produced in deep soil, needs some time to diffuse to the surface. The very tight relationship between topsoil temperature and Rs however indicates that deeper soil Rs production is quantitatively not so relevant, or that deeper soil Rs still co-varies with topsoil Rs, at a scale that is mostly covered within the model. We discuss the shortcomings of the model approach in the revised manuscript (L259-284).

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.

We changed to a more simple polynomial function which is consistently used now.

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.

We more intensively discuss the problems associated with trenching (L285-303). Soil moisture correction should have been fine as we used the moisture measurements directly obtained from trenching and control plots. This was somewhat unclear in the original ms and has been clarified.

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.

This was matter of a misleading formulation. We simply did not obtain litter CO2 efflux data under different moisture levels. Accordingly, no response function was available for litter, and the function for mineral soil was used instead. We clarified that in the text.

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.

We added the method (L66-70): Fine root ($\leq$ 2 mm) biomass was estimated by soil-core method (Makkonen and Helmisaari, 1999) once in spring 2014 at both sites. We used a cylindrical soil corer (7.5 cm diameter) for sampling. The extracted core samples were divided into three depth sections of 0-10 cm, 10-20 cm and 20-30 cm. After washing and sorting (live roots and necromass), roots were dried at 70 °C to constant mass before weighing dry biomass. Contribution of fine root C was estimated as 50 % of the plant tissue.

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

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Discussion paper

well established that this doesn't add much value to the paper.

We removed all 2014 data as suggested.

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.

This is true. We removed that.

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.

We removed the additional 2015 trench plot data completely.

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

We added the complete code and data as supplement.

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.

We re-worked this chapter. Actually, we deleted most of it as we decided not to use Q10 for Rs (as suggested). The whole chapter now is much clearer and only the important information is provided/discussed.

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

We removed the graph.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-291/bg-2016-291-AC7-supplement.pdf

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