Biogeosciences Discuss.,
doi:10.5194/bg-2016-291-AC8, 2016

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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Received and published: 25 November 2016

Anonymous Referee #3 This manuscript entitled "Soil CO2 efflux from two mountain forests in the Eastern Himalayas Bhutan: components and controls" by Wangdi et al. provides interesting, relevant and valuable information on a poorly studied region. Manuscript is mostly well written and easy to read. The use and comparison of different techniques of measurements and different models is interesting. Nevertheless, aspects of the methods and then of the results remained unclear because it was not easy to distinguish and understand when and also why measured or modeled results were used to suit the purpose.

Major revisions would be necessary to clarify the manuscript and to develop more

explicitly the objectives of the comparison between different measurements/models.

We highly appreciate the constructive comments which largely aligned with suggestions from reviewers 1 and 2. We better distinguish between modeled Rh and trenching results in the revised manuscript and we better explained why these methods were used. All other suggestions have been followed as well. 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!).

General comments:

I am not convinced that 2014 field Rs data should be presented in the ms as they are not relevant because influenced by pressure effects. In the same way given the trenched plots in 2015 didn't produced meaningful values, what does these data bring to the analysis? If retained, the trenched plot results could be better discussed. Important care must be given to distinguish between measured and modeled results. Authors should explain why and when each was performed and also why and when they are referring to each.

As suggested, we removed the 2014 Rs data as well as the data from 2015 trenching plots. The text is now easier to read and we did not lose relevant information.

Specific comments:

1) l.23-24 : unclear. I can't see why the variability of Ra indicates a methodological issue with the trenching

We removed this from the abstract. We further refined the discussion and point at shortcomings of both methods (model, trenching) in the discussion section (L259-303).

2) l.272 : prefer effect of sites rather than of forest type

Changed to "sites" in the whole manuscript

3) l.190 : discuss how constraining the model with the temperature in the soil at 5cm depth is sufficient and relevant. What about the deepest contributions to Rs?

We actually used soil temperatures from different soil layers (mineral soil 5 cm, and mineral soil 20 cm depth) for modeling (L180-183) of CO_2 efflux from the corresponding layers. CO_2 efflux from < 30 cm depth was neglected in our model. We discussed this in terms of the Rh model outcome. We extended the discussion accordingly (L259-284).

4)l.190 : The same parameters (of Eq1) are used to model Rs over the year without any discussion whether or not the Q10 could vary with the temperature range over the year.

We alternatively fitted a Gaussian function (where Q10 changes with temperature). The fit of the simple exponential function was slightly better. We therefore decided to stick to this function.

5)l.205-212 : agreed with reviewer #2 point 11. Indicate the uncertainties rather than that corrected value.

As both methods have some different sources of uncertainty, they are quite difficult to quantify. We therefore stuck to the graph, but better discuss uncertainties in the text.

6) l.218: what is Fig S1 ?

Supporting Figure 1 is a supplement. We deleted this figure as this is a common procedure in model anyway.

7) l.246 : report and discuss the method used to estimate fine root biomass

We added the method (L66-70).

8) l.259 : How can you be convinced that it 'indicates that a three-week interval is suf-

ficient' although you didn't measured with a higher frequency ? Restrain the purpose.

That's true. We deleted this sentence.

9) l.278 : useful ?

Deleted.

10) l.308-319: I have issues with the analysis presented here because I am concerned about the definition for the terms intrinsic and apparent sensitivities. Recently, Sierra et al. 2015, JAMES 7: 335-356 proposed consistent and formal definitions for intrinsic and apparent sensitivity. It would be nice if the authors referred to that definition or explained how they defined these conceptual sensitivities.

We completely revised the section. According to reviewer 2, we don't use Q10 for field Rs. The text passage is shorter and much easier to read now. We refer to Sierra et.al. in the discussion section with regard to moisture effects on temp sensitivity.

11) l.345: albeit ?

Should be "besides" – changed.

12) Figure 4: The figure is really confusing. Caption doesn't help

We adapted the figure. Should be easy to understand now.

13) Figure 5: not easy to understand that the lines are cumulative. Indicate by filling with different colors that the bottom area is Rh (10 – 30), the second area is Rh (0 –10), the third (upper) one Rh litter and the highest Ra.

We adapted the figure and caption. It should be clear now.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-291/bg-2016-291-AC8-supplement.pdf

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-291, 2016.