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Interactive comment on "Constraint of soil moisture on CO_2 efflux from tundra lichen, moss, and tussock in Council, Alaska using a hierarchical Bayesian model" *by* Y. Kim et al.

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Point-by-point response to Referee #2's comments

We appreciate the invaluable comments from Reviewer #2 regarding the improvement of this manuscript by careful revision.

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"Constraint of soil moisture on CO2 efflux from tundra lichen, moss, and tussock in Council, Alaska using a hierarchical Bayesian model" by Kim and colleagues

For clarity, see Reviewer #2 (blue) in the corrected pdf file (bgd-11-5903-2014-R#1-R#2.pdf). âĂČ (Response to Reviewer #2's Comments)

My general suggestion would be to make paragraphs smaller than they are now, put each significant statement and its implication into a separate paragraph, connect paragraphs better, and delete/merge some figures (more details below).

Additionally, I was not convinced that the model, as it was formulated in the manuscript, accounted for the effect of vegetation type on CO2 efflux. Effect of vegetation type on CO2 efflux was modeled as random effect, same as effect of year on CO2 efflux. Because the effect of vegetation and year were modeled the same way and were additive (according to the model formulation), I wondered whether it was possible to separate those effects? This can be checked by producing a matrix of correlations between the parameters from samples of the posterior parameter distributions. Also, authors discuss differences in CO2 efflux among different vegetation types listed in the Table 1, however, other environmental variables also differ among vegetation types, and may have caused the differences in CO2 efflux.

»> We appreciate your comments and the explanation you describe for P5906 of L28.

Lastly, model validation is an important step in the model development, and I suggest the model from this study is validated against data from couple other studies (Figure 9 shows the correspondence between observed and modeled CO2 flux, however the same data points were used for model calibration).

»> The HB model is an empirical model, and cannot calibrate part of the data in this study due to a lack of information regarding highly parameter-rich models. However, we did use information criteria (DIC) for model selection. Using information criteria enabled us to avoid over-fitting observation data.

»> Further, we re-arranged paragraphs and deleted/merged some figures, as suggested. We are also very really sorry for the confusion regarding section 2.3 on the HB model. The authors misunderstood model descriptions in Biogeosciences discussion and have revised to describe them correctly.

The correct model description is as follows, 2.3 Description of Hierarchical Bayesian (HB) model In a HB model, in order to evaluate the relationship between CO2 efflux and environmental variables, we modeled observed CO2 efflux using a HB model with four explanatory variables: soil temperature (ST), soil moisture (SM), vegetation types (Vege), and thaw depth (THAW). First, CO2 efflux (FCO2) was assumed as normally distributed with mean parameter (μflux) and variance parameter (ðĪIJŐ): F_(ãĂŰCOãĂŮ_2) âĹijnormal(μ_flux,σˆ2). (4) The scale parameter (μflux) was determined from the following equation: μ_flux=f_P f_ST f_SM f_THAW, (5) where fP represents the function of CO2 efflux potential, and fT and fSM are limiting response functions ranging from 0 to 1. fP was defined as follows: f_P=β_0+ ãĂŰVegeãĂŮ_[k] + ãĂŰYearãĂŮ_[l] + ãĂŰPosiãĂŮ_[ij] . (6) fP is a linear predictor with intercept (‘β0’) and three random effects (Vege, Year, and Posi). The Posi term represents a spatial random effect from a conditional autoregressive model (CAR) proposed by Besag et al. (1991). Temperature (fT) is a modified van't Hoff equation as follows: f_ST=eˆ((ST-ãĂŰSTãĂŮ_ref)/10 logâĄą(Q_10)), (7) where fST is the temperature response function, which varies from 0 to 1. The explanatory variable of this function, represented by ST and STref, is a constant, set at 25 °C in this study. The temperature sensitivity parameter is Q10. The soil moisture liming function (fSM) is defined as follows: f_SM=((SM-a)/(b-a))ˆa ((SM-c)/(b-c))ˆ((-d(b-c))/((b-a))), (8)

where the soil moisture response function is fSM, ranging from 0 to 1, and is the same as the temperature response function (Hashimoto et al., 2010). SM is the explanatory variable of this function, and a, b, c, and d are parameters for determining the shape of the soil moisture function. The function has a convex shape, and values range from 0 to 1. Parameters a and c are the minimum and maximum values of SM, respectively (i.e., g(a) = g(c) = 0). Parameter b, which ranges between a and c, is the optimum parameter (i.e., g(b) = 1). Parameter d controls the curvature of the function, though

the three other parameters also affect the shape. This function was adopted from the DAYCENT model (Parton et al., 1996; Del Grosso et al., 2000).

fTHAW is a function of thaw depth. We modeled this as follows: f_THAW=1/(1 + eˆ(k-r THAW)), (9) where the thaw depth function also ranges from 0 to 1. THAW is the explanatory variable of this function, and k and r are the parameters. We assumed CO2 efflux to monotonically increase with increase in thaw depth (depth of active layer); however, this increase is not simply proportional with thaw depth due to carbon depth distribution. Finally, we modeled priors of each parameter. For vegetation, we incorporated random effects as follows: ãĂŰVegeãĂŮ_k âĹijnormal(0,σ_vege) and (10) ãĂŰYearãĂŮ_l âĹijnormal(0,σ_year). (11) For spatial explicit random effect, we used a CAR model (Besag et al., 1991) as follow: ãĂŰPosiãĂŮ_il âĹijnormal(b_ij,σ_(ãĂŰposiãĂŮ_ij)/n) b_il âĹij1/n_ij $\sum\_(m = 1Ĳneighbors(ij))b\_m, wherenijisthenumberofneighborsforneighborhoodij.Forpriors, wed$ âĹijnormal(0,1000), Q_10 âĹijuniform(1,10), a âĹijnuniform(-2,0), b âĹijuniform(0.1,0.5), c âĹijuniform(1,3), d âĹijuniform(0.01,10), k âĹijuniform(0,10), r âĹijuniform(0,1), σˆ2 âĹijuniform(0,100), σ_vegeˆ2 âĹijuniform(0,100), and σ_yearˆ2 âĹijuniform(0,100), (12) For β0, we used a normal distribution with mean 0 and a very large variance. Priors regarding the soil moisture function (a, b, c, d) are based on Hashimoto et al. (2012). We set priors for σvege2 and σyear2 to be vague, with a large enough value for the actual observed CO2 efflux in this study. Joint posterior probability was described as follows: p(θâŤĆdata)âĹİ$\prod Normal(F_(CO_2)|\mu,\beta$_(o,10),a,b,c,d,k,r,$\sigma$_1,ãĂŮ σ_vege,σ_year,σ_(posi)) × p(β_o)× p(Q_10)× p(a)× p(b)× p(c)× p(d) × p(k)× p(r)× p(σ_1)× p(σ_vege)× p(σ_year)× p(σ_(posi)), (13) where p(θ) denotes priors. For this model, we used MCMC methods implemented with Bayesian inference, using Gibbs sampling software WinBUGS (WinBUGS, version 1.4.3; D. Spiegelhalter et al., 2007, available at http://www.mrc-bsu.ac.uk/bugs). We used the Gelman-Rubin convergence diagnostic as an index. For the model, we ran the Gibbs sampler for 20,000 iterations, for three chains, with a thinning interval of 10 iterations. We discarded the first

10,000 iterations as burn-in, and used the remaining iterations to calculate posterior estimates. R was used to call JAGS/WinBUGS and calculate the statistics in R.

Additional comments

P5905,L24: "Davidson et al. (1998) reported CO2 efflux increased with soil moisture of 0.2 m3/m3" I think giving an interval would be more appropriate, e.g. "with soil moisture from 0 to 0.2 m3/m3"

»> I rewrote the following, as suggested by R#2.

Davidson et al. (1998) reported that CO2 efflux increased with soil moisture from 0 to 0.2 m3/m3.

P5906, L7-10: such high Q10 value may not be a true temperature response value. The burst in CO2 efflux in spring may be due to release of CO2 trapped in soil over winter as described in Elberling and Brandt [2003]

»> We appreciate your comments; Higher CO2 concentration in frozen soil came from a spring burst event during soil thawing, and also related to the trapping of produced CO2 during the winter. Also, there is a distinct difference in Q10 value above and below zero; Q10 value below zero was 430 when water content was 39 % (Elberling and Brandt, 2003). On the other hand, Monson et al. (2006a; b), as noted, observed a much higher Q10 value of 1.25×106 in the beneath-snowpack soil of a subalpine forest in early spring.

»> We cited the reference in the introduction of P5906 L7-10, as suggested by R#2. Monson, R. K., Lipson, D. L., Burns, S. P., Turnipseed, A. A., Delany, A. C., Williams, M. W., and Schmidt, S. K.: Winter forest soil respiration controlled by climate and microbial community composition, Nature, 439, doi:10.1038/nature04555, 2006a. Monson, R. K., Burns, S. P., Williams, M. W., Delany, A. C., Weintraub, M., and Lipson, D. L.: The contribution of beneath-snow soil respiration to total ecosystem respiration in a high-elevation, subalpine forest, Global Biogeochem. Cycles, 20, GB3030,

doi10.1029/2005GB002684, 2006b.

P5906,L11: soil temperature is an analogue of soil microbial activity only under certain assumptions, e.g. under an assumption that soil moisture and substrate availability are not limiting factors.

»> We have added this comment to P5906 L11, as suggested by R#2.

Therefore, soil temperature, which is an analogue of soil microbial activity under an assumption that soil moisture and substrate availability are not limiting factors, is the most important factor in producing CO2 in the soil.

P5906,L28: vegetation type was not really an explanatory variable in this study. Like variable "year", it was introducing uncertainty into model prediction resulting from vegetation type variability (in other words, it was formulated as random effect in the prediction model). Is variability from vegetation type separable from interannual variability? Are those two parameters correlated?

In this plot, the vegetation is perennial. Change in the vegetation within the plot is mostly not observed in this study period. Therefore, in theoretically, these two parameters are not correlated with each other. Actually, there were very low correlation (R = 0.137) between τveg and τyear in our result.

»> We added to P5918 L3 explanation at the suggestion of Reviewer #2.

Because changes in vegetation within the plot were not observed in this study period, these two parameters are not correlated with one another. Actually, there was very low correlation (R2 = 0.019) between ïĄťveg and ïĄťyear in our results.

P5906, L29: "under assumption of lognormal distribution" In the methods section all probability distributions are either normal or uniform, where did you use lognormal distribution?

P5907, L2-3: As I mentioned earlier, I don't think that under current model formulation it is possible to evaluate the characteristics of dominant plants on CO2 efflux (unless you account for variation of other environmental variables). However it would be accurate to say that you evaluated random effects on CO2 efflux introduced by vegetation types, assuming they are separable from the random effect of “year”.

P5910, L19: variables beta1 and beta2 are not shown in the equation, and they are not shown in Table 3, where do they come into play?

»> We are sorry for the confusion regarding section 2.3 on the HB model. The authors misunderstood model descriptions in Biogeosciences discussion and have revised to describe them correctly, as previously described.

»> We rewrote section 2.3 on the ðİŻ¡0. Parameters ðİŻ¡1 and ðİŻ¡2 do not exist in this manuscript.

P5910, L21: I think “Qtem” should be changed to “Q10”

»> We corrected ‘Qtem‘ to ‘Q10‘ in Eq (2).

P5911, L7: please, include units and definition of variable WHPS (and THAW as well)

»> The WFPS does not exist in this manuscript and has been corrected.

P5911, L8: “a, b, c, and d are the parameters”

»> Explanation for parameters a, b, c, and d was added in P5911, L8:

The function has a convex shape, and values range from 0 to 1. Parameters a and c are the minimum and maximum values of SM, respectively (i.e., g(a) = g(c) = 0). Parameter b, which ranges between a and c, is the optimum parameter (i.e., g(b) = 1). Parameter d controls the curvature of the function, though the three other parameters also affect the shape. This function was adopted from the DAYCENT model (Parton et al., 1996; Del Grosso et al., 2000).

P5912, L10: again, beta1 and beta2 are not shown in the equations, and they are not

shown in the joint posterior probability and Table 3, what are those?

»> We rewrote section 2.3 regarding ðİŻ¡0. Parameters ðİŻ¡1 and ðİŻ¡2 do not exist in this manuscript after correction.

P5912, L12: what is sigma1? Is it sigma? If it is, the notation shouldn’t be changed

»> We are sorry for the confusion regarding section 2.3 on the HB model. The authors misunderstood model descriptions in Biogeosciences discussion and have revised for correct description, as above-described.

P5913, L6-11: all of these values are listed in the table, rather than re-writing them, I think it is better to summarize them

»> We deleted the values in P5913, L6-10, and described the summarized annual growing season average CO2 effluxes in 2011 and 2012, as suggested by R#2.

P5913, L14-15: environmental variables among the plots with different species differ. Can the differences in CO2 efflux be attributed to environmental variables rather than species cover?

»> Strictly speaking, we agree with these comments, regarding different species indicating differences in CO2 effluxes under different environmental variables. However, much higher CO2 efflux in tussock tundra was observed than in other species, as previously reported (Oechel et al., 1997; Fahnestock et al., 1998). Fahnestock, J. T., Jones, M. H., Brooks, P. D., Walker, D. A., and Welker, J. M.: Winter and early spring CO2 efflux from tundra communities of northern Alaska, J. Geophys., Res., 103, D22, 29023-29027, 1998.

As I mentioned earlier the Results and Discussion section should be carefully revised. Please, make sure that your conclusions are supported by clearly stated evidence. For instance, the conclusion from P5913, L21-23 states that “suggesting that CO2 efflux in tussock is a significant atmospheric CO2 source, ten times greater than in wet sedge”, however it is not supported by evidence the way it is given earlier in the sentence.

»> We rewrote the sentence from P5913, L21-23, as suggested by R#2.

CO_2 efflux in tussock and wet sedge was 0.23 and 0.022 mgCO_2 m-2 min-1, respectively (Oechel et al., 1997), suggesting that CO_2 efflux in tussock is a more significant atmospheric CO_2 source than in wet sedge. This may be due to the difference in the size of the tussock covered by the chamber.

P5913, L23-24: what does this sentence suggest? The conclusion I should draw from this sentence does not seem very clear. Paragraph on pages 5913-5914 needs to be broken down into 2 or 3 paragraphs.

»> We deleted the sentence from P5913, L23-24, and divided the information into two paragraphs of P5913-P591, L7 and P5914, L7-15, as suggested by R#2.

P5914, L16-29: I think the results will have better flow if changes in the environmental variables are described first, followed by description of changes in the CO_2 flux.

»> We deleted this sentence from P5913, L23-24, and divided the information into two paragraphs of P5913-P5914, L7 and P5914, L7-15, as suggested by R#2.

P5915, L6: "significant" instead of "significantly"; where is the result showing one-way ANOVA for thaw depth?

»> We added results from P5914, L6, as suggested by R#2.

P5915, L7-8: the statement that thaw depth was not related to CO_2 flux and soil temperature contradicted results in Figure 5.

»> We rewrote the sentence from P5914, L7-8, as suggested by R#2.

The distribution of thaw depth (not shown) seems similar to the pattern of soil moisture, which is inversely related to those of CO_2 efflux and soil temperature.

Table 2: Q10 values in this table are different from the value reported in Table 3, and are often outside of the 97.5% confidence interval. It would be very interesting to see

the explanation for the differences in the values. Where the differences caused by variation in soil moisture, thaw depth, and/or other factors?

»> We derived Q10 values suggested in Table 2 from the relationship between CO_2 efflux and soil temperature alone; however, the Q10 value reported in Table 3 is from the HB model. According to soil temperature as well as soil moisture/thaw depth from the HB model, the Q10 values from Table 3 may be much lower than those from Table 2, due to the inverse relationship between CO_2 efflux and two parameters for the entire growing seasons.

Table 3: where in equations was the term "deviance" estimated?

»> Deviance is the index of fitting the model to observed data, and not parameter. It may be confusing here. The fitness of the model is described in the following Figure (RMSE and ME), rather than the deviance.

Figures 2 and 3: I don't think figures 2 and 3 are critical to show in this study

»> We deleted Figure 3, as suggested by both R#2 and R#1.

Figure 6: this figure repeats what is already shown in figure 1 and figure 5

»> We deleted Figure 6 and provided description in the text, and added to Figure 1 the explanation of accumulative rainfall in 2011 from Figure 6, as suggested by R#2:

This seems to be the effect of heavy rainfall since August 20, 2012, as shown in Figure 1, which represents daily and accumulative precipitation in 2011 and 2012. Interestingly, accumulative rainfall began to surpass 2011 accumulative precipitation in August 20, 2012 (not shown).

Figure 7: it seems that temperature limitation function is well constrained unlike moisture limitation function or thaw function. Why do you think they are unconstrained? Can it be related to different vegetation types? It would be interesting to estimate parameters from table 3 for each vegetation type separately (except the standard deviation

for the Vege parameter), and see whether parameter values were significantly different from each other. This way it would be possible to estimate the effect of vegetation on the environmental limitation function.

»> Thank you for your comments. We tried to model different sensitivity for each vegetation type. However, we failed to estimate some (divergent) sensitivity due to some vegetation types with small numbers of samples.

Figure 8: not sure this figure is essential to present for this study

»> We deleted Figure 8, as suggested by R#2.

Figure 9: this figure is useful to illustrate how well your model represents the data used for calibration, however, model validation is an essential stage in model development. I suggest merging the data from 6 panels into one, and do some data mining from the literature to find co2 efflux, thaw depth, soil moisture etc to fit the model for validation. An example for model validation data could be data from Oberbauer et al. [1992], who also estimate model parameters to CO2 flux data. It would be also interesting to see whether the model in this study performs better than the model presented in Oberbauer et al.'s study.

»> I appreciate your suggestion. This reference is very important for our study. However, there were no data regarding soil moisture. On the other hand, our study also lacked observation regarding the soil water table. If we had observations for the water table, we could conduct the study you have noted here. The empirical model from Oberbauer et al. (1992) is very similar to our model. We have cited this model in the Introduction section of P5906, L16. We have aggregated six panels into one figure as follows.

Reference: Oberbauer, S. F., C. T. Gillespie, W. Cheng, R. Gebauer, A. S. Serra, and J. D. Tenhunen (1992), Environmental effects on CO2 efflux from riparian tundra in the northern foothills of the Brooks Range, Alaska, USA, Oecologia, 92(4), 568-

577.10.1007/bf00317851.

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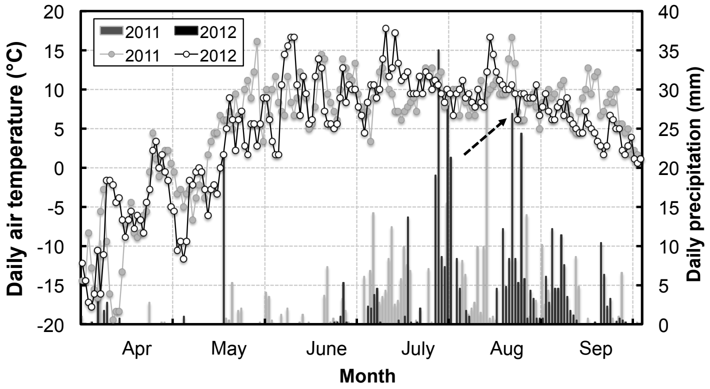

Figure 1

Fig. 1. Revised Figures and Tables