Point-by-point response to Referee #1 and 2's comments

We appreciate the invaluable comments from Reviewer #1 regarding the improvement of this manuscript by careful revision.

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Biogeosciences Discuss., 11, 5902-5939, 2014 (doi:10.5194/bgd-11-5903-2014)

"Constraint of soil moisture on CO_2 efflux from tundra lichen, moss, and tussock in Council, Alaska using a hierarchical Bayesian model" by Kim and colleagues

For clarity, see Reviewer #1 (yellow) and #2 (**blue**) in the corrected pdf file (**bgd-11-5903-2014-R#1.pdf**).

(Response to Reviewer #1's Comments)

General Comments

We have addressed the characteristics of the research site, which include limited accessibility and particular precipitation events in 2011 and 2012 (see Figure 1). Further, it was difficult to measure CO_2 efflux due to unstable, heavy precipitation events during the growing season of 2012. With this in mind, we had to conduct one or two CO_2 efflux measurements under clear sky for the observation period. We used the HB model to overcome limited efflux measurements for the observation month (Nashina et al., 2009; 2012).

As follows, my colleagues and I have carefully revised the manuscript as suggested by Reviewer #1's comments.

We also deleted Figures 3 and 4 and added supplementary material, as suggested by Reviewer #1.

Response to Specific Comments

Abstract L12-18: soil moisture causes 1.4-fold difference in CO2 efflux between two growing season, yet temperature "as the most important parameters in regulating CO2 efflux". More clarifications are needed here, maybe specify the importance of moisture and temperature on different temporal scales? That moisture contributes to interannual CO2 efflux more and that temperature controls seasonal variation?

>>> Yes, while temperature controls the seasonal variation in CO_2 efflux, soil moisture contributes to the interannual variation of CO_2 efflux, as pointed out by Reviewer #1.

>>> We rewrote P5904 L18-19 of the Abstract, as follows.

This reveals that soil temperature regulates the seasonal variation of CO2 efflux, and that soil moisture contributes to the interannual variation of CO2 efflux for the two growing seasons in question.

Abstract L24: the use of "period" as flux unit needs more clarification, do you mean growing season as a period? If so, then period-1 may be omitted. How is the proportion of annual rates of the whole western tundra ecosystem estimated? A brief sentence in the abstract explaining this would be preferred.

>>> We removed the unit and added explanation in P5904 L24 of the Abstract and

P5920 L16 as follows.

P5904 L24: The estimated growing season CO_2 emission rate ranged from 0.86 ~~$MgCO_2$ period^{-1}~~ in 2012 to 1.20 $MgCO_2$ ~~period^{-1}~~ in 2011, within a 40 m × 40 m plot, corresponding to 86 % and 80 % of annual CO_2 emission rates within the Alaska western tundra ecosystem, estimated from the temperature dependence of CO_2 efflux.

P5920 L16: That is, the simulated CO_2 emission rates were 0.86-1.20 $MgCO_2$ ~~period^{-1}~~ within a 40 m × 40 m plot during the growing seasons of 2012 and 2011, respectively.

>>> Regarding the calculation, we simply multiplied CO_2 emissions (539 and 742 gCO_2 m^{-2} period^{-1}) by 1400 m^2 (within a 40 m × 40 m plot). Further, annual CO_2 emission of the whole western tundra ecosystem can be estimated using Eq (2), as written in P5920 L10.

P5906 L16-17: "If spatial distribution is . . . cause estimation bias". The sentence may be further clarified. Spatial distribution of what? Do you mean the spatially clumped monthly CO2 efflux or the repeatedly measured (time series) of CO2 efflux? How is the ensemble average defined here? Expand this sentence into several and provide more details should make the message clearer.

>>> We deleted this sentence because it has no particular meaning for this study.

P5910 L15-21: "fp is a linear predictor that has three parameters", but only β0 appeared in eqn 6, where is β1 and β 2? Also, β is not defined in eqn7.

>>> We rewrote section 2.3 for the β_0. Parameters β_1 and β_2 do not exist for this manuscript.

P5911 L3: Is Qtem the same as Q10 in eqn3? Or should "tem" be "ten"?

>>> We changed Q_{tem} to Q_{10} in Eq (2).

P5911 L5: eqn 8, WFPS has not been defined in previous text.

>>> WFPS does not exist for this manuscript.

P5911-5912: how is the probability density function of hyperpriors obtained? For example, what is the basis for assuming the same variance of vegetation and year random effects? The posterior parameter distribution can be very sensitive to priors and the resulting conditional distributions. The hyperpriors for $\beta 0$, $\beta 1$ and $\beta 2$ are missing from the list and eqn 13. Should not eqn 13 be ". . .Normal(F| u,δ)×p(u, $\delta|\beta$, a,b,c. . ..)×p(β)×p(a)×. ."

>>> We have added explanation regarding how to get priors as follows.

We set priors for σ_{vege}^2 and σ_{year}^2 to be vague, meaning large enough in value to accommodate the actual observed CO_2 efflux of this study.

>>> We are very sorry for the confusion. The authors misunderstood model descriptions in ***<u>Biogeosciences discussion</u>***. **The four comments regarding the HB above** were revised in this version, as suggested. We appreciate your helpful comments.

<u>The correct model description is as follows</u>,

2.3 Description of Hierarchical Bayesian (HB) model

To evaluate the relationship between CO_2 efflux and environmental variables, we modeled observed CO_2 efflux using an HB model with four explanatory variables: soil temperature (ST), soil moisture (SM), vegetation types (Vege), and thaw depth (THAW).

First, CO_2 efflux (F_{CO2}) was assumed normally distributed with mean parameter (μflux) and variance parameter (σ):

$$F_{CO_2} \sim normal(\mu_{flux}, \sigma^2), \quad (4)$$

The scale parameter (μ_{flux}) was determined from the following equation:

$$\mu_{flux} = f_P f_{ST} f_{SM} f_{THAW}, \quad (5)$$

where f_P represents the function of CO_2 efflux potential, f_T and f_{SM} are limiting response functions ranging from 0 to 1. f_P was defined as follows:

$$f_P = \beta_0 + Vege_{[k]} + Year_{[l]} + Posi_{[ij]}, \quad (6)$$

f_P is a linear predictor with intercept ('β_0') and three random effects (*Vege*, *Year*, and *Posi*). The *Posi* term represents the spatial random effect of the conditional autoregressive model (CAR) proposed by Besag et al. (1991).

Temperature (f_T) is a modified Van't Hoff equation as follows:

$$f_{ST} = e^{\frac{ST-ST_{ref}}{10}\log(Q_{10})}, \quad (7)$$

where f_{ST} is the temperature response function, varing from 0 to 1. The explanatory variable of this function, represented by *ST* and ST_{ref}, is a constant, set at 25 °C in this study. The temperature sensitivity parameter is shows by Q_{10}. The soil moisture limting function (f_{SM}) is defined as follows:

$$f_{SM} = \left(\frac{SM-a}{b-a}\right)^{a}\left(\frac{SM-c}{b-c}\right)^{-d(b-c)/(b-a)}, \quad (8)$$

where the soil moisture response function is f_{SM}, ranges 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).

f_{THAW} is a function of thaw depth. We modeled this as follows:

$$f_{THAW} = \frac{1}{1 + e^{k-r\,THAW}}, \quad (9)$$

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 CO_2 efflux to monotonically increase together with increase in thaw depth (depth of active layer); however, these increases are not simply proportional, due to carbon depth distribution.

Finally, we modeled the priors of each parameter. For vegetation, we incorporated a random effect as follows:

$$Vege_k \sim normal(0, \sigma_{vege}); \quad (10)$$

$$Year_l \sim normal(0, \sigma_{year}). \quad (11)$$

For spatial explicit random effect, we used CAR modeling (Besag et al., 1991), as follows:

$$Posi_{il} \sim normal(b_{ij}, \frac{\sigma_{posi_{ij}}}{n})$$

$$b_{il} \sim \frac{1}{n_{ij}} \sum_{m=1}^{neighbors(ij)} b_m \;,$$

where n_{ij} is the number of neighbors for neighborhood ij.

For priors, we defined as follows:

$$\beta_0 \sim normal(0, 1000),$$

$$Q_{10} \sim uniform(1, 10),$$

$$a \sim uniform(-2, 0),$$

$$b \sim uniform(0.1, 0.5),$$

$$c \sim uniform(1, 3),$$

$$d \sim uniform(0.01,\ 10),$$

$$k \sim uniform(0, 10),$$

$$r \sim uniform(0, 1),$$

$$\sigma^2 \sim uniform(0,\ 100),$$

$$\sigma_{vege}^2 \sim uniform(0, 100), \text{ and}$$

$$\sigma_{year}^2 \sim uniform(0, 100). \quad (12)$$

For β_0, 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 σ_{vege}^2 and σ_{year}^2 to be vague, meaning large enough in value to accommodate the actual observed CO_2 efflux of this study.

The joint posterior probability was described as follows:

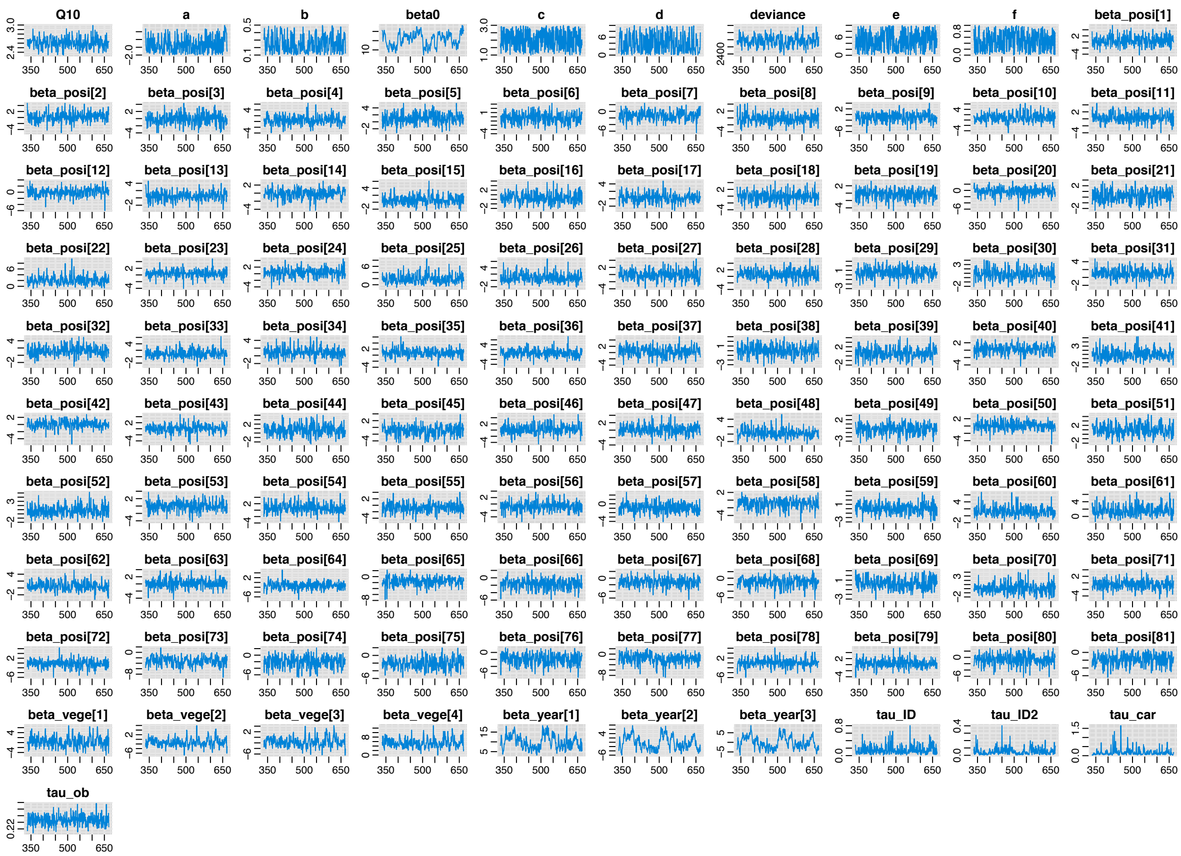
$$p(\theta|data) \propto \prod Normal(F_{CO_2}|\mu, \beta_{o,10}, a, b, c, d, k, r, \sigma_1, \sigma_{vege}, \sigma_{year}, \sigma_{posi}) \times p(\beta_o) \times p(Q_{10}) \times p(a) \times p(b) \times p(c) \times p(d) \times p(k) \times p(r) \times p(\sigma_1) \times p(\sigma_{vege}) \times p(\sigma_{year}) \times p(\sigma_{posi}), \quad (13)$$

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), and the Gelman-Rubin convergence diagnostic as an index. For the model, we ran 20,000 Gibbs sampler 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.

P5912: It would be good to have a graph showing the convergence of the Gibbs sampler results. Maybe put in the supplementary.

>>> We added convergence plots in the supplemental material as follows.



Supplementary material: Convergence plot of all HB model parameters

Fig 10: why did not soil moisture drop rapidly in Sep 2012 when temperature dropped to zero as oppose to 2011?

>>> I fully understand the concern. Soil moisture dropped rapidly in September 2012, when soil temperature dropped below zero. However, if we measured soil temperature after mid-September 2011, soil moisture would show a similar drop. Because of the weakness of the solar power supply in the late growing season, we could measure **only growing season** soil temperature and moisture for 2011 and 2012.

>>> Contrary to Sep 2011, air temperature in Sep 2012 dropped rapidly, as shown in Figure 1. These data came from the Western Regional Climate Center of the National Weather Service, Alaska. This is not *in-situ* data; however, *in-situ* air temperature in mid-Sep 2012 did read below zero, despite the short period of observation (ca. 2-month) caused by trouble from the power supply, as shown in the following Figure.

>>> Recently, we solved these mechanical problems by installing sensors and loggers for soil temperature and moisture, obtaining year-round data since 2012.

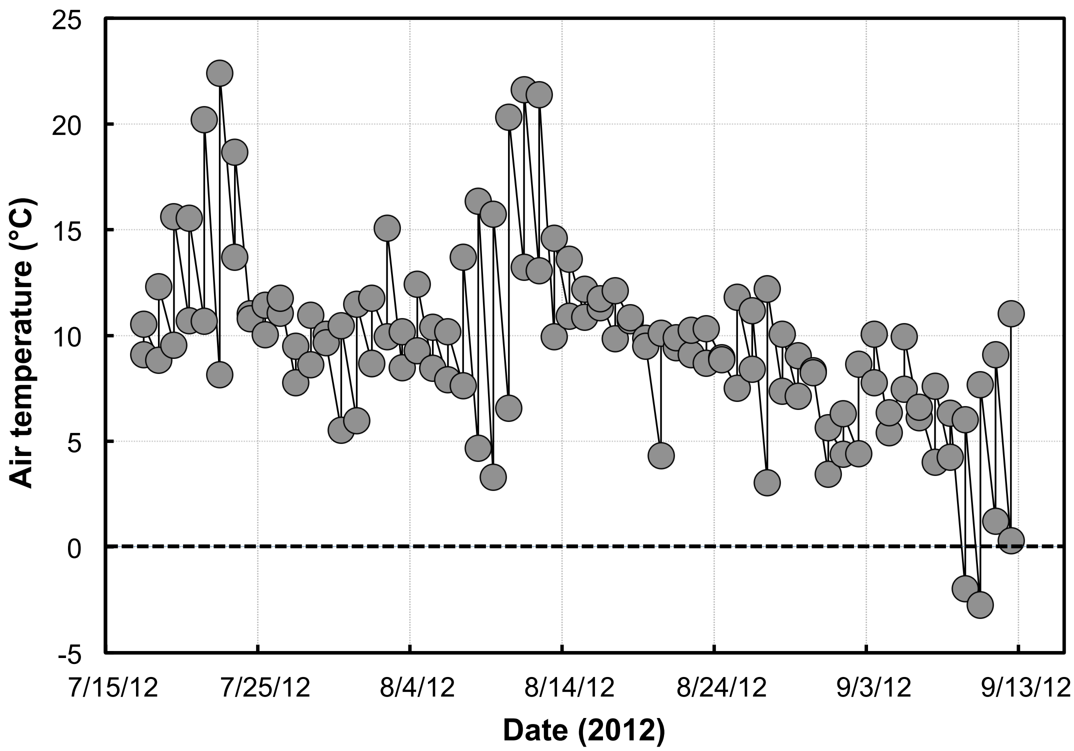


Figure. *In-situ* air temperature data in Council, Alaska from mid-July to mid-September, 2012.

P5918 L25: the effect of thaw depth shown in fig 5c is not quite similar to the limiting function used (assumed) in eqn 8 (fig7c). More discussion of the interacting effects of thaw depth and soil moisture may be needed, as it is likely that the thaw depth effect is masked by moisture.

>>> In view of the observation, we expected increasing CO_2 efflux as thaw depth deepens; however, this expectation was contradicted. Further, there were different meteorological patterns between years, which may be due to effects from heavy

rainfall in 2012. Some relationship between thaw depth and moisture may be represented if thaw depth was regulated by the masking effect of soil moisture. However, we could not find any relationship between the two in our simple empirical model. In the HB model, by assuming a possible relationship between flux and thaw depth, under constraint of parameter estimation from priors, we could estimate the positive (though weakly so, in the actual range of thaw depth during the measurement period) effect of THAW depth and non-linear relationship of soil moisture respectively.

P5920 L10: So the annual estimation of CO2 emission from tundra ecosystem is based on eqn 2. Did you use the HB results for parameters in this extrapolation?

I would recommend re-estimate those parameters in eqn2 as that way the new parameters can compensate model structural insufficiencies (compare with HB model) to some extent.

>>> We recalculated and corrected annual CO_2 emission for 2011 and 2012 using the parameters 827 and 609 gCO_2 m^{-2} $year^{-1}$, respectively, in P5920 L10, as suggested by Reviewer #1.

Table 3: some parameters showed quite a posterior 95% CI, especially soil moisture related parameters. I am wondering if a simpler moisture effect function (eqn8) can be used or maybe compared with the current one to see if there is an overparameterization issue with the complicated model with fewer degrees of freedom.

There seems to be too many figures in the manuscript, some of them deliver limited message (neither closely related to the main message of the manuscript nor receive ample discussion), such as fig3 and 4. I suggest replace them with other indepth results from HB model analysis if any or just delete or put in supplementary information.

>>> We deleted Figures 3 and 4 and added the convergence plot for all HB model parameters, as suggested by Reviewer #1.

Thank you for this suggestion. We have conducted two types of soil moisture function for the HB model and evaluated DIC (deviance information criteria) in each model as follows:

	DIC
1) This study (revised one)	2463.9

$$f_{SM} = \left(\frac{SM-a}{b-a}\right)^{a}\left(\frac{SM-c}{b-c}\right)^{-d(b-c)/(b-a)}$$

2) Simpler function 2505.3

$$f_{SM} = e^{\left(-ep\left(1-\frac{SM}{Wopt}\right)^2\right)}$$

In view of model selection using criteria, lower DIC means higher predictability for the fitted model, which is judged by a balance of performance and model complexity, owing to parameter parsimony. These results suggest that the current model still performs well, compared to the model with two parameters for soil moisture function. Therefore, we continue to use the current model in the revised manuscript. However, we have huge questions for possible models regarding non-linear function (including linear models). As a result, we cannot compare possible model combinations.

Supplementary material is unavailable following the link in the manuscript.

>>> We have attached supplementary material regarding the convergence plot for all HB model parameters, as suggested by Reviewer #1

Technical issues:

P5905-L27: I understand the authors use “parameter” to refer to environmental factors controlling CO2 efflux, but technically parameter refers to a time-invariant subject that characterizes the modeling system, and soil temperature in this context, is regarded as forcing of the modeling system whereas how we characterize the “effect” of temperature on soil CO2 efflux can be a parameter. I recommend the authors change the “parameter” to “factors” or “environmental variable” as such throughout the manuscript to clarify such mixed usage.

>>> We rewrote ‘parameters’ relating to the HB model, and ‘factors’ for other cases, as suggested by Reviewer #1.

P5913 L10: “Annual average” to “Annual growing season average”

>>> We changed ‘annual average’ to ‘annual growing season average,’ as suggested by Reviewer #1.

Table 3: “fro” to “of”. Some parameters in this list do not match those in the text.

>>> We rewrote and corrected them, as suggested by Reviewer #1.

(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

To evaluate the relationship between CO_2 efflux and environmental variables, we modeled observed CO_2 efflux using an HB model with four explanatory variables:

soil temperature (ST), soil moisture (SM), vegetation types (Vege), and thaw depth (THAW).

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The scale parameter (μ_{flux}) was determined from the following equation:

$$\mu_{flux} = f_P f_{ST} f_{SM} f_{THAW}, \quad (5)$$

where f_P represents the function of CO_2 efflux potential, f_T and f_{SM} are limiting response functions ranging from 0 to 1. f_P was defined as follows:

$$f_P = \beta_0 + Vege_{[k]} + Year_{[l]} + Posi_{[ij]}, \quad (6)$$

f_P is a linear predictor with intercept *('β_0')* and three random effects (*Vege*, *Year*, and *Posi*). The *Posi* term represents the spatial random effect of the conditional autoregressive model (CAR) proposed by Besag et al. (1991).

Temperature (f_T) is a modified Van't Hoff equation as follows:

$$f_{ST} = e^{\frac{ST-ST_{ref}}{10}\log(Q_{10})}, \quad (7)$$

where f_{ST} is the temperature response function, varing from 0 to 1. The explanatory variable of this function, represented by *ST* and ST_{ref}, is a constant, set at 25 °C in this study. The temperature sensitivity parameter is shows by Q_{10}. The soil moisture limting function (f_{SM}) is defined as follows:

$$f_{SM} = \left(\frac{SM-a}{b-a}\right)^{a} \left(\frac{SM-c}{b-c}\right)^{-d(b-c)/(b-a)}, \quad (8)$$

where the soil moisture response function is f_{SM}, ranges 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).

f_{THAW} is a function of thaw depth. We modeled this as follows:

$$f_{THAW} = \frac{1}{1 + e^{k - r\,THAW}}, \quad (9)$$

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 CO_2 efflux to monotonically increase together with increase in thaw depth (depth of active layer); however, these increases are not simply proportional, due to carbon depth distribution.

Finally, we modeled the priors of each parameter. For vegetation, we incorporated a random effect as follows:

$$Vege_k \sim normal(0, \sigma_{vege}); \quad (10)$$

$$Year_l \sim normal(0, \sigma_{year}). \quad (11)$$

For spatial explicit random effect, we used CAR modeling (Besag et al., 1991), as follows:

$$Posi_{il} \sim normal(b_{ij}, \frac{\sigma_{posi_{ij}}}{n})$$

$$b_{il} \sim \frac{1}{n_{ij}} \sum_{m=1}^{neighbors(ij)} b_m \ ,$$

where n_{ij} is the number of neighbors for neighborhood ij.

For priors, we defined as follows:

$$\beta_0 \sim normal(0, 1000),$$

$$Q_{10} \sim uniform(1, 10),$$

$$a \sim uniform(-2, 0),$$

$$b \sim uniform(0.1, 0.5),$$

$$c \sim uniform(1, 3),$$

$$d \sim uniform(0.01,\ 10),$$

$$k \sim uniform(0, 10),$$

$r \sim uniform(0,1)$,

$\sigma^2 \sim uniform(0, 100)$,

$\sigma_{vege}^2 \sim uniform(0,100)$, and

$$\sigma_{year}^2 \sim uniform(0,100). \quad (12)$$

For β_0, 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 σ_{vege}^2 and σ_{year}^2 to be vague, meaning large enough in value to accommodate the actual observed CO_2 efflux of this study.

The joint posterior probability was described as follows:

$$p(\theta|data) \propto \prod Normal(F_{CO_2}|\mu, \beta_{o,10}, a, b, c, d, k, r, \sigma_1, \sigma_{vege}, \sigma_{year}, \sigma_{posi}) \\ \times p(\beta_o) \times p(Q_{10}) \times p(a) \times p(b) \times p(c) \times p(d) \\ \times p(k) \times p(r) \times p(\sigma_1) \times p(\sigma_{vege}) \times p(\sigma_{year}) \times p(\sigma_{posi}), \quad (13)$$

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), and the Gelman-Rubin convergence diagnostic as an index. For the model, we ran 20,000 Gibbs sampler 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 CO_2 efflux increased with soil moisture **from 0 to 0.2** m^3/m^3.

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 CO_2 concentration in frozen soil came from a spring burst event during soil thawing, and is also related to the trapping of produced CO_2 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 1.25×10^6 in the beneath-snowpack soil of a subalpine forest in early spring.

>>> We have 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 the 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 vegetation within the plot is mostly not observed in this study period. Therefore, theoretically, these two parameters are not correlated with each other. In actuality, there was very low correlation ($R = 0.137$) between τ_{veg} and τ_{year} 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 during this study period, these two parameters are not correlated with one another. In actuality, there was very low correlation (R2 = 0.019) between tveg and tyear 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 β_0. Parameters β_1 and β_2 do not exist in this manuscript.

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

>>> We corrected 'Q_{tem}' to 'Q_{10}' 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 β_0. Parameters β_1 and β_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 described above.

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 CO_2 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 CO_2 effluxes under different environmental variables. However, much higher CO_2 efflux in tussock tundra than in other species was indeed observed, 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 CO_2 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 indeed a more significant atmospheric CO_2 source than wet sedge. This may in fact 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 CO2 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 CO2 flux and soil temperature contradicted results in Figure 5.

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

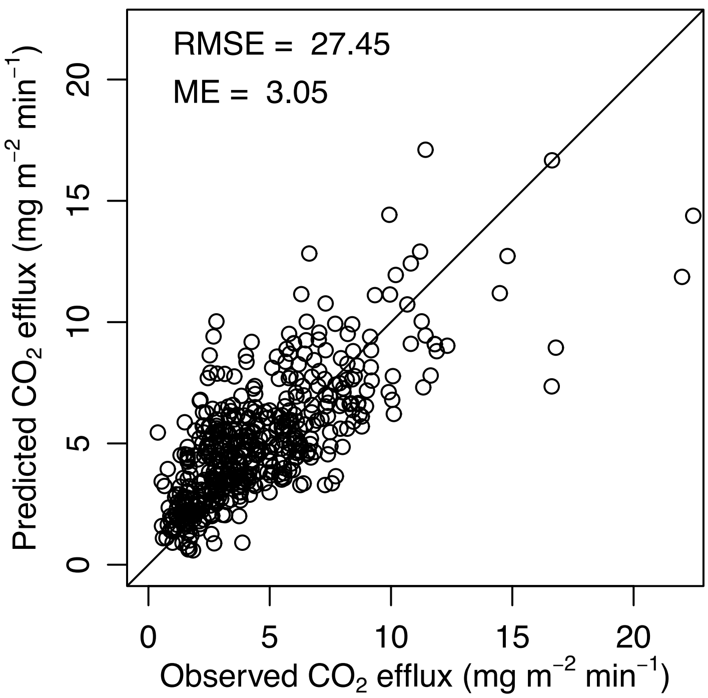
The distribution of thaw depth (not shown) appears similar to the soil moisture pattern, which is **inversely** related to ~~those of~~ CO_2 efflux and soil temperature.

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 beginning on August 20, 2012, as shown in Figure 1, which represents daily and accumulative precipitation in 2011 and 2012. Interestingly, cumulative rainfall indeed began to surpass 2011 cumulative precipitation on 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

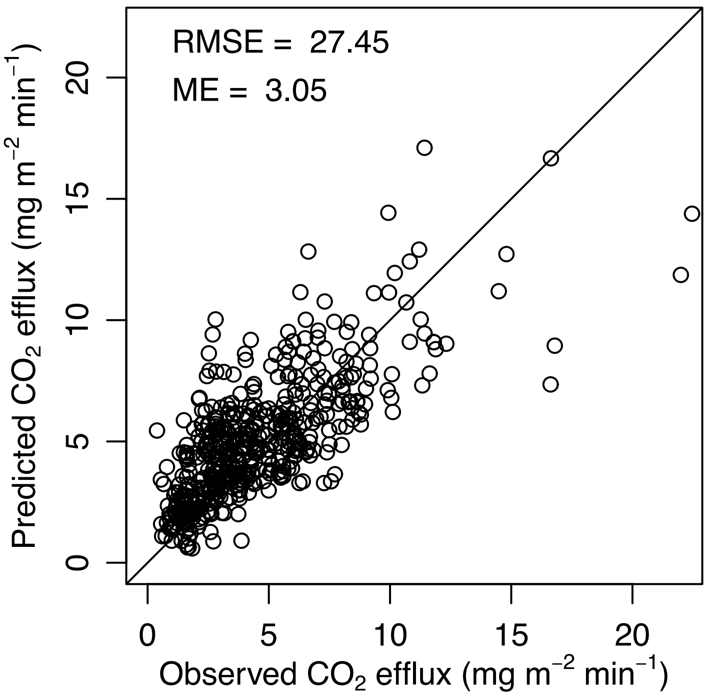
>>> We deleted Figure 8, as suggested by R#2.

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 CO_2 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.