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Interactive
Comment

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.

Anonymous Referee #2

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The paper by Kim et al. "Constraint of soil moisture on CO2 efflux from tundra lichen, moss, and tussock in Council, Alaska using a hierarchical Bayesian model" addresses an important issue in carbon cycle modeling research: using maximum amount of information from the observations to improve a model. The authors focus on constraining CO2 efflux in arctic tundra, one of the most vulnerable biomes to climate change. Kim et al. use Bayesian inversion to constrain parameters associated with environmental limitation of CO2 efflux, a method that allows improve a model and assign uncertainties to model predictions based on the observations. During their investigation they were able to constrain functions of temperature, moisture, and thaw depth limitation of CO2 efflux, and the results look very interesting. However, the presentation of results,

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which are very interesting and useful to the modeling community, needs considerable improvement. While the introduction and description of methods are clear, the presentation of results is rather chaotic, which makes it extremely hard to make sense of the results. 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. 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).

Please, see additional comments below.

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”

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]

BGD

11, C3432–C3436, 2014

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.

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?

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?

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

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

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

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?

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

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

P5913, L14-15: environmental variables among the plots with different species differ. Can the differences in CO_2 efflux be attributed to environmental variables rather than species cover?

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 CO_2 efflux in tussock is a significant atmospheric CO_2 source, ten times greater than in wet sedge”, however it is not supported by evidence the way it is given earlier in the sentence.

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.

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.

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

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

Table 2: Q_{10} 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?

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

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

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

BGD

11, C3432–C3436, 2014

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.

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

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.

References:

Elberling, B., and K. K. Brandt (2003), Uncoupling of microbial CO2 production and release in frozen soil and its implications for field studies of arctic C cycling, Soil Biology and Biochemistry, 35(2), 263-272.http://dx.doi.org/10.1016/S0038-0717(02)00258-4

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

Interactive comment on Biogeosciences Discuss., 11, 5903, 2014.