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Interactive comment on "Constraint of soil moisture on CO₂ efflux from tundra lichen, moss, and tussock in Council, Alaska using a hierarchical Bayesian model" *by* Y. Kim et al.

Anonymous Referee #1

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General comments:
Kim et al. and colleagues used a Hierarchical Bayes (HB) model to identify dominating factors controlling CO2 efflux from an arctic tundra ecosystem based on two years growing season field measurements. They used a well-established statistical method to fit the data and reproduced observed CO2 efflux. The layout of the manuscript is straightforward and easy to understand. However, the data presented were relatively limited (only one plot) and the findings were not quite new and novel. At its core, HB is a device using sub-models to account for uncertainty. This study does not really fully explored the advantages of HB model in the uncertainty analysis of the parameters and the subsequent knowledge we obtained from constrained parameters and model,

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but rather mainly focus their discussion on regression analysis results (ANOVA and correlation analysis). In future iterations, more indepth analysis of the HB model results, particularly posteriors of parameters and the model itself may be needed. One potential perspective might be how the HB model informs us of the limitations of our conceptual representation of the processes (see specific comments below) and how can we improve.

Specific comments:

Abstract L12-18: soil moisture causes 1.4-fold differences in CO_2 efflux between two growing season, yet temperature "as the most important parameters in regulating CO_2 efflux". More clarifications are needed here, maybe specify the importance of moisture and temperature on different temporal scales? That moisture contributes to interannual CO_2 efflux more and that temperature controls seasonal variation?

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.

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 CO_2 efflux or the repeatedly measured (time series) of CO_2 efflux? How is the ensemble average defined here? Expand this sentence into several and provide more details should make the message clearer.

P5910 L15-21: "fp is a linear predictor that has three parameters", but only $\beta 0$ appeared in eqn 6, where is $\beta 1$ and $\beta 2$? Also, θ is not defined in eqn7.

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

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

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

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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,δ|β, a,b,c....)×p(β)×p(a)×..."

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

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

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.

P5920 L10: So the annual estimation of CO_2 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.

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 over-parameterization 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. Supplementary material is unavailable following the link in the manuscript.

Technical issues:

P5905-L27: I understand the authors use "parameter" to refer to environmental factors controlling CO_2 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.

P5913 L10: "Annual average" to "Annual growing season average"

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

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