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Interactive comment on "Calibrating a wetland methane emission model with hierarchical modeling and adaptive MCMC" *by* Jouni Susiluoto et al.

Anonymous Referee #1

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Review of Calibrating a wetland methane emission model with hierarchical modeling and adaptive MCMC, Geoscientific Model Development

The authors use a reduced order model representation of a complex numerical wetland methane model with adaptive MCMC to estimate posterior distributions of model parameters. Because wetland methane models are complex and surface CH4 emissions are small differences between large gross fluxes, this type of calibration exercise is difficult and potentially valuable.

The authors did a nice job describing their results, given their model setup and assumptions. At this point, however, several assumptions in the approach leave me unconvinced of the reasonableness of the results, which I describe below. Also, the

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Discussion paper

underlying model has not been described in the literature, and citing “Raivonen et al. (2017)” is inappropriate, since it is apparently a paper in preparation.

Major comments 1. It is unclear why you need to vary the peat depth in your optimization. You apparently have measurements of the depth (lines 22-23, page 4), so you ought to use that as a non-calibratable value in the model. The high sensitivity of your parameter calibration to the peat depth implies to me that some other factor must be important and not properly resolved in your underlying model (e.g., O2 profiles below the WT depth or rooting profiles). The argument on line 31, page 6 that it is more computationally expensive to run with a deeper peat depth is not sufficient to justify this approach. 2. Changing z_exu and Q10 on a yearly basis seems arbitrary. For Q10, I would expect much larger seasonal than inter-annual variations, yet you ignore that possibility. I think you need to explicitly describe the mechanisms you are proposing for the inter-annual variability of these parameters. The citation to Bergman et al 2000, who noticed change in Q10, "even within a single year", seems to bely your approach. 3. Ignoring the temperature sensitivity of CH4 oxidation appear to be a flaw in your approach. There is substantial evidence that this temperature dependence is even larger than that for CH4 production. Further, since the net surface emission is a small balance of production and oxidation rates, and you explicitly account for the CH4 production temperature sensitivity (eq. (5)), not including Delta(E_R) in your equation (7) seems unreasonable. a. This concern propagates to the last sentence in your abstract. Oxidation often strongly affects the net CH4 emission, which is the measurement you are using to calibrate. If parameters affecting oxidation are ‘not identifiable’, then it seems unlikely that the production parameters are reasonable. 4. On line 21 of page 7, you state that V_R0 affects the rate of temperature dependent HR, but the T dependency is actually governed by Delta(E_R) which is not used in the calibration (Table 2). In general, it is unclear in your section 3.4 how the CH4 production occurs and its relationship with heterotrophic respiration. 5. In section 4.1, you say the model was linearized, but you did not show whether such a linearization is a reasonable approach. Please provide a quantitative evaluation of how appropriate this linearization

is. 6. Lines 22-25, page 9: Having to restart the model on January 1 of each year because realistic column gas concentrations were otherwise not predicted is a red flag for a problem in the model. The model should be able to run continuously without interruption. If this is a real problem in the model, you should rectify it. Once rectified and described in the paper, restarting each year for computational efficiency and parallelization is reasonable. 7. Section 4.2.4 seems to apply that your objective function is only based on annual values, but the text implies that you use the annual values to linearize the model, and then perform the parameter calibration with observed daily CH4 emissions. Please clarify.

Smaller comments: 1. Methane is the second most important anthropogenic GHG for warming (don't forget water vapor). 2. You describe annual calibration in they abstract, but not the fact that you used a ROM and then daily fluxes for calibration (as far as I can tell). This approach should be described in the abstract. 3. Line 13-15, page 2: cite recent methane model inter-comparisons here: Melton et al., Bohn et al. 4. Your assertion (lines 33-34, page 2 to line 4, page 3) that flawed physics representations, numerical errors, and coding errors are good reasons to calibrate a model is shocking. Calibrating a poorly constructed model is a cardinal sin of modeling, although it is regularly done. I think you might re-think the organization of this paragraph. 5. Line 5, page 3: Possibly the most mechanistic and realistic terrestrial CH4 model available today is ecosys (Grant, 2002), which you should cite. 6. Line 8, page 3: define 'multimodality' 7. Your figures are cited out of order in the text (e.g., figure 11 cited just after figure 2). 8. Line 27, page 12: do you mean 'inter-annual variability' instead of 'annual variability'? 9. Line 9, page 12: there is no figure 6g. 10. Does the model calculate the peat temperature? It is not clear from your description which T you are using to estimate your temperature sensitivity. Air T? 11. What happened to a discussion of figure 10? 12. Where did the NPP come from? Describe in Methods.

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