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Geoscientific Model Development Discussions

Interactive comment on "Integrating peatlands into the coupled Canadian Land Surface Scheme (CLASS) v3.6 and the Canadian Terrestrial Ecosystem Model (CTEM) v2.0" *by* Y. Wu et al.

Anonymous Referee #1

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This manuscript presents a description and initial evaluation of a new peatland model built for the Canadian Land Surface Scheme (CLASS) and the associated Canadian Terrestrial Ecosystem Model (CTEM). The manuscript does an excellent job introducing the importance of accurately modeling peatlands within earth system models, and includes a well-written, brief review of existing peatland models before presenting the new model developments. The conceptual description of the new peatland model is well written and clear, and the evaluation against eddy covariance flux measurements, water table measurements, and soil temperatures is well presented and generally well designed. The model itself appears to do a good job of incorporating current understanding of peatland vegetation and soil processes, and should be a useful tool for

simulating peatlands going into the future. I think there is some room for improvement in the technical descriptions of the model equations and the flux data used to evaluate the model. I also think that the conclusion that separate parameterizations for bogs and fens are unnecessary for this model is not adequately supported by the results, and may need to be reexamined or supported with more evidence.

0.1 Model equations

I think some of the equations may need another step of proofreading. Some of the notation is unclear, and there may be some errors in the equations. Specifically, Equations 10-17 may need another look.

Eq. 10 and 12: The integrals do not look correct. Integrating temperature over depth doesn't make much sense, unless it's intended to be an average temperature with depth. In that case, the integral should be divided by the depth that it's being integrated over. In addition, d_{wt} appears in these equations and is never defined. Is it meant to be z_{wt}? $Q_{10,a}$ and $Q_{10,o}$ are not explicitly defined. I assume that these are derived from the Q_{10} function in Eq. 11 using either $T_{s,a}$ or $T_{s,o}$ from Eq. 12, but this should be explicitly stated. Finally, I think the equations for $f_{T,o}$ and $T_{s,o}$ should be integrating from 0 (the soil surface) instead of 1 (which would be starting at 1 m depth).

As a general point, it's not easy to visualize the k values resulting from equations 13 and 14. These values are central to the resulting heterotrophic respiration, and a crucial part of the argument that different fen and bog parameterizations don't matter for the model's accuracy. I would suggest adding a figure that shows how k varies with water table depth, for both fen and bog parameterizations. I tried making my own figure (which I've attached), but I'm not sure it's totally accurate. Having such a figure in the paper would really help readers interpret the general behavior of the model with respect to water table, and would be really helpful for understanding why bog and fen parameterizations do or do not cause differences in simulated fluxes. When I plotted these equations, the anoxic decomposition rates had some very sharp transitions at water table depth of 0.3 m, which didn't seem very realistic.

As a final issue related to these equations: on page 10100, line 22: The model described here applies a factor of 0.025 to anaerobic decomposition, citing Frolking et al (2010). In Frolking et al (2010), the decomposition rate of anoxic carbon is in fact 0.001 (see Table 2 in that paper, where the parameter is described as "decomposition rate reduction factor at 'full persistent' anoxia.") The value of 0.025 used here actually appears in Frolking et al (2001), Table 1. Note that in that context, 0.025 is the value used for bogs, and a value of 0.1 is used for fens. In this manuscript, the bog value of 0.025 is used for both peatland types, and as well as being referenced to the incorrect paper. This seems like an important omission given the later argument that fens and bogs are not significantly different in this model.

0.2 Flux measurements used for evaluation

I think the origin of the eddy covariance fluxes used for evaluation should be described in more detail. In the manuscript, site parameters are listed in Table 4, but there is very little information about the origin of the fluxes. Were they downloaded from the Fluxnet database, or individually contributed by site PIs? Were they the result of standardized Fluxnet processing, or individual site processing procedures? What FLUXNET-defined level of data were used? What kind of filtering, gap filling, and quality control were done? Given that the observed fluxes in Figures 7-9 are quite noisy, and the clearly unrealistic QE for one site-year in Fig. 5, it's important to know whether these fluxes were screened for common sources of unrealistic values in eddy covariance (low turbulence, wind directions identified as unrepresentative, equipment problems). Some of the large outliers in Fig. 9 could be related to suboptimal atmospheric conditions, and it's important to know whether these measurements were screened for these types of known issues before being compared with the model.

ER and GPP are not strictly measured using eddy covariance, but are derived from NEP measurements using a range of partitioning techniques. A commonly used method is to fit nighttime NEP to a nonlinear function of temperature, and/or daytime NEP to a nonlinear function of PAR (see Stoy et al, 2006, Desai et al 2008, and Lasslop et al 2009). If this partitioning method was used, it makes comparisons with models problematic because the modeled values are being compared to another [data-constrained] model rather than to actual observations. It's really important to describe the partitioning method so readers can appropriately evaluate the results.

When calculating average fluxes, eddy covariance measurements are typically gap-filled, because varying atmospheric conditions, equipment issues, and quality control invariably produce gaps in data. What kind of gap-filling was applied to these eddy covariance measurements before they were compared with the model results? Gap filling is usually conducted in tandem with ER and GPP partitioning, and can introduce the same nonlinear models to the dataset (see Moffat et al 2007 for a comprehensive review). Was any gap filling applied to latent and sensible heat fluxes? If not, daily sums could be biased because data availability is generally lower at night than during the day.

There is no discussion of the inherent uncertainty in eddy covariance measurements, which is highly relevant when they are being used to evaluate a model. See Richardson et al (2006) for a starting point.

0.3 Conclusions regarding differences between bogs and fens

Figure 13 shows differences in r^2 and RMSE after changing model parameters related to the separate fen and bog parameterizations. From the K-SWAP test, it is clear that these separate parameterizations do not significantly change the model's fidelity to observations (measured using those two error metrics) over the time scales being investigated here. Based on this, the authors conclude that "it is not necessary to distinguish between fens and bogs." I think this conclusion is not supported by this analysis for a few reasons.

1. These results do say something about the specific parameterization being used here, but that is not enough to draw general conclusions about modeling fen and bog ecosystems. Based on the figure I attached, it's clear that, with this parameterization, decomposition rates below the water table are so low as to be essentially negligible. Any differences between fen and bog decomposition rates below the water table would therefore have very little influence on total fluxes. However, the previous manuscript that is the source of a key parameter f_{anoxic} in fact had a very large difference between bogs and fens originally (which is not reproduced in this manuscript). It's possible that a different parameter set could yield very large differences between simulations of bogs and fens. So, it's correct to say that this model with these parameters does not predict much difference between bogs and fens. But a different parameterization that produces equally good (or better) results compared to observations might be much more sensitive. I think it's premature to conclude that the difference between fens and bogs can be ignored entirely.

2. These simulations incorporated differences between bogs and fens that a global model would not have access to. Specifically, the plant functional types used to drive the model runs are different between fens and bogs, and could drive large differences in global model simulations depending on what vegetation is assumed to dominate different peatlands. Real fens and bogs have very different dominant plant communities, hydrology, and soil properties that can drive differing ecological behaviors (for example, see Sulman et al 2010). On the other hand, some studies have concluded that peatland type is not the primary driver (e.g. Humphreys et al 2006). A review of literature related to ecological differences between fens and bogs and how they might affect or not affect model simulations would really add to the discussion. The section of the discussion addressing this issue (Section 4.5) does not contain *any* citations to literature addressing observed contrasts or similarities between fen and bog ecology

and biogeochemistry, and this argument is begging for some more context.

3. The evaluation shown in Figure 13 is not really adequate to establish that there is no important difference between results using fen and bog parameterizations. The only data presented are RMSE and r^2, which only allow evaluation of the results with regard to a quite noisy observation-based dataset. It's quite likely that the parameter change introduces a small but significant persistent bias in heterotrophic respiration. This might not show up over short (several year) time scales, but could lead to large differences in peat carbon pools after decades or centuries of integration (which are the time scales of greatest interest for peatlands). Because eddy covariance data includes inherent uncertainty due to turbulence and micrometeorological variations, even an important difference in model predictions could be obscured by this minimum noise level in the analyses used here. It would be much more illuminating to see a comparison of modeled ER, or cumulative NEP, with the different parameter sets in order to evaluate how sensitive the model is to these differences. Even a comparison of time series between model and observations might reveal some persistent biases at seasonal or annual time scales that are too small to show up in the total RMSE and r^2 numbers.

0.4 Additional specific comments

10091, Line 10: A net C uptake of 3.3 GtC/year compared to the 5.0 GtC/year net C uptake seems awfully high. Are these estimates directly comparable?

10094, line 22-28: The discussion of peatland and non-peatland fractions and PFT fractional cover seems out of place, since the rest of the manuscript only discusses single-point simulations. Was this sub-grid-scale heterogeneity actually included in the simulations? If so, what basis was used to determine peatland fractions, and fractional PFT cover?

10096, line 15: It would be helpful to have the units for w_m here.

10098, Equation 7: θ_m does not seem to be defined anywhere. Is this the same as w_m?

10098, line 20: I think the 4.6 factor should have units of μmol m$^{-2}s^{-1}$ per W m^{-2}

10099, line 7-11: Were fractional PFT coverages included in these simulations? How were they parameterized based on the limited land-cover data from sites? Peatlands typically have open, patchy vegetation. Did the model incorporate this heterogeneity? Were multiple overlapping PFTs used for each site, or just one?

10101, line 1: What is the model time step?

Equations 13 and 14: What is the justification for the 0.3 m cutoff? It seems fairly arbitrary. The text says these equations are from Frolking et al (2001), but the table says the parameters are from the McGill Wetland Model.

10101, line 13: There is not equation for C_{hum}. Is it just a constant rate, or a fraction of decomposition?

10105, line 8-17: Were the parameter changes applied before or after spinup? If they were only applied to the spun-up model, than any significant changes that would have accumulated over 100 years would be ignored. These could be important in an earth system modeling context.

10106, line 11-13: Hummock-hollow topography is very typical of bogs. Did this affect any other study sites? I think it's worth discussing this issue in more depth, with respect to all of the sites and how these topographical variations could affect the model. See Dimitrov et al (2010), Baird et al (2009), Loisel and Yu (2013), etc for some good discussions of issues related to microtopography.

Section 4.2, 4.3: All of these evaluations used daily averages, correct? It might help to state this explicitly at the top of the section.

10108, line 23: NEP in the model is calculated by subtracting ER from GPP. In eddy covariance measurements, NEP is the measured quantity, while ER and GPP are derived from NEP (and therefore may contain additional errors).

10109, line 2: "Model errors for the extreme values at these two sites" implies that the eddy covariance values are "truth". Eddy covariance is an inherently noisy measurement because it relies on atmospheric turbulence. Furthermore, large spikes could be due to inadequate screening for poor meteorological conditions. I wouldn't place too much confidence that these big outliers in eddy covariance fluxes are actually real ecological fluxes. This is where it's important to check what kind of screening was done on the flux measurements.

10110, line 5-7: If this site were included in the Figures 7-9, readers could see what was going on much more easily.

10111: I think a bit more explanation of the Taylor diagrams would be helpful here. I don't think they're really common enough to forego a sentence or two about how to read them.

Section 4.5: I think these results would be stronger if readers could see a bit more than just r^2 and RMSE. Changes in modeled values between runs, or changes in mean bias, would be useful additions to this section.

Figure 1: There is no key for a lot of the notation in this figure. The soil layers are also a bit confusing. The model seems to calculate peat depth prognostically, but this diagram implies that there are fixed depths for fibric, hemic, and sapric layers. That doesn't appear to be the case in the actual model equations.

Figure 5: What is going on with the "observed" fluxes in UK-Amo QE in 2006? Those do not look like trustworthy measurements, and if they were included in the evaluation it casts doubt on whether the resulting statistics are meaningful. It's probably worth checking with the PI if there was some equipment problem in that year. Also, why are

only 4 of the 8 sites shown?

Figure 7-9: Why these six sites and not all 8?

Literature cited:

Frolking, S., Roulet, N. T., Moore, T. R., Richard, P. J. H., Lavoie, M., Muller, S. D. (2001). Modeling northern peatland decomposition and peat accumulation. Ecosystems, 4(5), 479–498

Moffat, A., Papale, D., Reichstein, M., Hollinger, D., Richardson, A. D., Barr, A. G., ... Desai, A. R. (2007). Comprehensive comparison of gap-filling techniques for eddy covariance net carbon fluxes. Agricultural and Forest Meteorology, 147(3-4), 209–232

Stoy, P., Katul, G., Siqueira, M., Juang, J., Novick, K., Uebelherr, J., Oren, R. (2006). An evaluation of models for partitioning eddy covariance-measured net ecosystem exchange into photosynthesis and respiration. Agricultural and Forest Meteorology, 141(1), 2–18

Richardson, A. D., Hollinger, D. Y., Burba, G. G., Davis, K. J., Flanagan, L. B., Katul, G. G., . . . Wofsy, S. C. (2006). A multi-site analysis of random error in tower-based measurements of carbon and energy fluxes. Agricultural and Forest Meteorology, 136(1-2), 1–18

Desai, A. R., Richardson, A. D., Moffat, A., Kattge, J., Hollinger, D. Y., Barr, A., . . . Stauch, V. J. (2008). Cross-site evaluation of eddy covariance GPP and RE decomposition techniques. Agricultural and Forest Meteorology, 148, 821–838.

Lasslop, G., Reichstein, M., Papale, D., Richardson, A. D., Arneth, A., Barr, A. G., . . . Wohlfahrt, G. (2009). Separation of net ecosystem exchange into assimilation and respiration using a light response curve approach: critical issues and global evaluation. Global Change Biology, 1–22.

Sulman, B. N., Desai, A. R., Saliendra, N. Z., Lafleur, P. M., Flanagan, L. B., Sonnentag,

O., . . . van der Kamp, G. (2010). CO2 fluxes at northern fens and bogs have opposite responses to inter-annual fluctuations in water table. Geophysical Research Letters, 37(19), L19702.

Humphreys, E. R., Lafleur, P. M., Flanagan, L. B., Hedstrom, N., Syed, K. H., Glenn, A. J., Granger, R. (2006). Summer carbon dioxide and water vapor fluxes across a range of northern peatlands. Journal of Geophysical Research, 111(G04011).

Dimitrov, D. D., Grant, R. F., Lafleur, P. M., Humphreys, E. R. (2010). Modeling the effects of hydrology on ecosystem respiration at Mer Bleue bog. Journal of Geophysical Research, 115(G4), G04043.

Baird, A. J., Belyea, L. R., Morris, P. J. (2009). Upscaling of peatland-atmosphere fluxes of methane: small-scale heterogeneity in process rates and the pitfalls of "bucket-and-slab" models. In A. J. Baird, L. R. Belyea, X. Comas, A. S. Reeve, L. D. Slater (Eds.), Carbon Cycling in Northern Peatlands (Vol. 184, pp. 37–53). Washington, DC: American Geophysical Union.

Loisel, J., Yu, Z. (2013). Surface vegetation patterning controls carbon accumulation in peatlands. Geophysical Research Letters, 40(20), 5508–5513

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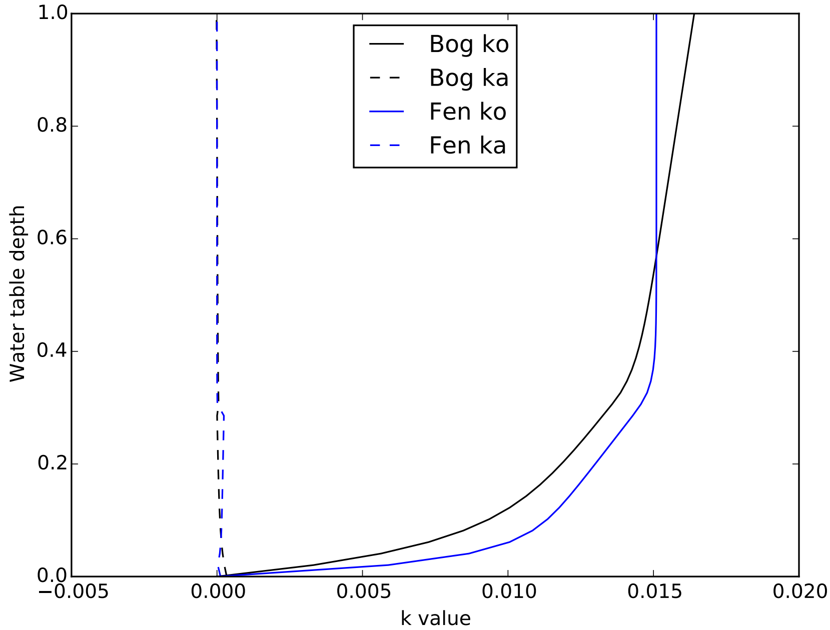

Fig. 1. Anoxic and oxic k values for bog and fen parameter sets, based on equations 13 and 14