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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.

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In this paper the authors combine a newer version of CLASS with a newly developed peatland carbon model based on the structure CTEM. This paper is an interesting and useful addition to the literature but I believe the authors need to do more to substantiate the models usefulness. Their work represents part of a movement by some global modelling groups to incorporate peatlands into the global models. The reason this is important is because peatlands represent the highest carbon density ecosystems in the world so while they cover a relatively small fraction of the land (4 to 6%) they contain up to 25% of the world's terrestrial biogenic carbon. Simulating the sensitivity

of their carbon stores to climate and land-use change is important for future projections of global carbon cycling. Since the carbon dynamics of peatlands is so tightly coupled to surface hydrology it is reasonable to ask if climate change will have a significant impact on their carbon function in the future. The only way to address this questions is through modelling and that modelling requires a reasonable representation of peatland ecosystems at an appropriate level for incorporation in global ecosystem models. The authors present the details on the development of such a model and then provide an evaluation of their model output for six different peatlands: three bogs and three fens.

I think the authors have done a reasonable job but I do have some constructive criticisms of their manuscripts as it current stands. My main concern is the authors provide little in explanation for why their models produces the results it did. The evaluation of the model against measurements is useful but it is very limited. They compare model out against observations but do not go into detail of why the model is successful in some cases and not very successful in others. They presentation of their results is very limited and not overly useful at time. Better visual presentation of their results (see suggestions below) and the inclusion of some sensitivity analyses would help demonstrate to the community the utility of the model they have developed. Several things stand out as being quite unusual that I think the modelling community, and certainly the peatland carbon modelling community will find unusual – for example the apparent lack of the influence of initial conditions and the apparent lack of sensitivity to peatland wetness on the carbon exchanges. These are quite at odds with the empirical observations from numerous long-term measurement sites (some of these sites are included in this manuscript). The simulation of wtd seems quite poor but it does not seem to really matter in the end? The authors should do some more analysis to better assess where the uncertainties in the simulated results are coming from (see suggestions below). All models are far from perfect but hopefully they are useful. To determine the usefulness readers have to understand why the model does what it does and whey it does not do what it was expected to do.

Page 1 Ln 36 This depends on the peatland type. Mineral peatlands with Ca concentration much above 2 mg/l there becomes less bryophytes and more sedges. Roughly this is the difference between bogs and fens.

Page 2 Ln 7-10 Yes but Wu and Roulet showed bogs are quite resilient and fens are not. Christensen et al and Ise et al. worked on poor fens. The conclusion from the literature you cite is that ombrogenic peatlands (bogs) may have sufficient resilience to maintain their sink function in climate change but fens, which rely on additional external inputs of water, may not. This is the crux of the problem in simulating the sensitivity of peatlands to climate change. We break forests into different functional types. Similarly peatlands should not be seen as one type of ecosystem. My guess is that that C store in peatlands is roughly 50 - 60% in bogs and 40 - 50% fens.

Page 2 Ln 17 I believe was should be were

Page 2 Ln 28-34 Unlike the other three models discussed in the sentence HPM it us not process based but it a phenomenological model. It is a one year time scale model and is not of the same temporal scale as the process based models.

Page 6 Ln 23 It may be a more theoretically sound representation but "better" needs to be justified by evidence.

Page 9 Ln 4- 16 What did you do for the spin up of the peat profiles of each peatland? You discuss the sensitivity of two of the initial parameters derived from the spin up at the end of the results but this is an important issue to discuss up front. Later on you show that it might not matter that the decomposition rates with depth are generalized and the presence or absence of moss is not that important (not sure I understand this) but the hydraulic properties of the profile are important to the simulation of the water table (wtd). Later on you show that you have marginal success in simulated the wtds but it might not matter that much for GEP, ER, or NEP. However, if this model will be extended to do methane this will be critical. At the very least the authors should be explicit here on the decomposition coefficients or base respiration they assign to each

layer to capture the drop in intrinsic OM quality. They also should show explicitly the hydraulic parameters for each layer so the readers can determine if the characteristic differences between fen peat and big peat are in the model parameter set or are not in the model parameter set. The authors do not address this issues until the end of the paper and it should be clear from the beginning.

Page 9 Ln 15-16 Does this assumes the relationship between C and density is the same across all peatlands. Is this true? The density - depth relationship and depth - age relationship can be quite different among individual peatlands and quite different between bogs and fens. The model appears to have an ombrotrophic bog set up? Is this used for all the simulations?

Page 10 Ln 1 – 14 You use Taylor plots in your evaluation but do not mention this here?

Page 10 Ln 16 A general comment on the presentation of the times series in the result sections. Time series are useful but they are difficult to sue to isolate if the uncertainty in the model result is random (ok assuming they cancel) versus systematic (which may or may not be OK). Scatter plots of simulated versus observed around a 1:1 line would reveal if there are some systematic errors - for wtd and LE there appears to be some systematic errors in the growing season. For NEE, GEP and respiration, it appears that for some peatlands the top and bottom 10% are systematically missed and in other sites both the GEP and ER are grossly under-estimated but because NEP is the difference in these two numbers the NEP does not look that bad. Using these plots does not necessarily negate the utility of the model but it helps the reader assess if the model is suitable or not for the task it is being developed for – the assessment of peatland carbon dynamics in changing climate conditions. For example St. Hilaire et al. (2010) showed that MWM truncated high GEPs and ERs but these represented less than 10% of the total exchanges so it did not matter for the overall annual exchange. Where this truncation may have implications for climate simulations is if there is a systematic shift into those conditions that are more favourable to greater GEP and/or ER. St, Hilaire et al explicitly show this so the reader is fully aware of the issues. In these models it is the accumulation of small systematic uncertainties that can give erroneous results over the long-term.

Page 10 Ln 18 On Figure 3 the value of 0.25 cm should be 0.25 m. This result is interesting. The authors are not aware but there are two papers (one in press and one in review at Ecohydrology) that show that the wtd changes over time across the hummock - hollow at MB move in unison. This means if you know a wtd at a single point and you know where that point is with respects to the difference in hummock – hollow elevations then you can estimate of the wtds across MB. So the authors' explanation is plausible though I am surprised the model offset is the same height of the height of the hummocks above the hollows. This raises the question why the same problem did not arise for Fajemyran. It has micro-topography, maybe even greater, than MB?

Page 10 Ln 30 – 35 The Wtds for the fens is poor in all three sites. They are generally between 0.1 to 0.3 m off. Part of this maybe the parameter set up is for bogs not fens. In terms of carbon function this has a much larger implication for fens than bogs - see Wu and Roulet 2014. Did you try any of the fen simulations by adding in extra water emulate additional water through groundwater seepage? We know fens receive some addition water from either surface inflows and/or groundwater seepage. What you do not know is how much extra water. My gut feeling, from visiting a few of these sites, is the extra water is quite small for AB, maybe about 5 to 10% for Degero, and probably more for Lom. You compare in Fig. 5 measured and simulated Et and it looks like Et is overestimated at most sites, hence I assume this is the reason for the problems with the wtds? It's important to a better handle on where the problems are with the wtd estimates because wtd is a critical variable to the NECB. It's even more important if you intend to eventually use this model to get at methane.

Page 11 Ln 4-6 As suggested in the general comment above scatter plots will reveal if there are consistent biases in the simulated turbulent fluxes.

Page 11 Ln 29 Again scatters plots of GEP and ER around a 1:1 line and this will illus-

trate the biases in the model relative to the size of the flux. It also illustrates the range of GEP and ER the model does well and where it does not do well. This is important because NEP is the difference of two much bigger numbers and it may do NEP reasonable well but for the wrong reasons. An alternative way to illustrate the uncertainty is to analysis the residuals of the regressions between observed and simulated and see if there are patterns. If the errors are random there should not be any pattern to the residuals but if the residuals show a patterns this suggests structural issues with the model.

Page 11 Ln 33-36 It is interesting that the model does well on Kaa given that an appa mire - i.e. it contains a lot of open water in the form of pools in the measurement footprint . Pools tend to be large sources of CO_2 with no mechanism for the uptake of CO_2 (see work by Hamilton et al. 1994 and recent work by Pelletier et al (2014). This suggests that the model gets the 'right' answer without accounting for the spatial variability. This is a little disconcerting.

Aurela, M., T. Laurila and J. Tuovinen (2002). "Annual CO_2 balance of a subarctic fen in northern Europe: Importance of the wintertime efflux." Journal of Geophysical Research - Atmospheres 107(D21): 4607, doi:4610.1029/2002JD002055.

Aurela, M., J.-P. Tuovinen and T. Laurila (1998). "Carbon dioxide exchange in a subarctic peatland ecosystem in northern Europe measured by eddy covariance technique." Journal of Geophysical Research 103(D10): 11289-11301.

Hamilton, J. D., C. A. Kelly, J. W. M. Rudd, R. H. Hesslein and N. T. Roulet (1994). "Flux to the atmosphere of CH_4 and CO_2 from wetland ponds on the Hudson Bay lowlands (HBLs)." J. Geophys. Res. 99(D1): 1495-1510.

Pelletier, L., I. B. Strachan, M. Garneau and N. T. Roulet (2014). "Carbon release from boreal peatland open water pools: Implication for the contemporary C exchange." Journal of Geophysical Research G: Biogeosciences 119(3): 207-222.

Pelletier, L., I. B. Strachan, N. T. Roulet and M. Garneau (2015). "Can boreal peatlands with pools be net sinks for CO2?" Environmental Research Letters 10(3).

Pelletier, L., I. B. Strachan, N. T. Roulet, M. Garneau and K. Wischnewski (2015). "Effect of open water pools on ecosystem scale surface-atmosphere carbon dioxide exchange in a boreal peatland." Biogeochemistry 124(1-3): 291-304.

Page 11 35-38 I also find this result reason for concern. In one case, RU, there is a huge mass of old carbon that sustains a larger than simulated ER and in the other case, UK, there is a relatively tiny mass of C that produces the same over-estimate. This does not really make sense to me unless the respiration below 1 m depth is insignificant. In both cases the errors in ER are offset by a grossly overestimated GEP. I can understand why there is little difference in GEP if the conditions are general the same at the peat surface. This section needs some more thinking – how much of the ER comes from autotrophic respiration? How associated in AR to GEP – if one is over estimated (GEP) does this push AR up? If HR is a very small component of total ER then is does not matter that there is a small or large mass of peat. Throughout the paper the authors tease the readers with interesting results that are often confounding but then provide little explanation of why the results come about. You have no idea why the observations are what they are, but you are simulating the carbon dynamics in your model so you can tell the reader where the ER is coming from, what makes it up, and why GEP is large enough to offset it. It is in these explanation that you come up with from how the different components of the model interact that will convince a reader your model is reasonable or not. The same is true for the results in the energy balance and wtds. You report your results but tell us from playing with the model why you get the results you do.

Page 12 Ln 9 See comment above (Page 11 Ln 33-36) on the presences of pools.

Page 12 Ln 20-21 What does it mean when the simulations averaged over a month look much better than the short-term comparisons? It means that errors cancel out,

which may be fine or may not be. If the reason for the lower agreement at the higher time resolution is one of timing and over some averaging period of several days the problems go away then it's fine. But if the problems are at certain periods of time and these periods maybe more frequent in climate change scenarios you wish to use the model to simulate then this could be a problem. Given the apparent variance in agreement across the various peatlands the authors would gain a better understanding of the models behaviour by doing some sensitivity analysis on the initial conditions and key parameters. This will also tell the authors if they are compounding errors with poor wtds influencing the C dynamics. Sensitivity analysis on a model like the one the authors present takes a lot of work and time but it reveals a lot of good information that readers want to know. I do not like referring authors to my own work but I think the papers of Wu and Roulet (2012) and others on the development and evaluation of MWM illustrate the value that the scatter plots serve and what a through sensitivity analysis can show. The sensitivity analysis in Wu and Roulet (2012) took a good month to run but it demonstrates what might happen if temperature and wtd change over time. Wu et al. (2013) and St. Hilaire et al. (2010) are also papers where this detailed sensitivity analysis revealed some explanations for the behaviour of the model.

St-Hilaire, F., J. Wu, N. T. Roulet, S. Frolking, P. M. Lafleur, E. R. Humphreys and V. Arora (2010). "McGill wetland model: evaluation of a peatland carbon simulator developed for global assessments." Biogeosciences 7: 3517-3530.

Wu, J., N. T. Roulet, M. Nilsson, P. Lafleur and E. Humphreys (2012). "Simulating the carbon cycling of northern peatlands using a land surface scheme coupled to a wetland carbon model (CLASS3W-MWM)." Atmosphere - Ocean 50(4): 487-506.

Wu, J., N. T. Roulet, J. Sagerfors and M. B. Nilsson (2013). "Simulation of six years of carbon fluxes for a sedge-dominated oligotrophic minerogenic peatland in Northern Sweden using the McGill Wetland Model (MWM)." Journal of Geophysical Research: Biogeosciences 118(2): 795-807.

Page 12 Ln 32 I do not believe these are not annual C budgets but the annual cumulative net ecosystem production. It is very important you get this terminology correct (see Chapin et al. 2006) to avoid confusion down the road. You do not simulate DOC export or methane exchange and in peatlands these are very important components of the annual C budgets. The net ecosystem carbon budgets from MB (Roulet et al. 2007) and Degero Stor (Nilsson et al. 2008) show that these two exports can offset the annual NEP by 20 to 40%.

Chapin III., F., G. Woodwell, J. Randerson, E. Rastetter, L. GM, B. DDi, D. Clark, M. Harmon, D. Schimel, R. Valentini, C. Wirth, J. Aber, J. Cole, M. Goulden, J. Harden, M. Heimann, R. Howarth, P. Matson, A. McGuire, M. JM, H. Mooney, J. Neff, R. Houghton, M. Pace, M. Ryan, S. Running, O. Sala, W. Schlesinger and E.-D. Schulze (2006). "Reconciling Carbon-cycle Concepts, Terminology, and Methods." Ecosystems 9: 1041–1050.

Page 13 Ln 31 What does it mean when the GEP, ER, and NEP cluster and appear to follow the observations much better than the energy balance terms? Does this mean the C fluxes in peatlands are constrained to the point that they are relatively insensitive to changes in environmental conditions? This is way the sensitivity analysis is so important. It impossible to know why the results are what they are without this further analysis. Mimicking three to five years of measurements is important but having the model reproduce changes in response to changes in the environmental conditions is also important for the intended use of the model.

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