

Interactive comment on “Coupled eco-hydrology and biogeochemistry algorithms enable simulation of water table depth effects on boreal peatland net CO₂ exchange” by Mohammad Mezbahuddin et al.

Anonymous Referee #1

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This paper compares the results of the ecosystem biophysical model ecosys against field measurements of environmental variables (primarily water table depth – WTD) and carbon fluxes measured by eddy covariance (EC) at a treed peatland in western Canada over a 5 year time frame when WT was decreasing at the site. The model, ecosys, is a very sophisticated tool and has been widely applied in the past against many different ecosystem types, with success. It is fair to say that is among the top ranked platforms for simulating ecosystem functioning. With that said, the purpose of this particular paper is a bit foggy. The EC flux measurements from this site, including the time series over which the WTD had declined, have been clearly reported in

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previous literature, as has been cited in this study. Therefore, is the purpose of this study 1) to simply to test if ecosys can simulate the trend in measured EC fluxes over the study period, or 2) to use ecosys to explain the behaviour of the EC-fluxes, which cannot be obtained from most common EC and environmental measurements? The paper seems to do a bit of both, but the main objective is not clear. However, given the extensive testing of ecosys at other peatlands and other ecosystems, the former seems to be quite a weak objective. The latter is more scientifically interesting, but that is not the way the paper is set out. There are 4 operating hypotheses (not repeated here). These are not stated in terms of what is reflected in the EC-derived measurements, which seems to be the main thrust of the paper from the rest of the introduction, but rather in terms of biophysical processes that will take place in the model. Hence, it is a source of some of the confusion about the purpose of this paper. It would be nice to see an attempt to improve the introduction with a clearer purpose. Another concern about the present manuscript is that there is a lot of attention to how ecosys performs in simulating the WTD. It seems to me that this topic was adequately covered in the previous paper, Mezbahuddin et al. (2016), so why do we need the emphasis here. I have a small worry about the comparison of ecosys modelled fluxes against gap-filled data (especially nighttime (Re) fluxes – section 3.3). Since the gap-filling is a model itself – now we are comparing one model against another. I realize there is a discussion of how this may have affected the comparison, but that does little to convince readers that the comparison of modelled and measured data is sound. Why not just compare half hours where measured data were available to test ecosys, if that is the point of the paper (see above). Section 4.2 Divergence between modeled and EC-derived fluxes makes some interesting points, but as it stands very little of this has been tested or analyzed in any detail, so we really don't know what the source of the discrepancy is. It would be nice to see some attempt or suggestions as to how to hone in on the most likely causes of the discrepancy. You seem to suggest the E-derived measurements are wrong, which may well be, but I am not sure that the model is not without fault. The Conclusion section is not really a conclusion. First, much of it is simply a re-statement

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of the main findings. Second, it suffers the same problems as the objective of the study namely, not really being clear. The final statements about the value and application of the ecosys model, while possibly true, seem a little self-serving.

Finally, the manuscript needs a good editing, although the writing is such that it is understandable, there are many awkward statements/phrases, issues with tense, or grammatical errors that could be addressed to improve the manuscripts readability. I have pointed out some of these in the minor points below, but there are several others.

Overall, this could be quite a useful contribution, especially if cast in the role of using ecosys to help understand the pattern and responses of EC-derived fluxes over time, something that is hard to get from just EC and environmental measurements, rather than just another test of the ecosys algorithms at another peatland site.

Minor Issues: 1. Line 81-82 & line 87, Lafleur et al. (2005) reference is inappropriate here, they discuss Re not GPP. Also relevant on lines 908-911. 2. Lines 95-97, as above this reference does not discuss a threshold for WTD and GPP, perhaps another reference by this author? 3. Lines 98-107, the start of this paragraph is poorly worded. First one does not start a new paragraph with the word 'therefore'. The sentence beginning "So, to adequately predict . . ." is awkward and doesn't quite read right. As with the next sentence – the phrase "do not have prognostic WTD dynamics that prevent simulation" is confusing and awkward. 4. Lines 314-317, these two sentences that describe the simulated mosses are very difficult to understand, some revision for clarity is needed. Should be put in terms of what a real moss is and where it grows. 5. Line 447, the word diurnal here is incorrect; Table 1 compares instantaneous half hour fluxes. Diurnal suggests some course of measurements over the daytime. 6. Line 457-58, the sentence here about 2009 is not needed here; simply add it as a foot note to the Table. 7. Line 478-81, this sentence is somewhat heuristic; the Figure certainly does not show these components. I think it is adequate just to say the model simulated measured WTD well. 8. Line 536, word 'also' is not needed 9. Line 537, what does 'It' refer to? 10. Line 538-41, this long sentence is somewhat awkward and doesn't really

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say anything new. 11. Lines 559-61, the sentence here seems to be missing a word or words, does not read well. 12. Lines 55-570, you seem to miss an opportunity here. You describe how warming does not stimulate Re when water table was high in 2005, and how it is stimulated by warming in low WT years (2006 and 2008), yet given the sophistication in ecosys there is not real explanation of why this works the way it does, what is the biogeochemical functioning that does or does not stimulate Re under low and high WTs respectively? Further down you describe the mechanisms associated with GPP, why not the same with Re? 13. Line 632-33, this was mentioned above (#6), no need to repeat it here. 14. Lines 664-69 Section 3.5, You state this drainage experiment "... would 667 also serve as a climate change analog in providing us insight into how potential WTD drawdown 668 under future drier and warmer climates would affect boreal peatland GPP, Re and hence NEP." I don't see how, as the atmospheric changes of higher temperatures and perhaps higher VPD are not included. I think it is fair to say this simulation represents the effects of WT drawdown only. 15. Lines 673-74, I don't think you need this sentence, it is rather obvious that changes would occur and you have a lot of words following to describe them. 16. Lines 683-696, is this enhanced evapotranspiration coming from the tree cover or ground vegetation or both? 17. Lines 848-51, should note here that his Dimitrov et al. 2010 study was on a temperate bog not a fen.

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