Geoscientific

Model Development

Discussions

[GMDD]

Interactive

comment

Interactive comment on "ORCHIDEE-PEAT (revision 4596), a model for northern peatland CO$_2$, water and energy fluxes on daily to annual scales" *by* Chunjing Qiu et al.

Anonymous Referee #1

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This manuscript describes a new peatland model implemented in the ORCHIDEE land model. The model was evaluated by comparing modeled water table, LE, GPP, and NEE to measured eddy covariance fluxes from several peatland field sites. The paper is generally well written and the key processes of the model are clearly described. The introduction section includes a useful review of recent peatland models that does a good job of setting the stage for this model. The paper generally does a good job of identifying uncertainties and potential weaknesses in the model that could be addressed in future work, although I think there is some room for improvement in describing some of these issues in more depth.

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[Discussion paper]

I think there are a couple of general areas in which the manuscript could be improved:

1. The key peatland-specific changes to the model are focused on peat carbon pools and hydrology, including a new architecture for simulating peat decomposition using acrotelm and catotelm layers. The modifications to plant processes are less dramatic. In my understanding the model uses an existing C3 grass plant functional type and does not introduce any new peatland-specific vegetation processes. Given the focus of model process changes on decomposition rather than plant processes, it seems strange that the evaluation is so focused on GPP. Why not show and evaluate modeled ecosystem respiration instead of or in addition to GPP? Analyzing respiration fluxes would allow a much better evaluation of the key new model features that are specific to peatland processes. Without an evaluation specific to these new processes, it feels like there is a big piece missing.

2. The approach to optimizing Vcmax is problematic. The optimized site-specific values are compared to a default value that is well outside the range of values that seem to be appropriate for these sites (within the model at least). Figure S3 demonstrates this very clearly for GPP and NEE: the model using the default Vcmax is not even close to reproducing the observed magnitude of photosynthesis at these sites. As a result, the comparison between optimized and default Vcmax simulations is not very informative. It would be more useful if that comparison used the mean or median of the optimized Vcmax values (which is actually used for a different analysis later in the paper). In that case, it would be possible to evaluate whether site-to-site variations in Vcmax were necessary for improving model fidelity. It's not very informative to show that optimized Vcmax is better than a Vcmax that is much too low for every site.

The fact that the default Vcmax based on observations does not work within the model raises further questions. The paper addresses this very briefly (lines 249-251) but I think a more detailed discussion of why the model Vcmax needs to be so much higher than observations would be useful. Were the other photosynthesis-related parameters (LAI, light absorption, etc) in the model consistent with site measurements?

Site-specific optimization of Vcmax could mask other issues with the model, for example underestimates of plant biomass or LAI. I think it would be really helpful to show how modeled LAI compares to measurements, especially among different sites, and whether errors in modeled LAI can explain the latitude/temperature relationship in optimized site Vcmax.

Specific comments:

Lines 173-176: I'm not sure it's that novel that this model is built into a land surface scheme that conserved water, carbon, and energy. Doesn't the LPJ-GUESS model described above have a similar purpose? In any case, if there is not already a peatland submodel built into ORCHIDEE then I wouldn't be that concerned about justifying the purpose of this effort. I think it's clearly valuable to build and evaluate a working peatland submodel within ORCHIDEE.

Line 232: Not all peatlands are grassy. Does this assumption cause issues when applying the model to shrubby or forested peatlands (such as the Old Black Spruce site mentioned a few lines after this)? Were all the peatland sites used for evaluation grassy peatlands?

Line 249-251: It's great that the paper brings up this issue of compensating errors, but it would be better if there were some evaluation of whether the model has systematic errors in LAI, etc.

Line 257: "drainage flux reduced to zero": So there is no water flow out of the peatland unless it is flooded? This seems inconsistent with a lot of real peatland systems.

Line 299-301 and Fig. S1: The difference between the soil carbon dynamics and the peat carbon dynamics is confusing. Do the peat pools contain the Active/Slow/Passive soil carbon pools, or do they replace them? Fig. S1 suggests that all of these pools are present in the peatland (metabolic litter, structural litter, acrotelm, catotelm, active, slow, passive) but this doesn't seem consistent with the description in the text. If the

peat layers are actually replacing the active/slow/passive pools, then Fig. S1 and the text should make that clearer.

Line 308-310: Did this use the observed or simulated water table? How would this be handled in larger-scale or global simulations?

Line 315-316: It would help to show the equation for beta instead of just describing it. Equations for acrotelm height and catotelm depth should also be included.

Is the depth of catotelm and total peat depth calculated? What does the model do if water table goes below the bottom of the peat layer? Can it represent a situation with no catotelm layer? Is there mineral soil beneath the bottom of the peat layers?

Line 331-332: k_A and k_C are defined as fixed parameters, but line 319 says that they have a temperature dependence that is not shown in equations 5-8. These equations should show the complete calculation, including temperature dependence etc.

Line 493-496: If this were the correct explanation, I would expect WT to be more accurately simulated in fens than in bogs. Was that the case?

Line 496-499: This seems like a very likely explanation to me, and something that could be tested by using a range of source-area/peatland-area ratios. Watershed analyses for the sites in question could provide some suggestions of realistic ratios.

Line 515-516: This really highlights how the main peatland-related processes in the model are related to decomposition and respiration, not plant growth. Since that's the case, why is the evaluation so focused on photosynthesis? I think analysis of respiration fluxes would be much more informative, particularly in this case where WT would be expected to have an effect.

Line 531: Water use efficiency doesn't really fit with these other variables. It's a biological parameter, not a climate forcing variable like the other ones.

Line 536-537: It's surprising that there is no difference in Vcmax between fens and

bogs, since those have very different vegetation types and productivities.

Line 540-541: This really seems like it could be compensating for some other error related to vegetation biomass, LAI, or productivity. I would expect higher biomass and LAI in warmer areas, which would drive exactly this type of relationship. I think this should be investigated since the optimization of Vcmax could be masking other important model issues.

Line 549: Why not use this mean value of 40 in the previous comparison, instead of the default value of 16?

Line 560-561: Why are only these two sites discussed and shown in the figure? Was the relevant data not available for other sites, or are these just being used as illustrative examples?

Line 564-566: The suggestion that the issues are due to errors in snow density implies that the snow mass was correct in the model. Is that true?

Line 582-585: Even if optimized Vcmax is an average for the ecosystem rather than a species-specific value, it should be comparable with the observed range among different species that exist in these systems. Other peatland models should definitely be comparable, because any peatland model would be representing an average plant type. I don't think this is a satisfying explanation for not comparing the optimized estimates with measurements. It's just as likely that the model underestimates LAI and needed to tune Vcmax higher to compensate. I don't find any of the three explanation below particularly convincing, and I think bias in LAI or plant biomass is a likely explanation that should be tested.

Line 591-592: Does ORCHIDEE not already take the influence of temperature on photosynthesis into account?

Line 593: If the issue were nutrient availability, I would expect strong contrasts in Vcmax between fen and bog ecosystems, which did not appear to be the case in this study.

Line 603-632: This is a nice review of observed drought effects on peatlands, but the paper doesn't demonstrate whether the model can reproduce any of these effects. Such a demonstration would be very informative.

Line 630-632: It would be better to show that the model reproduces this pattern (in a figure) rather than just asserting that it can.

Line 634-635: If GPP was captured well but NEE was not, then the difference must be due to simulated respiration. This is another case where more analysis of simulated respiration would be very helpful.

Line 666: This implies that water table is not an important feature of carbon cycling according to this model. This seems very inconsistent with the observational literature showing that peatland CO2 fluxes are quite responsive to water table fluctuations (much of which is cited in this manuscript). Some papers have demonstrated that compensating responses of GPP and respiration (e.g. both increasing under a drying trend) can cause NEE to be insensitive to water table fluctuations (e.g. Sulman et al. 2010), but the paper doesn't really demonstrate that the model is reproducing those compensating responses. Given the centrality of water table and hydrology in our understanding of peatland carbon cycling, I think this conclusion that water table isn't actually that important needs to be investigated in more detail, especially in how it affects peat decomposition and ecosystem respiration in the model.

Line 670-671: The paper definitely did not establish that nitrogen availability was the explanation for the latitudinal dependence. It was one of several proposed explanations. In fact, I think it's unlikely to be the explanation because it did not vary consistently with fen/bog type, which is closely related to nitrogen availability.

Table 2: In addition to bog/fen type, it would be informative to include something about the dominant vegetation type (grass, shrub, forested) and maybe aboveground biomass or LAI if available

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