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Geoscientific Model Development Discussions

Interactive comment on "Modelling northern peatlands area and carbon dynamics since the Holocene with the ORCHIDEE-PEAT land surface model (SVN r5488)" *by* Chunjing Qiu et al.

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This paper presents and evaluates a global model that simulates the spatial extent of peatlands and their C balance as a function of the environment. The peatland model is implemented as a module within the comprehensive land surface model ORCHIDEE. This is an important addition to this model as it allows to account for the effect of peatlands on the global carbon cycle, which is particularly important for long-term simulations, covering multiple centuries to the millennial time scale. The approach for simulating the spatial dynamics of peatlands across the globe is largely adopted from Stocker et al. (2014) GMD [thereafter referred to as ST14]. I don't want to hide the

fact that this is my own work and that I am pleased to see that it has stimulated other researchers to follow the same approaoch.

The paper by Qiu et al. goes a step beyond ST14 in that it evaluates the model not only by its accurateness in simulating the spatial patterns across the globe and the total northern peatland C storage, but it evaluates peat depth using information from a set of 102 peat cores, distributed across the northern hemisphere (mostly in the boreal zone), and deals with the challenge of accurately simulating the history of peat C accumulation throughout the Holocene, which adds substantial complexity. This work is also a substantial advancement in simulating wetlands and the distribution of flooding. Their comparison to a new observation-based dataset by Tootchi et al. (2018) shows a very good agreement (Fig. S7 - worth including this in the main text?), and seems to suggest that their model works much better in this respect than, e.g., the model presented in ST14. This in itself is a very useful innovation. I was also intrigued by the clever approach to simulate vertical growth of peat as an effective downward transport of soil C (down along the soil profile, across the 32 layers resolved by the model). This is a very useful innovation beyond the models resolved by ST14 and Kleinen et al., 2012. I think this work can be a very valuable addition to the literature and that the model presented here will be a useful addition to the very small set of comparable models available today (only two models, as I am aware). However, before getting there, I would like to see a few critical (MAJOR) issues addressed. I also think that the paper could gain from a clearer presentation in general. Below, I'm listing specific points. I hope the authors find my suggestions useful and I am looking forward to a revised version of the manuscript, and possibly a revision of the model and evaluation.

MAJOR

* The code is not accessible under the given URL. Although it's not offcicially required by GMD, I personally try to resist to accept model description papers without having open access code. I also think that the model should be easily reproducible in a simplified setup (without having to run the entire ORCHIDEE) and instructions should be

available to do so. Plug and play! Please make an effort to achieve this, it is greatly appreciated by the community and helps science to move forward (and it pays off for you).

* What the paper/model does not tackle/resolve, goes unmentioned. No tropical peatlands are simulated (?) nor evaluated. Are methane emissions from peatlands not resolved by the model? How does peat vs. mineral soil affect the extent of frozen soils (permefrost!)? The evaluation of inundation, particularly its timing is missing (or hidden in the SI).

* The simulated distribution of the peatland area fraction (Fig. 4) shows that the model is able to broadly capture the observed pattern, except that is quite strongly underestimates the peatland extent in the Hudson Bay Lowland (HBL). This reminds me of my own work, where the first version of my model (DYPTOP, ST14) also failed to simulate very high peatland area fractions (over 90%) across this large region. The HBL is, next to the West Siberian Lowland, the largest peatland region and therefore warrants special attention. The failure of the model by Qiu et al. in simulating large peatland fractions may be related to what one may call the "sponge-feedback" - the high efficiency of organic soils in retaining water (small runoff) which in turn increases persistency of flooding and the suitability for peat to accumulate - a positive feedback. I solved this by having (gridcell average) soil parameters that determine the soil hydrology depending on the internally simulated peatland area fraction, rather than using externally prescribed parameters from soil maps. I see that in the present model, some soil parameters are indeed prescribed for each gridcell separately from external data (soil bulk density, soil C fraction; l. 499). I would say that they should be affected by whether the model simulates peatland in the respective gridcell or not. This might be something worth looking into in order to better reproduce the observed Hudson Bay Lowland peatland area fractions. On l.131, it's mentioned that soil thermal (and hydrological?) properties are a weighted average of mineral and organic soils, where organic soil fraction is prescribed from an external dataset (NCSCD and HWSD).

* The explicit depth-dependence of the turnover rates is a bit obscure to me. While the rationale is defensible (l. 160 " priming effects, sorption of organic molecules to mineral surfaces"), it's not clear how important this factor is for the simulations here. Couldn't it be avoided? What's the e-folding scale in Eq. 2? (I see that the z_0 parameter is given later in the manuscript) And shouldn't this be accounted for by oxygen conditions, being subject to water content in different layers where the bottom layers will tend to be water-logged and thus have a very low turnover rate. From text S3, this is not evident.

* Comparison with cores. I am not sure if the model presented here can be compared to peat cores. The reason is that, in order to conserve C mass, an expansion of the peatland area fraction has to imply a reduction of the peat C mass per unit area - peat C is effectively diluted over an increasing area. Hence, the vertical growth of peat should slow upon lateral expansion. This is implied by the simplification that the model doesn't explicitly simulate the horizontal dimension. In reality, a peatland has substantial lateral structure and tends to be deep and have the oldest layers towards the center. That's also where peat cores are commonly taken (in order to maximise the temporal coverage). I am therefore not surprised to see that the model appears to generally underestimate peat depth. I suspect that separate simulations are required for this, where the peatland area fraction is held constant (no dilution!).

* The authors aim to model peat C dynamics during the Holocene (see title), but relatively little focus is given to forcing and evaluating the model with respect to this palaeo perspective. As far I understood, the model is forced with constant pre-industrial climate (although insolation and summer temperatures varied substantially during the Holocene, especially at high latitudes). Was a changing sea level accounted for? For applications in palaeo climate and -carbon cycle studies, the model is expected to reliably simulate the net C balance of peatlands. I am not convinced that the evaluation of C content across the soil profile, as presented in the paper, provides sufficient information to evaluate this aspect. Shouldn't a comparison be done against dated peat cores, where the amount of C (left today) per age bin is given? The model doesn't track

age bins explicitly, but could be extended to simulate C14 decay and transport across the soil layers (so that lower layers would have an older C14 age, which could then be compared to the C14 age across depth in dated cores). Alternatively, one could write out soil C inputs and decomposition rates at all time steps and resolve age cohorts explicitly offline (diagnostically). I understand that this is a substantial challenge, but I am not fully convinced that the evaluation presented here is sufficient. At least a discussion of these points should be added.

* I simply did not understand Fig. 1.

* Should become clear upfront what parameters are calibrated and what observational targets are used for calibration.

LESS MAJOR (BUT NOT MINOR)

* Better define the scope of the model and the evaluation, the scale at which the model is expected to yield reliable results, what simplifications have taken to get there, and where the model is not applicable. This can be achieved by more clearly stating upfront for what research questions the model is expected to be applied, and what it therefore needs to simulate with fidelity (and why these quantities). And then present the results with a focus and structure to address these quantities. This is largely done so already, but it would greatly help the reader to improve the structure of the paper in this sense. I would expect the following key quantities:

* total (northern) peat C: ok

* spatial patterns of peatland extent: ok, although the particularly extensive peat area in the Hudson Bay Lowland is largely missed by the model.

* basal age/inception, compared to first year of peatland establishment in model: It would be good to evaluate simulated and observed basal ages across space, e.g. with a map showing the simulated dbasal age across space and dots on top of it for observed basal ages from different cores.

* peat C accumulation/respiration history: The net C balance through time is what is relevant for the C cycle (what the atmosphere "sees"). I am not convinced that the evaluation presented here, looking at C content across depth, is giving us the right information to evaluate the model in this respect. The dimension time is missing (as mentioned above); there is no age scale of the cores factored into the analysis.

* Vertical peat growth model: I didn't intuitively understand the rationale for using bulk density data to formulate the vertical growth/downward transport model. Why didn't you use volumetric C content? Can your approach be described as a sequence of C-buckets that fill up by receiving inputs from the layer above (once this "spills over")? Then, spill-over is happening when the typical empirical volumetric C density at the respective depth, as measured in your 102 cores, is achieved. I'm just thinking out loud here, trying to make sense of the model. But maybe you can include such an intuitive description of your approach in the paper.

* While the striking performance in simulating inundation is definitely a plus, it remains unclear how this improvement over earlier publications (e.g., ST14) is achieved. Is it related to resolving the soil hydrology across layers instead of using a simple bucket model? The inundation sub-model is key for the peatland extent model and warrants a bit more attention in the paper.

* I don't think it's appropriate to require every model presented in GMD to be fundamentally novel. Furthermore, the model presented by Qiu et al. is largely an adoption of an existing model (ST14), which itself is based on Kleinen et al., 2012. Sufficient reference is made by Qiu et al. to this earlier work. However, the authors introduce and motivate their work with (l.94) "While both studies made pioneering progresses in the modelling of peatland ecosystems, they adopted a simple bucket approach to model peatland hydrology and peatland C accumulation, and neither of them resolved the diel cycle of surface energy budget." However, it is unclear why the diurnal surface energy budget needs to be explicitly simulated in this context, and what limitations the simple bucket model approach incurs. It definitely needs more clarification what the model

adds to our knowledge and our predictive power and I am sceptical that resolving the diurnal cycle of surface energy exchange adds a great deal. I am more curious about whether resolving soil hydrology across multiple layers helps better simulating relevant peatland-related processes, but the paper doesn't provide this insight. I think it is important that it becomes better clear what the merit of this model (over existing ones) is.

* Observed (Mc Donald et al., 2006) and modelled inception age could be compared across space rather than just showing the numbers across time in Fig. 10. Actually, this comparison is subject to a possible sampling bias in Mc Donald. You want to test whether the model simulates the right inception time at a specific location, and not only the fraction of total number of simulated against the total number of sampled peatlands sampled in each age bin.

MINOR

* l.21: I wouldn't subscribe to 'recently'.

* l.34: "270-540 PgC" Seems to be at the low end. What's the reference? On l. 44 references are given. But I suggest to use the latest (Yu, 2010) as the benchmark.

* l.48 "in environments..." Make a new sentence, as this is not related to the first part of the sentence

* l.49: Change 'despite' to 'while'.

* l.64/65: Weird sentence. The depth itself doesn't prevent oxygen supply.

* l. 69: Unclear: "critical level [of WTD???]"

* l.69-74: This sounds like the authors highlight a unresolved challenged here that the model/paper is going to address. However, it's unclear what is meant here (of course, WTD determines soil moisture or vice-versa), and how the model and results presented here address this particular challenge.

* l. 70: Isn't WTD linearly related to soil moisture content? Why the threshold?

* l.76: Style: don't refer to 'groups'.

* l. 92: I would say that the key in ST14 was to account for peatland-specific water storage capacity in typical organic soils ("sponge" feedback) which enabled to accurately simulate the particular patter of peatland areas across the globe.

* l. 98: Unclear what "discrepancies" are referred to.

* l.121: 'multi' instead of 'many'

* Eq. 4: Why isn't it flux = f * (C_l - M_th,l)? The way it's formulated, the C content may drop below the threshold after transfer. Shouldn't it stay "saturated" after accounting for downward transport?

* Eq. 5: What's the rationale for introducing parameter f_th? Why isn't it 1?

* l. 216: ... than what? Explanation would be helpful: More computationally efficient than determining water table depth for each sub-grid pixel.

* l. 227: Ambiguous formulation. Do you mean max(monthly values)?

* Section 2.2.2.: Put Fig. S2 into main text and highlight difference to ST14.

* l. 238: Not quite correct. I don't know what the authors refer to here.

* l. 249: Question: Are non-growing season months discarded or do they count towards N?

* l. 254: What's the difference to ST14? Using only water balance during summer months instead of entire year? Might be worth mentioning explicitly. What's the rationale for this choice? Note that winter precipitation is relevant too as summer snow melt is effectively delayed winter precipitation.

* l. 258 "May-September": Warning: this would mean that the model is not applicable in the south.

* l. 270: Clarify: C_lim “is defined here as. . .”

* l. 278: “SubC”: What’s this?

* l. 280-283: Add reference to ST14 as this is the same procedure as chosen by them.

* l. 350: Missing references

* l. 371: Figure for modelled vs. observed depth would be instructive.

* Fig 1/Sect. 4.1: I didn’t understand Fig. 1 and how I can read the RMSE from that figure. I expected a comparison of modelled and observed peat depth (or total column C), possibly split by temperate/boreal/arctic and/or bog/fen.

* l. 411: Worth including figure in main text.

* l. 425: Are leptosols and agricultural peatlands simply deducted from simulated peatland areas?

* l. 440: Abbreviations introduced?

* l. 455: “we can only make the..." I don’t understand this part.

* l. 489: “several”: delete

Beni Stocker

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