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Comment on bg-2022-218

Alex Cobb (Referee)

Referee comment on "A process-based model for quantifying the effects of canal blocking on water table and CO_2 emissions in tropical peatlands" by Iñaki Urzainki et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-218-RC2, 2023

General comments

This article addresses the question, "How effective are canal blocks in raising WTD [water table depth] over large areas?" in the context of tropical peatlands. This is important because drained tropical peatlands release CO2 by decomposition and are also vulnerable to catastrophic fire. These environmental impacts can be reduced where peatland drainage can be reversed, re-waterlogging the dry peat. Many recent studies have called for peatland rewetting, and canal blocks are currently the most widely deployed approach to that end. Therefore, study of the effectiveness of canal blocks is warranted. As the authors state, to evaluate the effectiveness of canal blocks, a combination of field measurements and simulation is required: before-after water table measurements are not enough because different precipitation patterns in different years can result in different water table behaviors, even without any canal blocking. The authors completed a simulation study, with a "reality-check" calibration, in which they simulated the dynamics of the water table in the peat, and water levels in canals, with and without canal blocks. Simulations were performed for four sets of peat hydraulic property profiles. They found that the effects of canal blocks were essentially restricted to the neighborhood of canals, so that effects on spatially and temporally average water tables were small. The simulated benefits of the canal blocks, relative to the unblocked condition, extended further in parameterizations in which peat was more permeable. Effects of canal blocks were larger with the precipitation forcing from a dry year than forcing from a wet year.

The main contribution of the work is the advancement of modeling of these landscapes. In particular, the authors highlight their approach to simulating open-channel flow in the canal network, which was coupled to their peat porous-medium flow model through an alternating-step approach. There are few simulation studies of tropical peatland hydrology on the scale of whole networks of canals and groundwater in the literature. This paper develops the authors' previous work further by also simulating open-channel flow in canals, with some novel aspects. The simulation results agree with the consensus that the benefits of canal blocking are fairly local to canals, at least on short (one-year) time scales. The bulk of the manuscript is well written and quite clear, and it cites appropriate references. The biggest weakness in the design of the study is in the validation and calibration of the model, which is limited to a "sanity check" against limited field data. This

could be highlighted a bit more in the Discussion, particularly in light of the noticeable differences in the distribution of measured and modeled water table depths, especially when the water table is high (see Specific Comments). I don't consider this to be a fatal weakness, as the advances in the modeling approach still seem useful.

I believe there is a subtle error in the mathematics underlying part of the peat hydrology model (equation 5 in connection with equations 8, B4 and B5; see further details in attached PDF). The impact on the results might be small, but it is hard to know for sure, and therefore the study cannot be fully evaluated until this error is fixed and the simulations are re-run. I have gone through other parts of the manuscript anyway, and provided comments that I believe will still be relevant after this error is corrected. Though I did not thoroughly check the discretized equations in the appendices, the Methods looked good otherwise, and I believe that this paper could make a valuable contribution to the literature after this error is corrected and the simulations and analysis are redone.

Evaluation with respect to review criteria

1. Does the paper address relevant scientific questions within the scope of BG?
Yes.
2. Does the paper present novel concepts, ideas, tools, or data?
Yes.
3. Are substantial conclusions reached?
Yes.
4. Are the scientific methods and assumptions valid and clearly outlined?
Almost entirely. There is an apparent error in the mathematics that invalidates the simulations. I believe it would not be very difficult to fix this error and re-run the simulations.
5. Are the results sufficient to support the interpretations and conclusions?
See 4., otherwise yes.
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
Yes.
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
Yes.
8. Does the title clearly reflect the contents of the paper?
Yes.
9. Does the abstract provide a concise and complete summary?
Yes.
10. Is the overall presentation well structured and clear?
Yes.
11. Is the language fluent and precise?
Yes, issues were very minor and are outlined below.
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
Yes, though see 4.
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
It reads well. Some features and text in some of figures are too small to easily interpret at printed size; this could be fixed by enlarging those features, or the figures.

14. Are the number and quality of references appropriate?
Yes, with one minor suggestion below.
15. Is the amount and quality of supplementary material appropriate?
Yes.

Specific comments

- Title: I suggest dropping the word "restored," at the authors' discretion. Though canal blocking is a restoration activity, the results presented here (and elsewhere) suggest that it would be premature to say that a peatland in which canals have been blocked has been "restored." Anyway, the restoration activity is implied by the phrase "canal blocking" in the title.

- Abstract

- Including the phrase "peatland restoration" in the abstract might make it easier for people to find the article by keyword searches.

- Lines 11-12: "blocks raised the annual mean WTD by only 0.9 cm."

This spatially-averaged amount is small, but perhaps the water table is raised the most where it is most important, near canals? This wouldn't matter much for CO2 fluxes if they are an affine function of water table depth, but it could affect the likelihood of fire. This is something that could be mentioned in the Discussion, at the authors' discretion.

- Introduction

- Line 20: "extensive tropical peatland areas have been converted to agricultural and plantation forest production"

Might consider also citing this paper, which focuses precisely on this issue: Miettinen, J., Shi, C., & Liew, S. C. (2016). Land cover distribution in the peatlands of Peninsular Malaysia, Sumatra and Borneo in 2015 with changes since 1990. Global Ecology and Conservation, 6, 67–78. doi:10.1016/j.gecco.2016.02.004

- Line 26: "export to water courses": change to "export of nutrients to water courses" to

avoid any ambiguity

- Lines 24-26: I think all the references in this paragraph except Laurén et al., 2021a and Nieminen et al., 2017 report findings from tropical peatlands. The inclusion of these references deviates from the paragraph's focus on tropical peatlands; are they needed?

- Line 50-51: "Putra et al. (2022), on the other hand, presented a good experimental setup to analyze block efficiency"

This study also examined the effects of bunds; this could be mentioned here or in the Discussion.

- Figure 1: The symbols in the legend and the main figure are hard to see. The text is also a bit small at printed size. Could the whole figure be enlarged, or at least the symbols and text enlarged?

It would be helpful to restate the resolution of the DTM here.

It would also be nice to see some more detail on the DTM in the vicinity of the canals (a zoom-in or cross-section?). Are depressions around canals visible in the DTM?

- Lines 79-80: "At each timestep, these modules work in an alternate fashion to update the next day’s canal water level (CWL, meters, negative downwards) and WTD across the study area."

Not a requirement for publication, just a comment: for future work, it would be very interesting to see error estimation (and possibly control) linked to this alternating-step approach. The following paper that the authors may have seen presents a clever and very general approach, based on Richardson extrapolation, to error estimation and correction of operator-splitting methods: Gasda, S. E., Farthing, M. W., Kees, C. E., & Miller, C. T. (2011). Adaptive split-operator methods for modeling transport phenomena in porous medium systems. Advances in Water Resources, 34(10), 1268–1282. doi:10.1016/j.advwatres.2011.06.004

- Line 114: "The Manning friction coefficient was described as follows"

Briefly state the basis or precedent for the functional form of n when the water surface is above the canal bed.

- Lines 118-119: "In the absence of better information sources the values for these parameters were chosen by trial and error"

Clarify - how were the outcomes evaluated when choosing parameters?

- Lines 135-138: "In our implementation of the model, disconnected components of the canal network were solved independently, in parallel processes. The accelerated Newton-Raphson method introduced by Liu and Hodges (2014) was used to solve the resulting linear system of equations."

If I understand correctly, should this not read "systems of equations" instead of "system" (one system for each connected component), and "nonlinear" instead of "linear" (each solver step solves a linear system, but each system of equations being solved is nonlinear)? If I misunderstood - please clarify.

- Line 144, equation 5:

It is not generally true that D grad Θ = T grad h with Sy and Θ as defined here; there is an additional term arising from the gradient in the peat surface elevation and nonuniform specific yield. Please see the PDF attached to this review.

- Line 166, Equation 9: T, α, and β are all functions of d, which varies in space; is it worth highlighting this by including this functional dependence in the equation by writing T(ζ, d), α(d) and β(d) here and / or in equation 10?

- Line 167: "where d is the local peat depth [m, negative downwards]"

Wording (with "negative downwards") a little confusing to me; consider "where d is the local peat thickness [m]"?

- Lines 175-175: "Fixed head Dirichlet boundary conditions with value ζ = −0.2m were applied at the domain boundaries."

Possible to state somewhere the sensitivity of model outputs to this choice of water table at the boundaries?

- Lines 178-186, Module interaction

"water flow between any two adjacent mesh cells that corresponded to the canal network was completely restricted by setting D = 0 in the cell faces."

Does this refer to cell faces completely within the canal? not cell faces along the boundary of the canal?

"the head difference at canal cells before and after the execution of the PHM was used to compute the lateral water inflow q for each timestep"

Does this refer to the gradient towards the canal, evaluated as a finite difference across faces along the boundary of the canal?

A very simple drawing showing the mesh and flows in the neighborhood of a short section of canal could help a lot to understand exactly what was done.

- Lines 188-189: "Whenever the CWL rose above ground, the volume of ponding water was distributed instantaneously throughout the cell area in the PHM step"

More explanation about how this was done would be helpful. What is "the cell area" in this statement? Was water allowed to spread to cells some distance away from the canal, depending on the peat surface elevations in the vicinity of the canal?

- Line 195: Minor point, for accuracy, was the lidar dataset collected by Deltares as stated? I know that in many cases they have analyzed data (and created DTMs) based on LiDAR datasets collected by third parties.

- Line 207, "reality check": What was done in the reality check? Were results examined visually?

- Figure 3: Units on left vertical axis: use "mm / d" for clarify, as in Figure 7?

- Line 239: "In an Indonesian peatland, Cobb et al. (2017)..."

The site is in Brunei, so this could be "In a Brunei peatland" or "In a Borneo peatland"

- Lines 248-249: "This small scale variation in peat elevation is known to be of tens of centimeters (Lampela et al., 2016)"

For what it's worth, this is also shown in the total station transect survey presented in Cobb et al., 2017 (Figure 3); that paper could also be cited here, if desired (given that it is cited anyway).

- Figure 4: Specific yield function: Not necessary for publication, no need to cite this paper or discuss this issue, but possibly of interest: Dettmann and Bechtold (2016) and references therein discuss the effects of microrelief on the specific yield profile near the ground surface.

This study also discusses the effects of the truncation of the soil moisture profile at the soil surface on the specific yield, which, in a homogeneous soil, results in a decrease in the specific yield as the water table approaches the surface from below.

- Line 266: "The modelled WTD raster that best agreed with those sensor measurements was selected as the initial condition..."

What is the set of simulations that are being chosen from here? Do I understand correctly that the initial-conditions raster was chosen from among the 50 rasters from the end of each of the 50 simulation days? Please clarify.

How was agreement evaluated?

- Line 288-289: "The daily fluctuations produced by variations in precipitation and evapotranspiration were comparable in their magnitude."

I wasn't sure what was meant here.

- Figure 5: The measured WTD is lost behind the modeled lines. It might help if the measured WTD were plotted on top, and made opaque or darker? It might be easier to follow the modeled WTD if it is plotted in back, because it is represented by lines.

- Figure 5: In my PDF viewer (evince) and on my printer, parts of the axis labels in this and some of the other plots are missing or replaced by triangles, so I couldn't read the axis labels in this figure, though I could guess at what they should be from context.

- Figure 5: It looks like high WTDs at some locations in the observed dataset are not being captured in the model. Or is this somehow an artifact of the DTM resolution, or the datum for some of the manual measurements?

- Figure 6: It would be nice to see the canal block positions in this figure too if this does not clutter it too much; it could help a reader to immediately interpret the areas where the water table was lower in the blocked scenario.

- Lines 323-324: "... higher hydraulic conductivities had the effect of increasing the spatial extent of the influence of blocks."

The higher conductivities correspond to different K and T profiles, so I wonder how the mean water tables were different for the different parameterizations without canal blocks? I guess that the water table will reside lower in the peat profile in simulations using the higher K parameterizations?

The motivation for the different parameterizations was not very clear to me; perhaps more could be said about this in the Introduction or the Discussion. Is this an exploration of the sensitivity of the results to a these physical properties, because they are not very well constrained by calibration or validation?

- Figure 7: The dashed lines and shaded areas were only visible to me at 200% zoom. Could the figure be made larger (full page width)?

- Figure 7 caption: "Spatially averaged WTD differences between the blocked and non-blocked scenarios for all the modelled scenarios": slightly confusing because the first rows are WTDs, not WTD differences. Perhaps "Spatially averaged WTD and WTD differences..."

- Figure 8: Good figure but too small to see without zooming way in (not sure if this has to do with how the PDF is created for review).

- Lines 337-338: "Finally, in order to make generalizable claims the sensitivity to weather conditions and peat hydraulic properties should be accounted for."

How do I use the peat hydraulic properties to generalize?

- Lines 342-343: "To our knowledge, the present work, which builds upon our previous study (Urzainki et al., 2020), is the first that tries to meet all the aforementioned criteria."

Is this true of studies of higher-latitude peatlands as well? For example, there have been a lot of outputs from rewetting studies in UK peatlands in recent years; I am not sure if any of them meet all the criteria stated here. Clarify that these statements are meant to apply to tropical peatlands (rather than peatlands generally), if that is the case?

Also, though the approach is different, this recent study might also be worth mentioning in this section: Salehi Hikouei, I., Eshleman, K. N., Saharjo, B. H., Graham, L. L. B., Applegate, G., & Cochrane, M. A. (2023). Using machine learning algorithms to predict groundwater levels in Indonesian tropical peatlands. Science of The Total Environment, 857, 159701. doi:10.1016/j.scitotenv.2022.159701

- Lines 407-410: "The coupling between the PHM and CNM approximates water balance, but it does not strictly conserve mass. However, this is an unavoidable drawback of any hydrological model that solves groundwater and surface water flow in different modules (Barthel and Banzhaf, 2016)."

As mentioned above - the approach in Gasda et al. (2011) could make it possible, in future work, to quantify and control the error from coupling.

- Lines 415-423: "... The parameters specifying canal geometry –width, depth and cross-section shape– were determined by local expert observation..."

Some of the explanation here of how parameters were set should also appear in the Methods.

- Line 435: "the modelled WTD presented similar ranges and slopes in the daily dynamics driven by precipitation and evapotranspiration."

This could be a place to mention the disagreement in measured and modeled water table depth distributions at higher water tables (see comment on Figure 5).

- Line 451-452: "In fact, having shallower WTD as the only optimization goal may not be desirable due to increased methane emissions"

Though I'm not aware of any tropical peatland study where methane emissions outweigh CO2? Worth mentioning this?

- Line 460: "Forest Carbon provided the data."

Perhaps "Forest Carbon Pte. Ltd." or a URL to make it easier to find the organization, for those who have not read the affiliations at the beginning of the article?

- Line 517, Equation B1: Sy is a function of ζ or of h and p (equation 8); T is a function of d as well as h (equation 9). Write Sy(ζ) or Sy(h, p) and T(ζ, d) or T(h, p, d)?

- Lines 524-525: Equation B3, "where i.b. denotes the impermeable bottom": I believe the lower bound of the integration needs to be at the fixed vertical datum z = 0 for consistency with the definition of Θ in equations B4 and B5.

- Line 527, equation B4: Show the limits of integration, which I believe should be z = 0 to h.

- Lines 533 and 537, equations B6 and B7: Is it worth writing Θo(p) to highlight the functional dependence of Θ on p, which varies in space?

Technical corrections

- Line 28: "onsets mechanisms": Initiates? Activates?

- Line 120: "rage" -> "range"

- Line 199: Missing a verb at "scenarios, the following section for more details"

- Line 236: "The values reported for the transmissivity and its derivative, the conductivity, K, vary ostensibly" - "ostensibly" does not seem the right word, given the context? If this is really what was meant, explain more?

- Line 538: "constrain" -> "constraint"

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2022-218/bg-2022-218-RC2-supplement.pdf