Reviewer Comments and Author Responses

The authors would like to thank the reviewer for the comments and suggestions. The reviewer comments are provided below in black and our response in green.

1. The authors are right that Howie and Hebda (2018) report large oscillations in peat surface level for *Sphagnum* peat. I admit to being surprised by the values reported, but am also satisfied that Howie and Hebda used robust techniques for measuring surface levels. Notwithstanding, the authors must also know that, in many cases, the amount of fluctuation is very much less than 20-30 cm. For example, co-author Large has measured directly much smaller oscillations in areas of *Sphagnum* blanket bog (Marshall *et al*. 2022: file:///C:/Users/User/Downloads/remotesensing-14-00336.pdf). Howie and Hebda also cite Mawby (1995) (Effects of damming peat cuttings on Glasson Moss and Wedholme Flow, two lowland raised bogs in northwest England. In B. D. Wheeler, S. C. Shaw, W. J. Fojt, & R. A. Robertson (eds), *Restoration of Temperate Wetlands*. New York: John Wiley & Sons Ltd.) who measured fluctuations of 3-3.5 cm on raised bogs in northern England. Relatively small annual ranges of a few cm are also reported for *Sphagnum* peat bogs in west Wales including for central bog areas in a near-natural condition (Hrysiewicz et al., 2023: file:///C:/Users/User/Downloads/remotesensing-14-00336.pdf). My point is that surface motion and poro-elastic effects may, in many cases, be much smaller than simulated by the authors; in other words their findings about the importance of poro-elasticity in peatland development may be exaggerated. It would be very useful if they could indicate what levels of surface motion are simulated in the model runs that they present in the paper so that readers can place the results in the context of published data on surface motion.

We do not model seasonal surface motion of the peatland in the current manuscript so it has no bearing on our results. In due course, we or others may use the 2D model to explore lateral variations in seasonal surface motion but that exceeds the remit of the current manuscript which is to publish a continuum mechanical model and analyse the effect of lateral variations on the peatland behaviour. The reviewers concerns are with respect to the work published in a different paper on the influence of mechanical-ecohydrological feedback on seasonal surface motion in 1D (Mahdiyasa et al., 2023). In that paper, we show that the magnitude of surface motion is affected significantly by Young's modulus, which represents the stiffness of the material, through the sensitivity analysis. Concerns related to the results published in that paper should be address to the Ecological Modelling journal in the context of the published 1D model.

2. I appreciate the authors wishing to showcase their model, but their point about saving a more detailed analysis of the poro-elastic effect doesn't make sense because in the paper they compare their model with the original version of DigiBog which does not contain the effect. I comment later on the problems of the comparison with DigiBog. Surely, the best way of gauging the importance of the poro-elastic effect is to run their model with the effect switched off and with the effect turned on such that it gives surface motions that are in the range of typical values noted above. I don't understand why the authors won't do this; it should be a key part of evaluating and presenting the new model. Hence, I disagree with their closing comment above suggesting their model "indicate[s] clearly the importance of mechanical-ecohydrological processes".

The reasons why we do not turn off the poroelastic effect on our model are highlighted below, which also relate to the use of the earlier version of DigiBog as a non-poroelastic proxy

a. Poroelasticity is fundamental physics that influences the density, porosity and hydraulic conductivity of porous material by allowing volume to change, and pore space to expand or compress.
b. To turn off poroelasticity we would need to stop the solid structure of the peat from deforming. This can be achieved by setting the stiffness of the peat so high that it cannot deform. In this circumstance, as decay proceeds mass would be lost by decay while volume would remain constant. So porosity would increase, density would decrease and hydraulic conductivity would increase. These results would be nonsensical.
c. In peat models that do not allow volume to change as consequence of mechanical deformation (e.g. compaction) the effect of poroelasticity is accounted for by various empirical relationships, for example, between hydraulic conductivity and decay. This ensures sensible results even though the underlying physics is not intrinsic to the model.
d. To change our model to have no poroelasticity and produce reasonable results we would need to create an entirely new model based on empirical relationships between decay and hydraulic conductivity and set the density to a reasonable value for peat. As a consequence, the model could not be a continuum model.

e. Rather than create an entirely new discontinuous model we choose to consider the lack of mechanical deformation by using the early version of DigiBog. Although the reviewer would prefer us to use a more recent version of DigiBog, the earlier version is more comparable as it maintains the annual increments and layer properties without lumping the layers for numerical efficiency into larger averaged layers. More importantly, it provides sufficient tests the effect of dialling down the poroelasticity, while still assuming reasonable hydraulic conductivity and density and not deviating substantially from our model.
f. In this earlier version of DigiBog annual increments are added, the density and porosity are set at the start, hydraulic conductivity is allowed to change as a function of decay and volume can only change as a result of the mass lost. There are no volume changes due to the deformation of the peat in this model.
g. The results presented in manuscript show that from a carbon accumulation point of view, there is little difference between the MPeat2D and Digibog, which is testimony to the similarities of ecological and decay aspects of the model and the comparable hydraulic conductivities.
h. The key differences as explained in the text relate to volume and lateral gradients which are expressed in the shape of the peatland and the onset of the unsaturated layer. These differences are to be expected in a model that cannot compact and it serves as a reasonable illustration of the additional influence of poroelasticity beyond the obvious influences on density, porosity and hydraulic conductivity.

3. The authors do not address my concern. It doesn't make sense to suggest the model is reasonable because its values of hydraulic conductivity fall within the several orders of magnitude range of hydraulic conductivities recorded for peats.

We added the comparison between hydraulic conductivity from MPeat2D with the field measurements (Figure 14).

4. I understand the authors do not take the first derivative of the age-depth curve. However, the studies to which they compare their model results do, and it is these studies that are in error; they should not be used to validate the net C balance of MPeat2D. So, again, I caution against comparing model results to these other studies.

We agree with the reviewer and remove the comparison with Charman (2002) and use the data from Aaby and Tauber (1975), Yu et al. (2010), and Yu et al. (2009).

5. The authors do not address my concern. My original point is that many raised bogs (most?) are approximately hemi-elliptical in cross section, and that MPeat seems to predict very different shapes from this common form. It is interesting to consider whether peatlands fit or don't fit Ingram's and Armstrong's models, but that is not the point I am making. I am concerned that MPeat doesn't give the right general shape as revealed by data from topographic surveys across many raised bogs. I agree with the authors' desire to avoid, at this stage, a detailed comparison with a specific site (see 6 below), but I think it would be useful for them to comment on the general realism of their model predictions.

A characteristic of the hemi-elliptical shape is the increasing curvature of the gradient toward the margin. Cobb et al. (2024) showed that the shape of the peatland through the appropriate comparative profiles is not hemi-elliptical (Figure 1), the gradients are much more linear. The only profile that approximates to a hemi-ellipse in the Cobb et al. (2024) paper is for a tropical peat. In the Ingram (1982) paper that proposed the hemi-elliptical shape as an approximation to the shape of a raised bog the actual bog profile used for comparison is also not hemi-elliptical but displays linear gradients towards the margins rather than increasing curvature. This more linear gradient towards the margins rather than a marked curvature is what we observed in the results obtained by MPeat2D. We agree to provide comments on the general realism of our model predictions because in this paper MPeat2D predicts the peatland shape under idealised conditions, including, fixed horizontal domain, flat substrates, and constant river elevation at the edges. Removing these assumptions might produce different peatland shapes and enhance the application of MPeat2D. We added a few lines to answer this issue (lines 601-607).

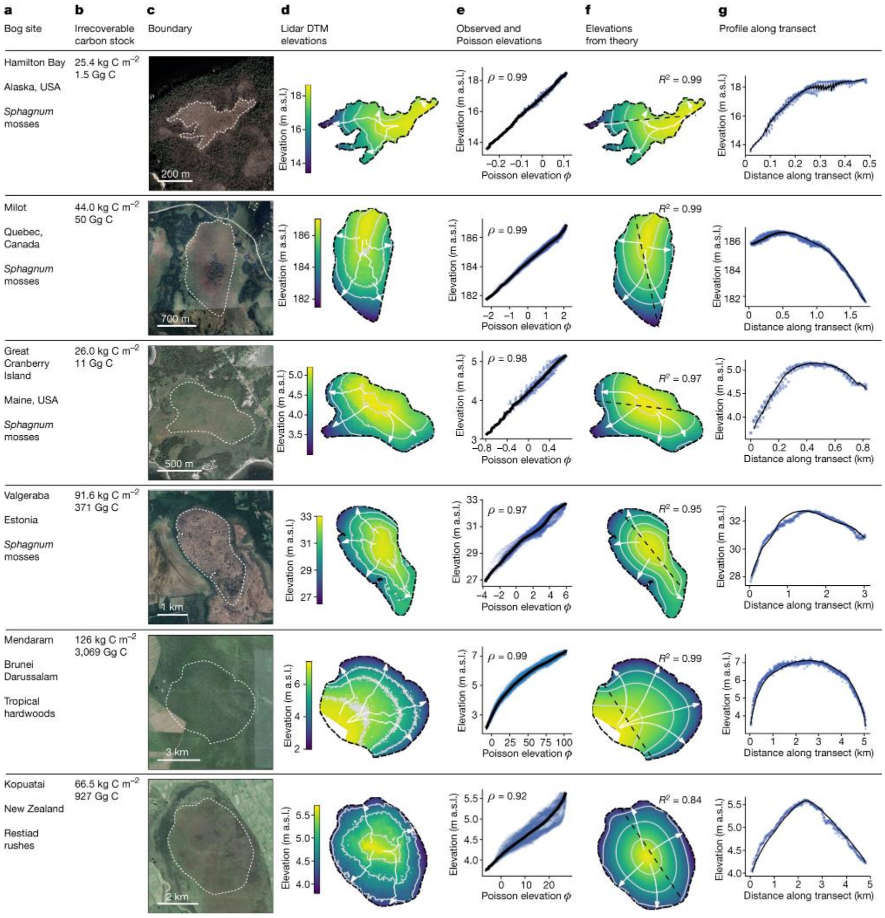

Figure 1. The peatland shape from Cobb et al. (2024)

6. I agree with the authors and look forward to their papers on such detailed comparisons.

7. I find these comments disappointing and somewhat unbecoming. I am quite happy for DigiBog to be criticised. However, the comments above are based on a lack of understanding of how the model works*. The original and now obsolete version of DigiBog was based on running the hydrological model to a steady state for each ecological time step. The new version of the model is fully dynamic hydrologically, and, for that reason, can be expected to give different results from the original. The DigiBog team have thoroughly tested the lumping part of the code and it produces near-identical results to unlumped versions provided the maximum lumping layer thickness is not set too high. The current version of the model can, of course, simulate flat landscapes as well as sloping ones, and can account for situations where no water is stored on the peatland surface (as with the original obsolete version of the model). The model can experience instability if the hydrological timesteps are set too low. Spatial oscillations can also occur in the model depending on how flows to the model boundary

are set and how surface ponding levels are chosen. Dealing with such issues is a normal part of numerical modelling. A bigger concern is that DigiBog is not equivalent to MPeat in all respects apart from having no poro-elastic effect. The results from DigiBog will depend on what values are set for drainable porosity, dry bulk density, and the hydraulic conductivity function. Very different results are possible for the model depending on how these parameters are set. This parametrisation issue is not discussed at all by the authors. As noted earlier (point 2 above), if the authors wish to know how poro-elasticity affects model outputs, they should simply run a version of MPeat with the effect removed – surely, they can parameterise the model in this way. If they do this and can show that ignoring the poro-elastic effect leads to significant differences in model output, then it is fine for them in their discussion to criticise models such as DigiBog for not including the effect. Therefore, I recommend they remove the comparison with DigiBog and run their model with the poro-elastic effect turned off.
*The DigiBog team provided the authors with the new DigiBog code and help with running it. The authors had some difficulties in setting up the parameters and getting the model to work properly. We were, and are, happy to continue helping them, but they have not followed up on this invitation.

We do not wish to undertake a review of different versions of DigiBog. As indicated above we choose the earlier version for good reason as a comparable non-poroelastic model. We have sought advice from the DigiBog team on more than one occasion and we thank the DigiBog team for their help in implementing their latest code. However, we find that problems persist with the functioning of their model. For example, changing the layer lumping threshold from 0.6 cm to 1 cm, 3 cm, 5 cm, and 10 cm has a profound effect on the output, including the peatland height and water table depth, although the other parameters are kept constant. Therefore, to maintain consistency, we choose to work with what we find to be a more numerically robust earlier version of DigiBog without the lumping threshold.

8. The authors do not address my concern here. They seem to be saying that, because peat is a poro-elastic medium, the poro-elastic effect *has* to be important. If they run their model with a poro-elastic effect set to reproduce a reasonable degree of surface motion and show that that produces a very different peatland from one where the effect is turned off (see 7 above) then I think they are in a position to say that the effect is important. I know from my own work on measuring hydraulic conductivity using piezometer slug

tests that sometimes the peat around the piezometer intake behaves like a rigid soil, and sometimes it doesn't; just because the medium is poro-elastic does not mean the effect is important. I am quite happy to acknowledge that it is, but want that to be demonstrated using the MPeat comparisons I recommend above. In particular, I think it is important to know at what degree of poro-elasticity does the effect become significant.

This is discussed in detail in the comment above (No. 2).

9. What the authors say here seems reasonable. I would add that variations in bubble contents may or may not cause changes in peat volume. This is an interesting area of research that requires more work.

10. The authors don't answer my question. For similar dry bulk densities and degree of decay, does MPeat2D produce a similar value of K to the Morris *et al*. (2022) model, which, of course, is data-based? Also, Morris *et al*. (2022) developed a model that predicts log-linear and log-log relationships with other physical variables, so it is not a simple linear model as implied in the authors' response above.

We provided a comparison of hydraulic conductivity between MPeat2D and Model 3 Morris et al. (2022) in Figure (14). The value of hydraulic conductivity predicted by Model 3 Morris et al. (2022) is in between the value of hydraulic conductivity simulated by MPeat2D obtained from the centre and margin (lines 538-541).

11. I agree that I wrongly highlighted Clymo (1984) and then discussed Clymo (2004). However, Clymo (2004) was referenced in the same parentheses, so my point surely remains. And why now delete reference to Clymo (2004)? It contains data that don't conform with the authors' model so should cited as counter-evidence that the model is producing the right patterns. I looked again at Clymo (1984) to check the authors' claim that it supports their model. I reproduce below Figures 1, 8, and 16 from Clymo (1984) as cited by the authors. Figure 1 shows a short section of a peat profile. There is a step increase in bulk density at a depth of 20 cm and then a decline to a depth of 50 cm, with no data from deeper in the profile, so I am not sure the pattern is as expected from MPeat2D. Figure 8 shows a general increase in bulk density with depth over a depth range of several metres. However, between the ground surface and a depth of ~2.7

metres the bulk density is constant. Again, this seems to be inconsistent with MPeat2D. Figure 16 shows data to a depth of ~90 cm and shows a general increase in bulk density. It is noteworthy that Clymo (1984) includes other datasets – in Figures 7 and 12, which are also reproduced below – some of which show no depth trend in bulk density values. It is not clear why the authors ignore these figures in their rebuttal. Clymo (1984) is perhaps not as supportive of the authors' claims as they suggest.

We added a few lines to acknowledge that some of the field observations might be different from the MPeat2D model including some data from Clymo (1984) and Clymo (2004). However, the main point is that MPeat2D could offer reasonable explanations and simulations on how bulk density changes in space and time by allowing the volume to change, and pore space to expand or compress through the poroelasticity and continuum model (lines 500-503)

12. Lines 52 This remains unaltered from the original submission. Models without poro-elasticity also predict these patterns. Therefore, I think this statement has the potential to mislead.

We had removed the sentence (Line 52).

13. Lines 61 This remains misleading. The new wording is more or less the same as the original. Models such as HPM and DigiBog simulate strong spatial variations in peat properties.

We had paraphrased the sentence (lines 61-65)

14. Lines 241 More straightforward in what way? Is it less accurate?

It is more straightforward to analyse the influence of mechanical deformation on peatland hydrology because the active porosity and hydraulic conductivity are a function of solid displacement (lines 242-245).

15. Surely, it would make far more sense for the authors to compare two versions of MPeat: one with the poroelastic effect and one where the effect has been 'dialled down' or switched off. DigiBog varies in other ways from MPeat, not just in its lack of a poroelastic effect. The way hydraulic conductivity varies with degree of decay is different in the two models and there may also be differences in how the Boussinesq equation is solved and parameterised. Therefore, a comparison with DigiBog isn't a

simple comparison between two models that are identical except for the poroelastic effect.

This is discussed in detail in the comment above (No. 2).

16. Lines 335 This is not true. The lumping works well and produces near-identical results to versions without the lumping, provided the maximum thickness of a lumped layer is not set too high (> 0.6-1.0 cm). I don't know why the authors make this point.

This is discussed in detail in the comment above (No. 7).

17. Lines 576 See my main report. There is ample empirical evidence that many raised bogs are approximately hemi-elliptical in cross section.

This is discussed in detail in the comment above (No. 5).

References

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