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.

Reviewer 1: Andy Baird

This paper reports on an interesting new version of the MPeat model (Mahdiyasa et al., 2023) that simulates peatlands in 2D. The example used in the paper is a raised bog in cross section. The new model appears to have been set up correctly, and the numerical scheme reproduces analytical results for simple geometries very accurately. Much of the paper is given over to reporting on the simulation of the growth of a raised bog under a constant climate and a varying climate. The authors show that the 2D model gives different results from the 1D version of the model for the centre of the bog and explain how the differences are due to variations in peat properties between bog centre and margin in the 2D version. I think the paper complements the recent paper on the 1D model nicely and deserves to be published, although not in its current form, due to concerns I have about how the model is presented and compared with data and other (non poro-elastic) models. I explain these concerns below.

1. The authors present the results from a single model parameterisation. In a previous paper on the 1D model, Mahdiyasa et al. (2023) report modelled fluctuations of surface level of as much as 25 cm in response to variations in water-table position of about 50 cm. Such a large range in surface elevation seems generally implausible for *Sphagnum* peats, except floating mats in bog pools. In my experience, variations in surface elevation are typically a factor of four to five less than simulated by the 1D model. The authors cite Whittington and Price (2006) who report substantial changes in the position of the peat surface relative to the tubes of unanchored piezometers, but such instruments cannot be taken as reliable indicators of surface elevation.

Although this paper does not present the 1D model, the comment is worth considering. The range of surface motion simulated from 1D model of MPeat (Mahdiyasa et al., 2023) is in agreement with the field observation from Howie and Hebda (2018), who measured the surface oscillation of the raised bog with different plant communities. The range of surface motion reported by Howie and Hebda (2018) from peatland sites dominated by *Sphagnum* is about 15-30 cm. Furthermore, it should also be noted that the actual magnitude of the surface oscillation will depend on the specific site history, which we have not yet attempted to model.

2. The parameterisation used in the current paper is different from that in Mahdiyasa et al. (2023) and the surface motion across the 2D model is not presented or discussed. However, I'd be interested in knowing what happens when the poro-elastic effect is 'dialled down'. How different are the model results? At what point does the poro-elastic effect become of secondary importance compared to the ecological and hydrological processes? I think the paper would benefit from a short section looking at model sensitivity to the degree of poro-elasticity.

We have added a short section related to the sensitivity analysis of the model (**lines 341-345 and 443-459**). We changed the peat Young's modulus, which determines the peat stiffness and is an important variable in the poroelasticity model. A more detailed analysis of the poroelastic effect is beyond the scope of this paper as it would ideally be done relative to field measures and specific peatland types. Our objective in this paper is to present the structure of a fully coupled mechanical-ecohydrological model for peat growth in two dimensions and consider the potential implications of feedback within this model system. This is discussed throughout the manuscript, and this message is strengthened in the discussion to indicate clearly the importance of mechanical-ecohydrological processes together with the spatial variability of water table depth, plant functional types composition, and peat physical properties on peatland behaviour.

3. The authors compare the spatial pattern in their data with data from a blanket bog in Ireland. Although there is some overlap between raised bogs – which is what the authors simulate – and blanket bogs, the two peatland types can be quite different, and I am not sure it makes sense to compare the model of one type with the field results of the other. The authors also report that their simulated peat properties fall within the ranges reported in the literature. I don't think such a comparison is that useful because properties such as hydraulic conductivity can show enormous variation across different peats – 'peat' is not a single soil type. This means that, almost regardless of the values simulated by the model, it will fit within the observed range.

Although the peatland type from Lewis et al. (2012) is different from our simulations, the main reason for the comparison is to demonstrate the ability of the model to produce reasonable outputs of the spatial variability on peat physical properties, including bulk density and hydraulic conductivity. We do not parameterise the model to simulate specific peatland sites and focus on developing a general peatland model. Therefore, we compare our results with the typical range of peat physical properties obtained from the previous studies. We added a few lines to clarify this issue (**lines 522-524**).

4. A somewhat different point applies to the model-data comparison for the rate of peat and carbon accumulation. As shown by Young *et al*. (2021) (https://www.nature.com/articles/s41598-021-88766-8) it is not possible to obtain past rates of net peat and carbon accumulation from the first derivative of the age-depth curve. Studies that purport to do so are, unfortunately, in error and shouldn't be used for model-data comparisons.

We do not take the first derivative of the age-depth curve. We calculate the long-term rate of carbon accumulation based on the total amount of carbon and the total time of simulations. After that, we compare the results with available data from the previous study. We added a few lines to clarify this issue (**lines 539-540**).

5. The authors don't compare their predictions of peatland shape with data. Many raised bogs approximate a hemi-ellipse in cross section, but the MPeat2D results shown in Figures 5 and 8 show what seems to be a very different profile. I am not convinced the model has that much skill in representing overall peatland form. The authors are encouraged to compare the modelled cross-sectional shape with real raised bogs.

The hemi-ellipse shape of the peatland in the cross-section is proposed by Ingram (1982) through the Groundwater Mound Hypothesis (GMH). This shape is obtained by assuming constant hydraulic conductivity throughout the profile, which is not true because the field observation from Baird et al. (2008) and Lewis et al. (2012) showed that hydraulic conductivity changes in the vertical and horizontal directions. Armstrong (1995) modified the GMH by proposing non-uniform hydraulic conductivity that exponentially decreases with depth, showing different predictions of peatland shape and thickness. This model produces a lower hydraulic gradient at the margin, which is in agreement with our model MPeat2D. Comparing MPeat2D with the shape of a real raised peatland requires specific parameterisation of that site, including peat physical properties, substrate characteristics, and information about peatland age, which might reduce the generalisation obtained from MPeat2D simulations. Note that the primary purpose of this paper is to present a model that can then be developed for a wide range of purposes. We added a few lines to clarify this issue (**lines 578-584**).

6. I understand the desire of the authors to produce some 'generic' model results, but it would also be useful, whether in this paper or a follow-up paper, to apply the model to a particular site to see how well it simulates overall peatland shape, peat properties, and the age-depth curve.

We agree with the referee's comments that a comparison with the particular site would also be useful. However, we believe the site comparison should be conducted after the conceptual and generic model is developed.

7. In the discussion section the authors compare their model's predictions with those from DigiBog. I can't be sure, but they seem to have used an early prototype of DigiBog from 2012 which has long been superseded (since 2014). More recent versions of DigiBog produce a more realistic margin to raised bogs. The authors do not indicate how DigiBog was parameterised, so it is unclear what is being compared here. The DigiBog team, of which I am a member, would be happy to share more recent model code with the authors should they want to use it. Finally, the comparison with DigiBog should be reported in the Model Implementation and Results sections, and not just the Discussion; it is odd to report results in a discussion section.

We compare MPeat2D with the earlier version of DigiBog because both models have similar characteristics, including the flat and impermeable substrate with the symmetric assumption of peatland growth. Moreover, both models also assume that water ponding was lost immediately to the margin. We have tried to use more recent versions of DigiBog by contacting the DigiBog team. However, because of the different and complex parametrisation and setup, the more recent DigiBog versions produce incomparable simulation results to the MPeat2D. The more recent DigiBog versions employ a layer lumping system after some specific time and thickness. This approach results in faster simulation because it reduces the number of layers that become the domain of calculation. However, the different parametrisation of the layer lumping appears to change the results and stability of the DigiBog. Furthermore, the more recent version includes the parameter of mineral soil and water ponding thickness, which also influences the model outputs. These additional features and parameters lead to incomparable conditions with MPeat2D. We agree that the comparison with DigiBog should be reported in the Model Implementation and Results sections. We added a few lines to explain the reason for choosing the earlier version of DigiBog and provide the DigiBog parameter in the Implementation and Results sections (**lines 326-338 and 428-440**).

8. When building a model, modellers usually try and include all the key processes, leaving out those to which the model is not sensitive. There are many ways in which models such as DigiBog might be improved, such as the decay routines which are heavily empirical. The decision on what to include and exclude is also dependent on how much is known about a process. If information on the process is sparse then it will be difficult to include. I welcome the authors looking at the effects of poro-elasticity on peatland development, but I think there remains considerable uncertainty about the importance of the process.

Peat is a mechanically weak poroelastic material due to the low value of Young's modulus (Dykes, 2008; Mesri & Ajlouni, 2007), shear, and tensile strength (Boylan et al., 2008; Dykes, 2008; Dykes & Warburton, 2008; Hendry et al., 2011; O'Kelly, 2015). As a result, the changes in peat pore structure, which significantly influence hydraulic properties, are not only determined by progressive decomposition (Moore et al., 2005; Quinton et al., 2000) but also compression. Hydraulic conductivity decreases when the water table drops due to the mechanical deformation in the pore structure (Whittington & Price, 2006), an important process that can reduce water discharge from peatland. In addition, compression also reduces peat volume, causing the peatland surface to drop. This drop in the peat surface acts to maintain the relative position of the water table, which in turn helps sustain PFTs associated with wet surface conditions (Schouten, 2002; Waddington et al., 2015). The detailed explanation related to the importance of poroelasticity on peatland development is presented in the Mahdiyasa et al. (2022) and Mahdiyasa et al. (2023).

9. Other processes about which quite a lot is known include the build up, release, and dissolution of biogenic gas bubbles below the water table on an annual cycle. Bubbles may occupy more than 20% of the total peat volume, blocking pores and reducing the peat's hydraulic conductivity, and also making the peat more buoyant. To me, these effects would seem to equal or perhaps exceed the effects of poro-elasticity and I would be interested in hearing, via the discussion section, what the authors thought about this possibility.

We agree that entrapped gas bubbles could have a significant influence on the peatland behaviour. The entrapped gas bubbles influence hydraulic conductivity (Baird & Waldron, 2003; Beckwith & Baird, 2001; Reynolds et al., 1992) and pore pressure (Kellner et al., 2004), which results in variations of effective stress. Consequently, the mechanical deformation of peat pore space, including the shrinking or swelling, is also affected by the presence of gas bubbles. The simulation from Reeve et al. (2013) suggested that a higher gas content results in a more significant peatland surface deformation. We could expand the poroelasticity formulation below the water table to accommodate more than one fluid, for example, water and gas mixture (Kurzeja & Steeb, 2022). This modification requires generalisation in Biot's theory of consolidation to model multiphase fluid saturation. We added a paragraph to provide a brief discussion related to this possibility (**lines 605-613**).

10. I have made more comments on a pdf of the paper and this is posted separately for the authors and the editor. Some of the points made on the pdf are covered in the comments above, but the authors are encouraged to respond to those that aren't. Of particular importance is that Equation (17) is given wrongly – as reproduced, it is non-homogenous – I think specific storage should be replaced with specific yield.

We changed specific storage with specific yield in Equation (17) to solve this issue (**line 223**).

11. Line 13 Influence of these on what exactly?

On the peatland behaviour (**line** 14).

12. Line 27 This is a non sequitur. The significance of the effect can only be obtained by comparing models with the real world. It's necessary to compare two models - MPeat2D and a model that doesn't have poroelastic effects - to real-world data.

The comparison with the real-world data requires specific characteristics of the site that could limit our understanding related to the importance of mechanical-ecohydrological feedback. Therefore, to clearly analyse and evaluate the significance of this process, we need to compare it with the other conceptual model that does not include mechanical feedback. We provide comparisons between MPeat2D with real-world data of spatial heterogeneity in peat physical properties, including bulk density, active porosity, and hydraulic conductivity. These comparisons indicate that MPeat2D can produce reasonable outputs of peat physical properties profile, which becomes the limitation of the stiff model without mechanical feedback.

13. Line 51 Spatial variations are also predicted in models that don't have poro-elastic effects.

We agree that some of the 2D peatland development models that ignore mechanical feedback also predict spatial variabilities. However, these spatial variabilities are obtained from empirical relationships or only applied to the specific variable. For example, in the model from

Borren and Bleuten (2006), the spatial variations in the bulk density and hydraulic conductivity are developed based on the empirical relationship between different peatland types, consisting of bog, throughflow fen, and fen. DigiBog (Baird et al., 2012; Morris et al., 2012) predicts the spatial variations of hydraulic conductivity but assumes constant active porosity and bulk density throughout the peatland area. In order to provide a more comprehensive analysis of peatland spatial variations, a fully coupled model that incorporates mechanical, ecological, and hydrological feedback is required.

14. Line 60 Two things are being suggested and both don't apply to all of the cited papers.

We rephrased the sentence to clarify the issue (**lines 60-64**).

15. Line 70 The key point here is whether poro-elastic effects are important enough as factors in peatland development for them to be included in a model. I am not sure they are.

This is discussed in detail in the comment above (No. 8).

16. Line 208 Was the original paper consulted? If not, cite the more recent source

We changed the reference in the manuscript (**line 219**).

17. Line 210 This equation is dimensionally inhomogeneous. Specific storage should be specific yield which is dimensionless.

We changed specific storage with specific yield in Equation (17) to solve this issue (**line 221**).

18. Line 213 Cite the source reference(s) if the original papers were not consulted.

We changed the reference in the manuscript (**line 226**).

19. Line 217 Is surface water lost from the model solution? Is that realistic?

We assume the ponded water above the peatland surface will flow as surface water. This would appear to be a realistic assumption because we do not simulate patterned peatlands. We added a few lines to explain this assumption (**lines 229-230**).

20. Line 225 How does this equation compare to what is revealed in the meta-analysis of Morris et al. (2022)?

The Equation (19) in the MPeat2D is developed based on the exponential relationship between hydraulic conductivity and active porosity through the generalized Kozeny-Carman equation. The basic idea for this relationship is that changing active porosity due to compression affects hydraulic conductivity because water cannot move easily as the pore size becomes smaller. Contrastingly, Morris et al. (2022) developed a linear model to predict hydraulic conductivity from other independent variables, including depth, bulk density, von Post score, and categorical information. We added a few lines to provide the explanation related to the comparison with Morris et al. (2022) (**lines 239-242**).

21. Line 284 Unclear what this phrase means here. I suggest rewording.

We had changed the phrase (**line 209**)

22. Line 288 Why was this used? Why not use a more realistic palaeo climate?

We employ a sinusoidal function with some noise for non-constant climates to capture wet and dry conditions. We do not use the palaeo climate reconstruction model because we want to keep it as simple as possible while also maintaining the effect of variable climate on the peatland growth over millennia. We had rephrased the sentence to clarify this issue (**lines 305-306**).

23. Line 293 It would be better if the parameter values were based on actual data. See again Morris et al. (2022) referred to in an earlier comment.

We use these parameters to produce results that are comparable with the one-dimensional model MPeat (Mahdiyasa et al., 2023; Mahdiyasa et al., 2022).

24. Line 347 Here and in Figure 5, the shape of the peatland doesn't look that realistic. How does the shape compare to real peatlands which tend to follow (approximately) a hemi-ellipse in section?

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

25. Line 349 Is mean annual water-table depth shown in Figure 9?

Figure 9a shows the variation in mean annual water table depth between the centre and margin under a non-constant climate.

26. Line 353 This could be confused with how BP is used in palaeo studies to denote 1950.

We use BP as a general term for before the present and do not indicate a specific year. As such it will not matter how this is interpreted. However, to clarify this issue, we changed the Age (years BP) with Simulation time (years) throughout the manuscript (**Figures 4,6,8,9,10, and 11**).

27. Line 356 'm-2'?

We had changed the unit kg m^{-2} (**line 397**).

28. Line 378 Just this? DigiBog simulates something similar, with the lower K values at the margin being due to the peat being more decayed.

We agree that the spatial variation of hydraulic conductivity is also affected by the degree of decomposition, as shown by DigiBog. However, DigiBog cannot capture the spatial variations of bulk density and active porosity due to the omission of mechanical feedback. Therefore, the spatial variations of peat physical properties are not only affected by decomposition but also by compaction. We had rephrased the sentence to clarify this issue (**lines 473-474**).

29. Line 382 Okay, but the cross-sectional shape predicted by MPeat doesn't look very realistic.

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

30. Line 389 'between peatland microforms'

We had applied the referee's suggestion (**lines 483-484**).

31. Line 392 I don't quite follow what is being said here. I recommend re-phrasing.

We had rephrased the sentence to clarify this issue (**line 488**).

32. Line 396 I don't think this paper is cited correctly here. Clymo (2004) actually shows bulk density being constant while K declines with depth, which is contrary to what is predicted by MPeat.

In the manuscript, we cite Clymo (1984), who provides the data of bulk density with depth, as shown by Figures 1, 8, and 16. The bulk density profile from Clymo (1984) is in agreement with MPeat2D simulations, which indicates an increasing value from the top surface to the bottom layer. For Clymo (2004), we agree to delete this reference from the paper (**line 491**).

33. Line 414 'based their finding on a sensitivity analysis of a steady-state groundwater model.'

We had applied the referee's suggestion (**lines 508-509**).

34. Line 422 I don't think this makes sense; it is not a comparison of like with like. The simulations were for a raised bog and not a blanket bog.

This is discussed in detail in the comment above (No. 3).

35. Line 431 Peats are enormously variable (in the same way that mineral soils are). Therefore, there is a very wide range of reported peat physical properties. Just because the model falls within that very wide range does not provide validation that its predictions are sound or good.

This is discussed in detail in the comment above (No. 3).

36. Line 439 But the cross-sectional shape of the peatland doesn't seem to be.

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

37. Line 442 Unfortunately, these cannot be used to give past rates of net C accumulation as explained by Young et al. (2021): https://www.nature.com/articles/s41598-021-88766-8. It would be better to simulate a real site and compare the age-depth curves from the model and real peat profile.

This is discussed in detail in the comment above (No. 4).

38. Line 456 An old version of DigiBog seems to have been used here - this version was superseded in 2014. I recommend using a more recent version of the model, which the DigiBog team will be happy to share with the authors. This later version of DigiBog has a greater slope at the margin with a less dramatic 'cliff'.

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

39. Line 460 What parameterisation was used for DigiBog? What bulk density was used, and was K set to be comparable to the values used in MPeat2D?

We used the parameterisation from Morris et al. (2012) with the value of bulk density equal to $100\ \mathrm{kg\ m^{-3}}$ and the hydraulic conductivity parameters a and b equal to $1 \times 10^{-5}\ \mathrm{m\ s^{-1}}$ and 8, respectively. We added the DigiBog parameterisation in the Model Implementation Section (**lines 326-328**).

40. Line 463 Is this reasonable, however? What do water-table reconstructions using testate amoebae reconstructions of water-table depth show from real bogs? Do real bogs also show systematic changes in vegetation with time associated with wetting? Some do undoubtedly, but I am not sure such change is anywhere near universal. The opposite has also been observed.

We agree that the site characteristics might affect the relationship between the water table and vegetation composition on the peatland. However, our results are obtained from the first principle that a lower water table position supports the growth of shrubs, while the higher position of the water table increases the proportion of *Sphagnum* in the peatland vegetation communities. Therefore, our approach to simulate the changes in vegetation composition during the development process of the peatlands is theoretically reasonable and in agreement with the field observation.

41. Line 470 I don't think MPeat2D produces realistic peatland profiles which tend to be quite well approximated by a hemi-ellipse, which has a steep margin. Also, the simulations here are from an old, and no longer used, version of DigiBog - see my earlier comment. Finally, the cliff effect is partly an artefact of the discretisation used and the choice of model boundary condition.

This is discussed in detail in the comment above (No. 5 and 7).

42. Line 503 It's not clear that it does. As Baird et al. (2017) note, the very high K is negated by the very low hydraulic gradients in tropical peatlands.

We had rephrased the sentence to clarify this issue (**lines 621-622**).

43. Line 521 Possibly, but patterns occur across bog plateaus with low surface gradients where the peatland can be expected to be mechanically stable.

The peatland surface patterns might appear due to the tensile or compressive failure condition (Briggs et al., 2007; Dykes, 2008) that dominantly occurs under a low slope angle (Dykes & Selkirk-Bell, 2010). Furthermore, mechanical instability can also be linked to wrinkling thresholds and internal stress states. Until such ideas are tested, the mechanical influence on surface patterning remains unknown. We added a few lines to provide the explanation related to this issue (**lines 641-642**).

Reviewer comments in black and response in green.

Reviewer 2: Anonymous

Mahdiyasa et al. developed a two-dimensional peatland process model that incorporates essential mechanical-ecohydrological feedbacks, which can simulate the spatial variability of an individual peatland, especially the differences in physical properties between the peatland centre and edge. The main difference from previous models is that the model considers mechanical deformation and simulates variable peat porosity and dry bulk density. Sensitivity simulations of MPeat2D successfully produce different vegetation compositions between the margin and the centre and show a higher bulk density and lower hydraulic conductivity at the peatland margin compared to the centre. The methodology on plant weight is interesting and the methods section is generally well described. Overall, I enjoyed reading this manuscript, which generates some new ideas about the development of peatland models. I recommend the acceptance of the manuscript after considering the following suggestions/comments.

1. Does peat decomposition take temperature and recalcitrance effect into account? I didn't see this part described in the article

We do not include the influence of temperature and recalcitrance in the decomposition model because they will increase the number of empirical parameters and assumptions, which might lead to a higher uncertainty of the model. The effect of temperature on the decomposition process could be employed through Q_{10} parameter (Morris et al., 2015). However, this parameter has a high range of values between 1 and 10, which depends on the peatland types and temperature characteristics (Hardie et al., 2011; Xiang & Freeman, 2009). Moreover, the inclusion of the recalcitrance effect requires additional assumptions related to the changes in the rate of decay that could decline linearly or quadratically, as shown by Clymo et al. (1998). Therefore, to reduce the model uncertainty, we use a fundamental decomposition model based on Clymo (1984) without temperature and recalcitrance effect. We added a few lines to clarify this issue (**line 206-212**).

2. Taking into account the weight of the plant is an innovation. The manuscript sets the plant weight to the surface portion, which may be applicable to *Sphagnum*. In the case of sedges or shrubs, the underground root system is also part of their productivity, and in some sedges the underground productivity is even greater than the above-ground part.

How do you define plant weight for this type of vegetation with a rich root system? The roots of these plants can be up to 1 metre long and penetrate the peat layer

The plant weight from vegetation with a root system could be modelled through the data of above-ground and below-ground biomass. Furthermore, the plant weight is applied not only at the top surface but also at the specific depth of the peatland, depending on the root characteristics. However, implementing this process in MPeat2D might increase the complexity and reduce the generality of our model because the depth of maximum biomass production from the root, which influences the total weight, is controlled by the peatland types (Moore et al., 2002). The influence of the root system might become crucial for the future development of MPeat2D for modelling tropical peatland behaviour because it affects the mechanical stability of the peatland. We added a few lines to emphasize the importance of the root system and how to implement it on MPeat2D for future development (**lines 624-626**).

3. Line 28: It would be useful to explicit to state what is being highlighted, so the sentence would be more informative. Or reword to "we argue that …feedbacks are important for spatial hetereo…."

We agree to state the important findings of the paper explicitly and rephrase a few lines in the abstract (**lines 27-28**).

4. L47-49: The meta-analysis by Morris et al. (2022) on the variation of dry bulk density and hydraulic conductivity with peat depth (including the relationship between dry bulk density and hydraulic conductivity) would support your point.

The suggestion had been implemented in the manuscript (**lines 50-51**).

5. L175-179: Ecological submodel (2.2). The peat production model in this study is based on the formation of Morris et al. (2015), which was for Sphagnum-dominated peatlands (Belyea and Clymo, 2001). However, in the PFT section you have set Sphagnum, sedge and shrub depending on the water table depth, I'm not sure that the peat production model used here is suitable for calculating sedge and shrub production.Need to explain this clearly. The formula of Swinnen et al. (2021), which has no restriction on plant type, could be an option.

The authors agree that the peat production model used in this manuscript has its disadvantages. However, this peat production model can couple the ecological and hydrological processes through the dependency between peat production and water table depth. Furthermore, it also includes the effect of air temperature, which leads to a more realistic model. Swinnen et al. (2021) employ the global Thornthwaite Memorial equation (Lieth, 1975) that models the primary productivity of the world. This model might omit the unique characteristics and the important feedback from the peatland ecosystem. We added a few lines to clarify this issue (**lines 179-184**).

6. L298. Figure 4 and other relevant figures. As the simulation results were from sensitivity simulations and the results are not really comparing with down-core observations, it is probably better to use "Simulation time (years)" as time unit, rather than "Age (years BP)" throughput the manuscript. Indeed, in the text and some figures (like Figure 5) the time is often referred to as "years".

The suggestion had been implemented in the manuscript **(Figures 4,6,8,9,10, and 11)**.

7. L300: change "between 0.6-1 m yr-1" to "between 0.6 and 1 m yr-1" and elsewhere to use "between … and …" phrase structure.

The suggestion had been implemented in the manuscript.

8. L320: The MPeat2D model output of water table depth under constant climate conditions continues to decrease after the initial period of peat accumulation (380 years), indicating that the peatland is becoming wetter (Fig. 6). This would provide some cautions to the study of palaeoclimate change using peat as an archive, as peatlands are generally thought to maintain a stable hydrological environment for long periods of time without climate change and disturbance. The model does not seem to be able to discriminate clearly whether changes in water table depth are due to climate change or to the model itself (autogenic process).

The decreasing water table depth under constant climate (Figure 6) occurs due to an autogenic process. The loading from peat accumulation increases as the peatland grows, which provides internal feedback mechanisms on the water balance through the deformation of peat pore space. The smaller pore space results in the reduction of active porosity and hydraulic conductivity, which supports the water accumulation. We added a few lines to explain the decreasing water table depth under constant climate on the MPeat2D (**lines 363-365**).

9. L429-430. In the vertical direction, a comparison of the model output peat bulk density with field measurements (layer-by-layer comparisons) could demonstrate the superiority of the MPeat2D model, rather than just a comparison between values. However, such data may not be available.

We agree that layer-by-layer comparison in the vertical directions of bulk density could indicate the superiority of MPeat2D for modeling the changes in peat physical properties. However, this comparison method requires specific information and input data, including Young's modulus, PFT composition, climate conditions, and topography from the observed area. Moreover, it might increase the number of free parameters from the model to capture the particular characteristics of the peatland site. The aim of this paper is to provide a general model of peatland development that incorporates mechanical, ecological, and hydrological processes in two dimensions and consider the potential implications of feedback within this model system. Therefore, we employ the comparison between values obtained from the typical range of peat physical properties from the previous studies.

10. L451-454: The MPeat2D model outputs water table depths that are dramatically expanded during the early stages of peat accumulation (hundreds of years). Hydraulic conductivity is variable in both MPeat2D and DigiBog, and its highest in the early stages of peat formation, what causes the initial water table depth in MPeat2D to be different from that in DigiBog (Fig.11)? Is there a difference in initial hydraulic conductivity?

The sharp expansion of the water table depth is due to the fact that the water table does not rise with peat accumulation; was the peat layer free of water during that period? Can vegetation still grow and accumulate peat in the early peat layer without water?

The variation in water table depth between MPeat2D and DigiBog during the early stages of peatland development occurs due to the difference in the bulk density assumption. MPeat2D allows bulk density to evolve during the development process, while DigiBog assumes bulk density constant over time. The changes in bulk density with time in MPeat2D occur because of the mechanical compaction on the peat pore space. Consequently, in the early stage of development, the value of bulk density from MPeat2D is lower than DigiBog, producing a more rapid increase in peat thickness and a faster appearance of the unsaturated zone. The vegetation can still grow and accumulate peat because the maximum water table depth from MPeat2D, with the value of about 0.3 m, is in the range of water table depth that supports the growth of peatland vegetation (Moore et al., 2002). We added a few sentences to explain the difference in the water table depth profile between MPeat2D and Digibog, particularly at the early stage of peatland development (**lines 559-561**).

References

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