

Dear Editor,

Thank you for your consideration of our paper and thoughtful insights. We wish to acknowledge the two referees for their thorough work and helpful comments, which improved and benefited the manuscript. Below, we have attached each comment along with our detailed reply (*italic*) and actions taken to fully address points raised. We hope you will find the improved outcome suitable for publication.

Sincerely,

Guy Sapir, on behalf of all authors.

### **Associate Editor: Dr. Park's comment**

**C1:** Do your data support your claim that the analyzed section be termed 'pristine' peat?

*R1: This point was also raised by the second reviewer (R2-C2), and was therefore further clarified in the reply below as well as in the revised text itself (L343-353).*

### **Reviewer #1**

#### **C1: General comments:**

This paper presents an interesting work conducted on a peatland in Israel. The place was drained to be used for cultivation, then rewetted when problems (fires, erosion, etc.) began to arise. The authors sampled five 4-meter cores in the deepest part of the peatland, and identified three different parts: drained, rewetted, and pristine peat. They conducted Rock-Eval thermal analyses to characterise the thermal stability, stoichiometry and properties of the peat. The results show that rewetting the peatland clearly helped reduce SOM loss, although the rewetted part does not come back to its anterior, pristine state.

My main concern regarding this work was whether it is robust to use Rock-Eval to analyse peat; we know that Rock-Eval shows some limits with highly organic compounds (e.g. litter), which most probably apply to the case of peat. This problem has been addressed in Supplement: the authors took the precaution to investigate this question with LOI procedure. I would still be very cautious when applying Rock-Eval to such organic soil, however on this specific case the results are clear and consistent. This paper is a nice addition to the peatlands knowledge.

*R1: We agree with the reviewer that Rock-Eval can have some limitations in evaluating the SOC content. Specifically, there are two issues that need to be addressed:*

- 1. Compared to rocks, the default cutoffs between organic and inorganic C in soils need to be adjusted, otherwise SOC can be underestimated by up to ~10% (relatively), and mineral C, SIC, is overestimated (Stojanova et al., 2024). While we did not apply the correction method of Stojanova et al. (2024), we did re-evaluated the default cutoffs between organic and inorganic C using LOI as the reviewer states. We prefer our approach, as the nature of the samples in the present study differs from those in Stojanova et al. (2024). Additionally, the low SIC in the present study, for most samples, should minimize any discrepancy in inaccurate cutoffs between SOC and SIC.*
- 2. Fresh plant tissues can result in under-estimation of SOC, probably because of inaccuracy of the FID detector of the Rock-Eval. However, for diagenetic soils multiple studies have shown that the Rock-Eval is producing meaningful data both for peat and organic-soils with SOC reaching up to 40 wt.% (e.g., Disnar et al, 2008; Marchand et al., 2008; Sebag et al, 2016; Brown et al., 2023). Specifically,*

the work of Marchand et al. (2008) compared SOC of Rock-Eval to total C using LECO combustion in the absence of carbonates, and the comparison was almost in full agreement up to ~ 20% SOC. While we cannot exclude some underestimation of the Rock-Eval SOC, the correlation between SOC and LOI in the current study (Fig. S3) further support that inaccuracy of the Rock-Eval is minimal. Hence, we conclude that the use of the Rock-Eval, despite potential limitations, produced valuable and quantifiable data for discussing peat degradation and CO<sub>2</sub> emissions. In the revised manuscript, a comment was added to address this issue (L198).

References:

- Brown C, Boyd DS, Sjögersten S, Vane CH (2023) Detecting tropical peatland degradation: Combining remote sensing and organic geochemistry. *PLoS ONE* 18(3): e0280187. <https://doi.org/10.1371/journal.pone.0280187>
- Disnar JR, Jacob J, Morched-Issa M, Lottier N, Arnaud F. Assessment of peat quality by molecular and bulk geochemical analysis: Application to the Holocene record of the Chautagne marsh (Haute Savoie, France). *Chemical geology*. 2008 Aug 30;254(1-2):101-12.
- Marchand C, Lallier-Vergès E, Disnar JR, Kérais D. Organic carbon sources and transformations in mangrove sediments: a Rock-Eval pyrolysis approach. *Organic Geochemistry*. 2008;39(4):408-21.
- Sebag D, Verrecchia EP, Cécillon L, Adatte T, Albrecht R, Aubert M, Bureau F, Cailleau G, Copard Y, Decaens T, Disnar JR. Dynamics of soil organic matter based on new Rock-Eval indices. *Geoderma*. 2016 Dec 15;284:185-203.
- Stojanova, M., Arbelet, P., Baudin, F., Bouton, N., Caria, G., Pacini, L., Proix, N., Quibel, E., Thin, A. and Barré, P., 2024. A validated correction method to quantify organic and inorganic carbon in soils using Rock-Eval® thermal analysis. *Biogeosciences*, 21(18), pp.4229-4237.

**Specific comments:**

**C2:** L135: does that mean the WTL was at surface level before drainage? Do we have information on this? (I understand 'old' information is scarce.)

**R2:** *Our understanding of the water table prior to the drainage of the marsh, during the period of the peat accumulation, is derived from a documented expedition by Jones (1940) and hydrological data that were documented by the hydrological survey and published in a book by Dimentman et al. (1992). Jones reported that the characteristic water table in the marsh was 0.5-2 m above the surface in the main papyrus stands. He also mentioned that in places, during the summer months (probably in the northern sections), the peat was exposed and the water table was found 0.5-1 m below the surface. These data align well with monitoring data of the hydrological survey that recorded the water table absolute elevation since 1935 and brought by Dimentman et al. (1992). These data show that during most years the water level fluctuated in a range of 1.0 to 1.5 m, with corresponding total flooded area between ~15 km<sup>2</sup> in summer (August-September) and ~45 km<sup>2</sup> during the winter floods (January-March).*

*Relevant data added in the manuscript, L135.*

References:

- Dimentman Ch., Bromley H.J., and Por F.D., 1992. *Lake Hula, Reconstruction of the Fauna and Hydrobiology of a Lost Lake*. The Israel Academy of Science and Humanities, Jerusalem, 170 pp.
- Jones, R. F. (1940). Report of the Percy Sladen Expedition to Lake Huleh: A Contribution to the Study of the Fresh Waters of Palestine: The Plant Ecology of the District. *Journal of Ecology*, 28(2), 357–376. <https://doi.org/10.2307/2256234>

**C3:** L198: an illustration, similar as Fig.1 in Cécillon et al. (2018), could be useful to visualize how you cut/sum your signals.

**R3:** *Done. A figure was added to the SM, section 1.4. As the purpose of this manuscript is not method development, we have included this figure in the supplementary material. We added the CO curves to the original Fig. S4 and added vertical dashed lines to indicate the cutoff between organic and mineral carbon. The figure is cross-referenced in the revised manuscript.*

**C4:** L210: why consider only the CO<sub>2</sub> emitted during pyrolysis, and not CO? Same, why disregard oxygen emitted as both during oxidation? More generally, you chose to follow Behar et al. (2001)'s definition of oxygen index, taking CO<sub>2</sub>-carbon into account, rather than more recent definition focusing on oxygen only (with stoichiometric correction as in Cécillon et al., 2018; Saenger et al., 2013; Delahaie et al., 2023). Did you consider both definitions before choosing Behar's?

**R4:** *We agree with the reviewer. The text was updated according to Cécillon et al. (2018). The oxygen index (OI[RE6]) represents in the revised version the oxygen emitted as CO<sub>2</sub> and CO in the pyrolysis phase (200-400°C), corresponding to oxygen in the OM (L222). This update changed the absolute values of the OI, while the overall patterns were kept. Thus, our interpretation of the data had not changed.*

**C5:** L245: the explanation for why 22°C and 33°C could figure above, L231, so that we immediately understand why you chose these.

**R5:** *Accepted and implemented. L243.*

**C6:** L249: I think the equations should be rewritten formally so as to only contain variables, not mixed up with units. Describe the variables and their units above or below.

**R6:** *Accepted. The units for the flux and k are written near their definitions above the equations and were removed from the equations. Note that the units in the right side of Eq. 2 were left to explain the calculation. L263.*

**C7:** L261: The explanation as to why start at -30 cm and not above could be there instead of L277.

**R7:** *Done. L277.*

**C8:** L263: why isn't there a rewetted section in the cores A, C, and D? Perhaps I missed the explanation, but I don't understand why the rewetting seems to not have 'worked' everywhere.

**R8:** *This is because the water table post drainage and the water table post rewetting were similar in depth (gray and light blue shaded area, Fig. 2) in those cores, limiting the development of "rewetted sections". We clarify this in the revised manuscript. L280.*

**C9:** L288: usually, when talking about 'persistent', it is good to precise which duration you are referring to, as it cannot be forever: does it persist for decades? Centuries, millennia?

**R9:** *We agree with the reviewer. However, the persistence of the OM cannot be readily evaluated. Rather, we can state that the fraction of persistent OM remains stable for a duration much longer than the degradation period observed and modeled. That is, much longer than 66 years. We clarify this in the revised manuscript. L305 and in L520.*

**C10:** L356: there is debate on the significance of TpkS2, as the peak is not always related to the quantity of hydrocarbons evolved during the whole process (you can have a very short peak at the beginning

while most of the matter evolves later). Did you have a look at other indices, such as T90\_HC\_PYR, the temperature at which 90% of hydrocarbons have evolved (as described in Cécillon et al. (2018) for instance)?

**R10:** *We agree with the reviewer that this peak is not always correlated with the quantity of HC evolved, especially as the FID is not a symmetric Gaussian peak in soils and peat as in rocks. Specifically in this study, the FID signal of the pristine peat is composed of two overlapping peaks of similar size (Fig. 8). With degradation, the more labile peak diminishes at a higher rate. The shift in TpkS2 resembles this relative change, and hence we gave it as an example of how each peat section is geochemically distinguished.*

**C11:** L459: sentence unclear; perhaps the subscript disappeared, it would make more sense with it...

**R11:** *Corrected. '[' was missing. The corrected sentence is: "Since the experimental  $k$  constant is calculated for the entire SOC content, it must be corrected if  $SOM_p > 0$  according to the labile SOM [ $k$  corrected =  $k_{\text{experimental}} / (1-P)$ ]"*. L507.

**C12:** L485: Also, the priming effect would probably not extend much under the root depth, which you excluded by starting at -30 cm.

**R12:** *Agreed. We added clarification that in the model the top 30 cm were excluded, to minimize this potential effect, L353.*

**C13:** L493: did you consider radiocarbon analyses?

**R13:** *We did not consider radiocarbon analyses, and we are not certain that this will give a definite answer to the source of the persistent OM. As discussed in the original MS, the persistent OM can be generated with the chemical changes of the peat due to its decomposition. Even if it was there originally, as considered by the model, it can differ from the labile OM because of its interactions with minerals, or because of its chemical structure. In other words, the persistent OM can be of similar age as the labile OM.*

**C14:** L526: The PARTY<sub>SOC</sub> model has some limitations, even in its v2 form. In particular, soils with a high SOM should not be treated with this model, as it has never been trained nor tested on such data.

**R14:** *Indeed, those discrepancies may rise, as the PARTY<sub>SOC</sub> model was fitted to soils, which are not as organic-rich as the peat (Cécillon et al., 2021). Clarification was added. L576.*

### **C15: Technical corrections:**

General: check for grammar, non-verbal sentences, etc.

*Thank you. Revised throughout the revised manuscript.*

General: the abbreviation for gram is g, not gr.

*Updated.*

L313: 'purple' as in the caption, not light pink. Maybe homogenize the color with other figures...

*Done.*

## **Reviewer #2**

### **Overall view**

This study presents valuable and somewhat rare long-term information from the Hula Valley peatland in Israel, tracking almost seventy years of drainage followed by three decades of partial rewetting. It provides valid evidence that drainage leads to rapid carbon loss in warm-climate peatlands and that partial rewetting can slow this degradation. The data and methods are solid, but the manuscript would benefit from clearer terminology, a more careful interpretation of thermal indices, and a balanced discussion of uncertainties.

I believe it will interest readers studying soil carbon dynamics, restoration of degraded peat, and greenhouse gas mitigation. Still, some interpretations are too confident for the evidence provided, and a few sections need simpler explanations. In particular, the differences between thermal ‘stability’, biological ‘resistance’, and actual field-scale preservation should be made clearer. Some of the experimental assumptions and terminology e.g., ‘tropical’ and ‘warm-temperate’ also need amendment/correction.

*Accepted, rephrased throughout.*

### **Specific Comments**

#### **C1: Climate description**

The site is described several times as ‘tropical’. With an annual mean temperature of about 20 °C and ~ 600 mm precipitation, the Hula Valley should be better classified as warm-temperate or subtropical. The wording should reflect this to avoid confusion with true tropical peatlands.

*R1: We agree. The climate definition was updated where mis-clarification was evident, including change to the article title and short summary text.*

#### **C2: Definition of ‘pristine’ peat**

The deep layer is called pristine, but it has been under the same area for decades, with drainage above it. Downward oxygen diffusion or chemical change is possible. Please clarify whether redox or sulfur data confirm that it truly remained unaffected.

*R2: The OM content at the deep part is comparable to that of the peat pre-drainage. Moreover, the deep section has significantly lower sulfate content (see Fig. 3), confirming that oxidation of pyrite (and hence of OM), was limited. Hence, we conclude that oxidation of the deep peat is insignificant. Accordingly, L345-353 were updated to emphasize and clarify that our data indicate no significant sulfur speciation shifts in the deeper pristine peat, while in the intermediate, rewetted sections, accumulation of reduced sulfur species is evident. This suggests the downward flushing of oxidized SO<sub>4</sub> and concomitant reduction under anoxic conditions.*

#### **C3: Rock-Eval results and interpretation**

**C3a:** The Rock-Eval data are strong, but some interpretation goes beyond what this method can tell. Higher RI or TpkS2 values show greater thermal stability, not necessarily biological resistance. The discussion should separate these two ideas.

*R3a: Agreed. This was clarified in the revised version (L374-L384, L391-L394 and L562).*

**C3b:** The figure suggests some overlap in OI values between sections, so ‘significant differences’ should be checked.

*R3b: Accepted. We have revised to clarify more specifically which parameters indicate significant differences between sections, and which are not (OI) (L369-L373).*

#### **C4: Respiration experiments**

**C4a:** The incubation tests at 60 % water-holding capacity probably exaggerate natural respiration rates. The authors note this, but the limitation should be discussed more directly.

*R4a: This is a value commonly used for WHC, intended to be associated with the maximum respiratory rate. We acknowledge that in the field, the drained part is often drier, and sometimes saturated. We added an explanation to address this issue (L412-418).*

**C4b:** The result that rewetted peat produced the highest CO<sub>2</sub> flux is surprising. Possible reasons could be the temporary oxygen exposure, recent organic inputs, or sulfide oxidation but it should be acknowledged rather than presented as a steady condition.

*R4b: Agreed. A short discussion of this issue was added (L425-L430).*

**C4c:** The ARQ values below one likely reflects partial oxidation or iron/sulfur reactions; this is interesting but needs a sentence of explanation and a reference.

*R4c: This is indeed part of the explanation. L445 revised to further emphasize it.*

#### **C5: Two-pool SOM model and Persistent SOM fraction**

**C5a:** The model is appropriate and well fitted, but it assumes no new organic input and no loss from the ‘persistent’ pool. Because this area is farmed, these assumptions are not fully realistic. Please mention how they might affect the result.

*R5a: To minimize OM addition from the top surface, we excluded the 30 top cm from the sections. This assumption was emphasized and clarified, as well as the effect of OM addition from the surface in the revised version (L535).*

**C5b:** The estimate of 13- 21 % persistent SOM is plausible. However, the statement that other models like PARTYsoc are ‘inadequate’ sounds dismissive. It would be better to briefly explain why this method might not fit peat with high organic content. Plus, the metadata (codes and Excel files), show reasonable internal design/structure but mostly represent the drained condition; the distinct ‘rewetted’ and ‘pristine’ patterns claimed in the manuscript are not strongly evident here. Variability within the drained layer and method heterogeneity in historical SOM data could explain much of the reported differences.

*R5b: Agreed. Accepted, revised and clarified (L576). The limitation of the model and it's suitability are discussed. Regarding the heterogeneity comment, the range of the model's results are related to the degrees of freedom and input data. Yet, we accept the reviewer's comments regarding the natural heterogeneity and it's effect on our interpretation of data. Thus, the revised version includes a clear statement acknowledging natural variation due to sedimentary settings, and suggest where the thermal signals are strong enough to may provide additional information that we postulate is contributed to degradation processes (L370). In addition, we added a clearer referral in the text to the sampling statistical limitation that was incorporated in Fig 4 (now referred to in line L367). We acknowledge that it could have been beneficial to have had more profiles with clear distinction between sections. This issue was a main concern of us during the planning stage, sampling campaigns, and special attention was dedicated to it while carrying out the field work. Unfortunately, despite our prior efforts, the constrains posed by the historic data availability as well as the regional groundwater regime left us with limited spatial options. Additional clarifications were added to address this limitation.*

## **C6: Sulfur and pyrite**

The finding of pyrite peaks below the water table is a good indicator of anaerobic conditions. This part is important and could be discussed in simpler terms: it shows that rewetting successfully re-established reducing conditions.

*R6 Agreed and clarified (L343).*

## **C7: CO<sub>2</sub> emission comparison**

The conversion from lab fluxes to tons CO<sub>2</sub> ha<sup>-1</sup> yr<sup>-1</sup> involves bulk-density and shrinkage assumptions that vary widely. A short uncertainty range would make the comparison to IPCC factors more convincing.

*R7: Agreed. We have revised this section to clarify the calculations (L602), and included a new simplified figure and explanation to the Supplementary Material (1-10).*

## **C8: Other suggestions**

### **C8a Figures and tables**

Figure 1 should mention land use or rewetting zones.

*R: To clarify the issue raised by this comment, we added to the map an orange line marking the location of the underground hydrological barrier and the drainage canals. Those border the area in which the ground water table is largely affected by the current hydrological regime in the valley. L168.*

Color schemes for drained, rewetted, and pristine sections should be consistent across all figures.

*R: We tend to agree with this comment. However, after careful review, we have decided to retain the current color schemes to better serve the distinct narratives of the manuscript's sections. Figures 2–5 focus on thermal stability and bioavailability, using a color code designed to distinguish between specific parameters. In contrast, Figures 6–8 place the results in a spatial context (bulk organic matter), utilizing a depth-based color gradient. We believe maintaining these distinct schemes provides the best clarity for the specific data types presented in each section.*

Table 2 could list the exact loss-on-ignition temperature used in each historical dataset.

*R: The table lists the Methods by numbers (i.e., Method #1-3). We chose this approach rather than to simply specify LOI temperature since in some cases methodological manipulations were carried as well. Thus, the details of known ignition temperatures and methodological manipulations are stated in short at the bottom of the table. L467.*

### **C8b: Typo/grammar notes**

Typo line 120: 'hat two main objectives' should be 'had two main objectives.'

*R: L124 Corrected.*

Ensure consistent units (wt %, °C, etc.).

*R: Revised throughout.*

Verify all references, especially Manzoni & Francesca (2024) because this looks incomplete.

*R: L106 revised.*

Some very long sentences in the discussion section should be shortened for easier reading.

*R: Revised.*