

We thank the reviewer for his important comments, that will help to improve the manuscript. Below is the reviewer comments (in black) and our response and the action we will take (in blue).

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.

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-evaluate 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 limitation, produced valuable and quantifiable data for discussing peat degradation and CO₂ emissions.

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>**

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- Marchand C, Lallier-Vergès E, Disnar JR, Kéravis D. Organic carbon sources and transformations in mangrove sediments: a Rock-Eval pyrolysis approach. *Organic Geochemistry*. 2008 Apr 1;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:

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

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 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 water table was found 0.5-1 m below 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 km² in summer (August-September) and ~45 km² during the winter floods (January-March). Relevant data added in the manuscript, L132.

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>

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

Done. As, the purpose of this manuscript is not method development, we add 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 corrected manuscript.

L210: why consider only the CO₂ 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 CO₂-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?

We agree with the reviewer and will consider the CO in the total OI of the revised manuscript.

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

We will add these explanation there.

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.

Done. 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.

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

Done.

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.

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 (L262): "A rewetted section is absent from the other cores as the WTL's post-drainage and post-rewetting were very similar (Fig. 2, purple and light blue shaded areas)".

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?

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 (L288): "a persistent SOM pool (SOM_p), with decomposition rates that are significantly lower than the observed duration of degradation for the labile pool". And in L471: "Given the degradation duration of the labile SOM was of the order of 70 years, we estimate that the persistent SOM should be stable for several centuries given similar environmental conditions".

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)?

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 how each peat section is geochemically distinguished.

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

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 [$k_{corrected} = k_{experimental} / (1-P)$]"

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

We agree with the reviewer. The following sentence was added to the revised manuscript L485: "Furthermore, in the model the top 30 cm were excluded, minimizing this potential effect".

L493: did you consider radiocarbon analyses?

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.

L526: the PARTYSOC 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.

Agree, in fact our result agree with this.

Technical corrections:

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

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

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

All technical correction will be done.

References:

- Behar, F., Beaumont, V., and Pentead, H.L.: Rock-Eval 6 Technology: Performances and Developments, Oil & Gas Science and Technology – Rev. IFP, Vol. 56 (2001), No. 2, pp. 111-134, <https://doi.org/10.2516/ogst:2001013>, 2001.
- Cécillon, L., Baudin, F., Chenu, C., Houot, S., Jolivet, R., Kätterer, T., Lutfalla, S., Macdonald, A., van Oort, F., Plante, A. F., Savignac, F., Soucémariadin, L. N., and Barré, P.: A model based on Rock-Eval thermal analysis to quantify the size of the centennially persistent organic carbon pool in temperate soils, Biogeosciences, 15, 2835–2849, <https://doi.org/10.5194/bg-15-2835-2018>, 2018.
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- Saenger, A., Cécillon, L., Sebag, D., & Brun, J. J. (2013). Soil organic carbon quantity, chemistry and thermal stability in a mountainous landscape: A Rock-Eval pyrolysis survey. Organic Geochemistry, 54, 101-114.