We deeply 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).

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.

Specific Comments

1. 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.

We agree, and the climate definitions will be changed accordingly.

2. 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.

Firstly, the OM content pre-drainage was homogenous throughout the peat column. Further, the OM content we found at the deep part is comparable to that of the peat pre-drainage. In addition, the deep section has minimal sulfate and high reduced sulfur concentrations (see Fig. 3), reinforcing that oxidation of pyrite (and hence of OM), was limited. Hence, we conclude that oxidation of the deep peat is insignificant. This will be further clarified in the revised version.

3. Rock-Eval results and interpretation

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.

We agree with the reviewer and this will be clarified in the revised version.

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

We agree with the reviewer and this will be clarified in the revised version.

4. Respiration experiments

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.

We will explain this choice of WHC value more clearly in the revised version. This is a value commonly used, intended to be associated with the maximum respiratory rate. Of course, at the field, the drained part is often drier, and sometimes saturated.

The result that rewetted peat produced the highest CO_2 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.

Agree. We will discuss this further in the revised version.

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.

This is indeed part of the explanation. We will discuss this further in the revised version.

5. Two-pool SOM model and Persistent SOM fraction

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.

To minimize OM addition from the top surface, we excluded the 30 top cm from the sections. We will discuss the validity of this assumption, as well as the effect of OM addition from the surface in the revised version.

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.

We agree with the reviewer and this will be clarified in the revised version.

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.

We don’t understand this comment, please clarify.

6. 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.

We agree with the reviewer and this will be clarified in the revised version.

7. CO_2 emission comparison

The conversion from lab fluxes to tons CO_2 ha^{-1} yr^{-1} involves bulk-density and shrinkage assumptions that vary widely. A short uncertainty range would make the comparison to IPCC factors more convincing.

We agree with the reviewer and this will be clarified in the revised version.

Other suggestions

Figures and tables

Figure 1 should mention land use or rewetting zones.

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

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

The figures will be modified in the revised version.

Typo/grammar notes

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

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

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

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

Thanks and Good luck.
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