Assessing the impact of rewetting agricultural fen peat soil *via* open drain damming: an agrogeophysical approach

Author response to reviewer comments.

All author comments are highlighted below in this colour.

Page and Line numbers referenced by the reviewers refer to the original version of this manuscript

Page and Line numbers references in this response by the Authors refer to the revised version of this manuscript.

Note that other spelling and typographical errors have been corrected throughout the revised text, and not all of these are captured in this response to reviewers.

The authors wish to thank the Executive Editor, Topic Editor and both reviewers for their valuable input and time in the production of this manuscript.

Reviewer 1:

The manuscript entitled "Assessing the impact of rewetting agricultural fen peat soil via open drain damming: an agrogeophysical approach" by O'Leary et al. evaluates how the implementation of open drain damming affects the hydrology of a fen peat site. It is shown that the interpretation of sparse information from wells and SWC monitoring stations provides little insights, but that the spatially continuous nature of electromagnetic induction (EMI) measurements (an important agrogeophysical tool) provides important insights on the limited extent of the rewetting impacts of such damming activities. This is achieved through an advanced cluster analysis of the EMI data, followed by an inversion to obtain typical EC profiles with depth for the identified clusters. Overall, I found this to be an interesting case study highlighting the added value of agrogeophysical measurements in a peat hydrology context. Below I have provided specific comments that should be addressed in a revised version. Although not considered in my evaluation, I would say that the quality of the writing can also still be improved. I recommend to avoid very short paragraphs, and I would like to ask the authors for a careful proofreading before submitting the revised manuscript.

Many thanks to this reviewer for their comments. A full review and proof reading will take place prior to final submission. Specific comments are dealt with in order below.

SPECIFIC COMMENTS

Figure 1. I wonder whether this figure is not too basic for the readership of SOIL? It seems like textbook material to me. Is it critically important for the narrative to explicitly address the different types of bog?

While the authors acknowledge that the diagram may be simplistic in nature, the various peat soil types remain ambiguous and relatively unknown across the soil community at large. Reviewer two notes "Nice introduction; coming from someone with barely any knowledge on peat soils". Considering both reviews, the authors have decided not to remove the current Figure 1, specifically as it highlights where "fen" peats are in the overall peat soil landscape, which are of particular importance for agricultural peat studies, which typically take place on Fen peats.

Line 87. Remove "… that …".

Removed

Line 110. There is a range of studies dealing with time-lapse EMI measurements. The perspective seems to be a bit too narrow here.

Line 108 - 112: Text updated and references added to highlight the importance of timelapse EMI surveys to agricultural hydrological dynamics.

"The possibility of analysing EMI survey results in the temporal domain via multiple repeat surveys has allowed for the assessment of, for example, hydrological dynamics in agricultural settings (Blanchy et al., 2020; Moghadas et al., 2017), especially in areas where there is little change from other external sources (land management, soil textural changes) (Boaga, 2017), but has not been explored in the context of agricultural grassland peat soils and rewetting."

Line 136. Are the 10 wells only open at the bottom (piezometer), or are they filtered along the entire length of the tube.

The Dip-wells are fully screened with 3 mm slots and are suitable to ascertain the true water table level. The text is updated to reflect this better,

Line 137 - 138: "…this field has been instrumented with ten × 2 m deep fully-screened dip-wells (Baird and Low, 2022)…

Line 137. Please provide type and manufacturer of these SWC probes (if presented later in the manuscript). I also could not find the location of these sensors in Figure 2a.

Probe manufacturer is provided in the reference VanWalt 2025b and added to the text. The location of these probes is next to a well in the Normal and Rewet areas. The location of the VWC probes has been added to Figure 2

Line 139: "…gropoint probes (Vanwalk, 2025b)…"

Line 164. I recommend to not use bulk in this context. It is typically reserved for the electrical conductivity of a mixture of materials (here: water, air, organic matter). I would prefer the introduction of the classical terminology here (i.e. “apparent electrical conductivity”).

Line 166: updated to “ECa refers to a measurement of apparent electrical conductivity…..”

Line 230. This statement only makes sense if your tubes are only open at the bottom. If the tubes are filtered (have slits), this would not make much sense to me. Please note that tubes that are open at the bottom do not indicate the position of the water table but instead the pressure potential at the opening. Please clarify your situation.

The dip wells are fully screened from base of the peat to the base of the well. As such, W1 reflects a composite WTD across peat and mineral substrate and not the peat water table specifically, so it was excluded from peat-layer WTD analysis. We followed the work of Baird & Low (2023) with respect to our dip well design.

Line 245 – 248: “The exception to this is W1, which shows a deeper WTD. This well is fully screened from the peat through to the underlying mineral subsoil, likely capturing a composite water level dominated by the substrate, rather than the peat layer and is therefore is removed from analysis.”

Figure 3. A volumetric water content of 100% is confusing. Please clarify what is reported here. Does 100% indicate pure water here. Or do you mean saturation in terms of filled pore space? This would not be a volumetric water content anymore.

We confirm that we are measuring Volumetric Water Content using Gropoint probes: https://www.gropoint.com/products/soil-sensors/gropoint-profile. However, the maximum VWC measured is 99.9% (clarified in the updated text). Peat soils in general, and as measured at some similar sites in Ireland, have porosities up to 96+%, and very low bulk densities (0.01 g/cm^3) therefore VWC measurements in gropoint sensors can read as high as 99.9% as shown in our results where the soil is fully saturated. However, we note that the presented results are a daily averaged reading from these probes with a standard deviation of between 1 and 2.5% (as reported in the text).

Line 139: Added definition of VWC for clarity “…the percentage of water volume…”

We have updated the text removing the term “maximum saturation” and replacing it will “full saturation”

Line 265 - 267: Added “Full saturation of the peat soils is achieved and measured as an average VWC of 99.9 %, indicative of high porosity, low bulk density peat soils being fully saturated with water (Galvin, 1976).”

Additionally, the figure 3 caption has text added to read:

“Note that full saturate refers to a reading of 99.9 % VWC indicative of a fully saturated peat soil.”

Table 2. Please clarify whether the reported EC has already been corrected to a standard temperature. If not, can the difference in EC be explained by temperature only? Typically, 2% per degree is assumed for water, which would be a difference by 14%. The measured difference seems to be bigger. However, the salinity seems to be constant. What is then the cause of the remaining difference in EC? I think some more discussion and reflection is warranted here.

The EC reported for the open drain water is not corrected to a standard temperature. This is due to the fact that we are correlating these reading to the EMI survey, which was also not corrected standard temperature. We agree that 2 – 3 % change of EC per deg C would result in 14 – 21 % difference in EC between the survey dates, which report a ~26 % drop in EC between the June and December surveys. We have updated the text in the associated paragraph to read:

Line 288 - 291: “Similarly, the electrical conductivity of the open drain water in December is ~ 20 mS/m lower than during the June survey, most likely attributed to the temperature difference between the dates (Corwin and Lesch, 2005) along with any changes in the composition of the open drain water due to run off upstream. This value was not corrected to a standard temperature in order to compare to the respective EMI survey.”

Line 320. At some point, I would like to see a clear statement that relates the clusters to the area affected by rewetting measures.

Line 323 - 325: Added text to “Cluster 1 is located in the area of higher ECa noted in both summer and winter surveys close to the dam and flume (Figure 4), which is considered the area affected by rewetting measures.”

Line 379. The challenge with inversion is that EMI measurements need to be calibrated to obtain consistent inversion results. How was this addressed here?

We agree with the reviewer’s comment regarding the challenge with EMI inversion due to calibration of the instrument to a known distribution of ECt in the subsurface or other calibration methods. Apparent (ECa) data are commonly used to infer spatial patterns (Brogi, 2019) without calibration. In this study, by applying unsupervised clustering to group the apparent (ECa) data and performing a relatively simple 1D inversion on representative data points from each cluster we provide representative ECt distribution across this site, allowing us to explore spatial trends without requiring a full calibration procedure. While no in-field calibration was performed, rigours start up procedures were followed to ensure the data integrity by allowing the instrument to fully warm up and by correcting for the presence of the sled. Additionally, the CMD instrument is factory calibrated. To address this in the text the following changed were made:

Line 189: Added "…is factory calibrated and…"

Line 198: Added ". No in field calibrations were performed."

Line 399 - 400: Added "…due to volume of data to be inverted and restrictions in performing in-field calibration of EMI instruments,…

Line 402 - 403 Added "The use of representative 1D ECa data (cluster centres) simplifies the inversion process and still provides representative distribution of ECt across this site."

Line 385-391. This paragraph needs to be improved. Argumentation currently is not fully clear to me.

This paragraph has been reworded to, which we find to be more descriptive of our argument that infiltrated open drain water, where EC is known, may be different from that of natural ground water, and this difference can be exploited to help interpret the results of this EMI survey:

Line 411 – 421: "One of the assumptions of rewetting is that by damming water in the open drains this water will infiltrate into the surrounding soils, effectively rewetting them (Heller et al., 2025). Therefore, it can be assumed that some physical properties of this open drain water will be present in the water content of these infiltrated soils, specifically electrical conductivity. In this study, the inclusion of the Multi-Parameter probe measurements of open drain water electrical conductivity (Table 3) has provided a constraint when interpretating EMI survey results. via a measured ECt for water which is assumed to have infiltrated into the soil. This value is compared to the inverted cluster centre ECt profiles to highlight which areas of the site are impacted by this infiltration. The inclusion of these data, or similar, measurement of open drain water electrical conductivity should be included with using EMI measurements to determine the effect of drain damming on agricultural fen peat soils."

Line 403. Should only water content be considered here, or should the electrical conductivity of the pore water also be considered? I am not sure that it can safely be assumed that the water in the open drain matches the pore water in the soil. A more in-depth reflection would be appreciated here.

We understand the reviewer's question at this point and believe it stems from a poorly written argument in this section of text. We have updated this to reflect our point that in the December the landscape level ground water has more influence on the electrical conductivity of the pore water in the peat soils than the open drain water as it is naturally shallow, whereas in the June survey, with a deeper landscape ground water level, the open drain water can infiltrate into the peat soils, meaning a high proportion of the pore water in the peat soils in Cluster 1 is from the open drain, according to our analysis. See updated text below:

Line 426 - 433 : “During the June survey, the WTD in the field is deeper, thereby creating a large gradient between the in-field WTD position and the open drain water level. Water from the open drains infiltrates into the subsoil of the adjoining field, immediately around the dam and flume, resulting in electrical conductivity readings being closer to that of the water in the open drain. While this effect is still present in the December survey, it is not as obvious. This is due to the shallower WTD in winter, and so less infiltration of the water in the open drain into the surrounding soils. This would result in the pore water content being proportionally more ground water influenced, resulting in the electrical conductivity (Henrion et al., 2024), of the peat soil layer to be more uniform in winter across the site, as observed in this study.”

Reviewer 2:

This manuscript presents a multi-method assessment of the hydrological effects of drain ditch damming on fen peat soils. The authors combine well-based water table observations, volumetric water content (VWC) profile measurements, and electromagnetic induction (EMI) surveys to characterize spatial changes following dam installation. The manuscript is well-structured and flows nicely, with a particularly accessible introduction that provides useful context for readers unfamiliar with peatlands.

The study demonstrates the potential of EMI surveys in combination with clustering analysis to distinguish hydrological zones within the field site and to derive vertical electrical conductivity (EC) profiles through inversion (based on the use of multiple coil spacings, and the clustering). This approach enables better spatial coverage than traditional point measurements. However, the data from wells and soil moisture probes located upstream (W / "Rewet") and downstream (D / "Normal") of the dam showed no measurable difference in water table depth or soil moisture content, suggesting that the hydrological influence of the dam is spatially limited, likely confined to a few meters around the drain itself.

Overall, the manuscript effectively illustrates the added value of EMI in identifying spatial hydrological patterns, the potential for repeated EMI surveys, and the derivation and interpretation of vertical EC profiles. Nonetheless, several points require clarification or further discussion.

Many thanks to this reviewer for their detailed review and additional commented pdf. We attach a similar PDF with comments addressed and specific comments are dealt with in order below.

Some general comments or questions that I have after reading the manuscript:

- The EMI surveys are said to be conducted in “summer” and “winter,” yet the dates provided are 26/06/24 and 10/12/24. The former seems more representative of late spring, and the latter of late autumn/fall rather than winter. This mislabeling could mislead readers about hydrological conditions. When I think of winter, I think of very high water tables, filled drain ditches, and saturated soils in January/February. I recommend adjusting the terminology, e.g., late spring and fall.
    - Thank you for highlighting this potential confusion. To be more specific, we have changed all relevant instances or references to “summer” and “winter”

to “June” and “December”, removing any ambiguity as to when these surveys took place.

- The study would benefit from a brief overview of meteorological conditions in the study region during 2024. Was the year, spring, summer particularly wet or dry? This context could help readers interpret the water table, EMI and VWC findings.
    - This context has been added to the revised text. Additionally, the section titled “Rainfall data” has been moved to earlier in the text as suggested.
    - Text added “Generally, the meteorological condition in Ireland reported that the spring and summer were cool and dry (Met Eireann, 2024) and that autumn and winter were mild with below average rainfall (Met Eireann, 2025)”
- The height of the dam and the corresponding change in water level in the drain ditch (before vs after installation, upstream vs downstream of the dam) are not reported. These data are essential for understanding the potential impact of the dam.
    - Thank you for this comment. We agree that the water level above and below the dam are indeed important factors in understanding the impact of the dam. However, on this site, the WTD is largely controlled by landscape ground water fluctuations as demonstrated by our dip-well analysis. Additionally, our EMI analysis is not attempting to delineate a WTD depth across the site, merely the presence of infiltrated water from the open drain, which is effectively demonstrated. We argue that this is evidence of impact of the dam, hence no measurement of the water levels in the open drain were accurately taken. A visual check on both survey dates estimated that there was between 20 and 40 cm difference in the water levels above and below the dam on both survey date, however we did not include this in the article as it is not accurate and not relevant to the results presented.
    - No changes to the revised text.
- The manuscript refers to two 1.2-meter deep VWC profiles, but their locations are not clearly indicated in Figure 1 or the text. From the results (Figure 3), one appears to be in the Rewet area and the other in the Normal area, but this should be clearly stated and shown in the figure legend or map annotation.
    - Figure 2a now highlights the location of the 2 VWC probes
- It is unclear how VWC is defined in the study. In standard usage, volumetric water content is the volume of water divided by total soil volume (m^3/m^3 or %). In Figure 3, values up to 100% are shown, which seems too high, even for peat. If this is a normalized or relative VWC (e.g., relative to porosity or saturation), that should be clearly defined throughout the text and on all relevant figures. Without clarity on this, interpretation of Figure 3 becomes difficult.

- We confirm that we are measuring Volumetric Water Content using Gropoint probes: https://www.gropoint.com/products/soil-sensors/gropoint-profile. However, the maximum VWC measured is 99.9% (clarified in the updated text). Peat soils in general, and as measured at some similar sites in Ireland, have porosities up to 96+%, and very low bulk densities (0.01 g/cm^3) therefore VWC measurements in gropoint sensors can read as high as 99.9% as shown in our results where the soil is fully saturated. However, we note that the presented results are a daily averaged reading from these probes with a standard deviation of between 1 and 2.5% (as reported in the text).
- Line 139: Added definition of VWC for clarity "…the percentage of water volume…"
- We have updated the text removing the term "maximum saturation" and replacing it will "full saturation"
- Line 265 - 267: Added "Full saturation of the peat soils is achieved and measured as an average VWC of 99.9 %, indicative of high porosity, low bulk density peat soils being fully saturated with water (Galvin, 1976)."
- Additionally, the figure 3 caption has text added to read:
    - "Note that full saturate refers to a reading of 99.9 % VWC indicative of a fully saturated peat soil."

- Both EMI surveys were conducted after the dam was already in place. Given this, I find it unclear how the EMI data can be used to infer the impact of damming unless a clear temporal difference is observed that can be linked to the dam. Without baseline (pre-dam) EMI data, attributing changes solely to damming remains speculative, I would say.

  The observed zone of elevated electrical conductivity (ECa) near the dam is interpreted as a result of (increased) soil moisture from the drain ditch due to damming. However, could this signal also result from other factors such as soil compaction, increased iron or salt concentration, or historical management effects? It would be helpful if the authors could discuss alternative explanations and, if possible, provide supporting data (e.g., soil chemistry or structure observations).

    - Thank you for this relevant comment. We acknowledge that this point was missing from our study however and have updated the the text to address this shortcoming. As mentioned by the reviewer, other factors (e.g., iron, salinity, compaction) may contribute to ECa variation, no evidence of such was observed. For example, there was no water trough in this area, which may indeed lead to increase compaction from cattle. While we acknowledge the lack of pre-dam EMI data limits definitive conclusions; we argue that the consistent ECa spatial patterns between the two survey dates combined with the differences in the vertical distributions of ECt strongly suggest hydrological change as the main influence on the EMI data. Additionally, the position of the dam and flume provide a plausible explanation for the results.

  - Line 407 - 410: Added "While, this would be particularly impactful for an EMI survey performed prior to and after dam installation, the methodology and results of this study effectively demonstrate the ability of temporal EMI surveys to identify areas of hydrological change. It should be noted that other soil characteristics which change on similar timescales, such as compaction, may influence temporal EMI survey results, however no evidence of additional influencing factors (e.g., iron, or saline intrusions) are present in this study."

- The discussion does mention the potential use of EMI prior to dam installation to optimize rewetting strategies. While this is an interesting idea, it is not demonstrated or supported by the current study. If this is a forward-looking statement, I suggest clearly framing it as a suggestion for future work, rather than a direct conclusion from the current results. I also do not fully understand how prior knowledge from EMI measurements might optimize the rewetting?
  - Thanks for your comment. Indeed, the argument is for the inclusion of EMI surveys in future work. The text has been updated as below.
  - Text was removed which related to how an EMI may have optimised the dam location on this particular site
  - Line 439 - 441: Text added "EMI surveys in combination with peat depth via probe campaigns should be done prior to any rewetting efforts as they can cover large areas quickly and can yield spatial information on peat soil characteristic changes."
- There are some inconsistencies in capitalization. Please ensure consistent formatting throughout.
  - Thank you for the detailed read through and highlight this. The text has been reviewed in full and inconsistencies removed.
- I assume the abbreviations D & W originally come from 'dry' and 'wet' while the 'Normal' (D) areas are not really dry. D is a weird letter for 'Normal' areas; consider calling it the control (C) area?
  - Thanks for this observation and indeed we did originally name the nested dip wells, D and W for precisely the reason you highlight. We have updated the text to show either "Control" or "C" in place of "Normal" or "D"

I have included further detailed comments, questions, and suggested edits in an attached pdf version of the manuscript. E.g., some figures are not colorblind-friendly, and are difficult to read due to a small font size.

The referenced PDF has been attached along with these responses and each comment has been addressed.

In summary, this manuscript addresses an important question regarding the spatial effectiveness of peatland rewetting through drain ditch damming. The integration of EMI into such assessments seems promising, but needs more clear statements on how and why. I hope the authors will find my comments constructive and take them into consideration during revision. I look forward to seeing the updated version.