Response to comments from referee 2

> **[R2-1]** *This paper presents a very interesting approach to a common problem when interpreting field*
> *measurements of redox potential. What do they actually mean in the chemical world out there? The*
> *authors have put thorough work in modelling the interactions in their system and present an*
> *explanatory model that is of great inspiration to the readers. They actually use another research*
> *question, in which they want to identify the effects of a single disturbance of the system in time, but they*
> *do come back to the focus in the title of the paper. Interpreting redox potentials from field sites is not*
> *easy and I want to thank the authors for the big effort in it.*
> *This paper contributes well to the scientific knowledge in this field. I highly recommend publishing, but*
> *I would also like to challenge the authors to improve and/or clarify some parts of the manuscript. This*
> *mostly refers to the documentation of the field work and disturbance.*

[Response to R2-1]

We sincerely thank the referee for the positive and encouraging assessment of our manuscript. We are grateful that the referee found the mixed-potential interpretation and explanatory model interesting and potentially useful for interpreting field measurements of redox potential. We also appreciate the constructive suggestions to improve the documentation of the field setting and disturbance. In response, we have revised the manuscript to clarify the site description, channel-construction history, pumping conditions, leaf-litter treatment, and the interpretation of the disturbance-related EC, DO, and Eh dynamics.

> **[R2-2]** *Fig 2: It appears from the text that a side channel was dug. This is not clear from the*
> *image (a). Can you please clarify which part of the water system was dug out when? Also, please*
> *add an arrow/indicator of point of view of the photographs (b, c). Is/was the lower channel a*
> *closed system/pipe, or an open side channel? The pumping was intermittent. What was the*
> *frequency? Was the pumping slow, thus not disturbing the water visibly, or was this on such a*
> *scale that the water became turbid again, with sediments like in photo c (which might influence*
> *the vertical profile). The subscript to Fig 2 mentions the leaf litter in the system. This litter is a*
> *large input of organic matter and thus microbial activity, influencing the Eh. Was this litter*
> *removed at any stage? Or was it left to decompose?*

[Response to R2-2]

We thank the referee for these helpful comments. We agree that the original Fig. 2 and its caption did not make the channel configuration, excavation history, and photograph viewpoints sufficiently clear. In the revised manuscript, we clarified that the lower channel was an open peripheral side channel, not a closed pipe, and that the channel-construction works were conducted in two stages: an initial manual excavation in August 2022, followed by a larger-scale mechanical excavation in May--June 2023.

We also added viewpoint markers to Fig. 2a to indicate the approximate viewing directions of the photographs shown in Fig. 2b and 2c.

Regarding pumping, we clarified that water was pumped intermittently during daytime using a solar-powered system. The available pumping-volume measurements indicate variable daily pumping, with a mean of 499 L day^{-1} during 26 August - 14 September 2022 and a range of 124 - 1300 L day^{-1}. The short-term on/off frequency of the pump was not continuously logged. We also clarified that the visibly sediment-laden inflow shown in Fig. 2c was observed after the June 2023 excavation and was associated with freshly disturbed soils.

Regarding leaf litter, we clarified that leaf litter was not actively removed during the monitoring period and was left to decompose within and around the pond. We agree that this organic-matter input may influence microbial activity and Eh, and this point is now stated explicitly in the site description.

[Changes in manuscript]

We revised Sect. 2.1 to clarify the treatment of leaf litter, the open-channel configuration, the timing of the excavation works, and the intermittent nature of pumping. The revised text now states:

> Leaf litter was not actively removed during the monitoring period and was left to decompose within and around the pond.

and:

> To promote water circulation, the open peripheral side channel shown in Fig. 2a was constructed in two stages: an initial manual excavation in August 2022, followed by a larger-scale mechanical excavation in May--June 2023. After channel construction, water was pumped intermittently (daytime only) using a solar-powered system. Pumping volumes measured during 26 August - 14 September 2022 averaged 499 L day^{-1} (range: 124--1300 L day^{-1}). The short-term on/off frequency of the pump was not continuously logged. Following the June 2023 excavation, sediment-laden inflow from freshly disturbed soils was observed to enter the pond.

We also revised Fig. 2a by adding viewpoint markers for photographs (b) and (c), and revised the Fig. 2 caption to clarify that the channel was an open peripheral side channel and that the markers indicate the approximate viewing directions of the photographs.

> **[R2-3]** *Line 80. Possibly out of scope, but have the authors found any consistency in the length*
> *required for the reading and the disturbance/stability of the water flow?*

[Response to R2-3]

We thank the referee for this comment. We agree that the time required for the readings to stabilize could potentially provide useful information about local flow stability or disturbance intensity. However, stabilization time was not recorded systematically during the monitoring period, and we therefore cannot quantitatively evaluate its relationship with water-flow stability or disturbance conditions.

> **[R2-4]** *Line 102. Please explain and/or add reference to "partial polarization conductance". This*
> *term is not a common term. line 135. Another explanation lies in the leaf litter on site that is being*
> *degraded in the water, releasing particles contributing the EC.*

[Response to R2-4]

We thank the referee for pointing this out. We agree that "partial polarization conductance" is not a common standalone term and should be defined more explicitly. In the revised manuscript, we define G_k as the local polarization conductance of partial reaction k, i.e., the slope of the partial current--potential curve near the equilibrium potential E_k . We also clarify that this quantity is equivalent to the reciprocal of the local polarization resistance for that partial reaction. The word "partial" refers to the partial current associated with each interfacial reaction in the mixed-potential formulation.

In addition, we revised the corresponding derivation in Appendix A to use the same terminology consistently and to clarify the small-overpotential, near-linear polarization approximation under which the conductance-weighted expression for E_{mix} is obtained. We also revised the description of the second term in Eq. (5) from a "kinetic reweighting contribution" to a more general "reweighting contribution", because changes in the relative polarization conductances may reflect changes in the relative contributions of the partial reactions.

[Changes in manuscript]

We revised the definition following Eq. (3) as follows:

where $G_k \equiv (\partial i_k/\partial E)_{E=E_k}$ is the local polarization conductance of partial reaction k, defined as the slope of the partial current--potential curve near E_k; equivalently, G_k is the reciprocal of the local polarization resistance.

We also revised Appendix A to define G_k consistently as the local polarization conductance of partial reaction k, and to state explicitly that the mixed-potential expression is obtained by evaluating the linearized current balance at $E = E_{mix}$ and imposing the zero-current condition under the small-overpotential approximation.

Finally, we revised the explanation of Eq. (5) as follows:

> Equation (5) decomposes the sensitivity into (i) a weighted sum of Nernst contributions, through $\partial E_k/\partial x$, and (ii) a reweighting contribution that arises when the relative polarization conductances, and hence the relative contributions of the partial reactions, change with x.

The referee also noted that degraded leaf litter may have contributed to EC. We agree that leaf-litter decomposition can contribute to background ionic and organic inputs and may influence microbial activity and Eh. We have clarified in Sect. 2.1 that leaf litter was not actively removed and was left to decompose within and around the pond. At the same time, the EC anomaly discussed in Sect. 3.2 was a synchronous, pronounced increase across sites following the May-June 2023 mechanical excavation, and we therefore used it as an operational proxy for the excavation-related physical disturbance rather than as a direct or exclusive measure of redox-network reconfiguration.

> **[R2-5]** *Fig 3. It seems the authors assume EC to be stable throughout the year ("Baseline Level").*
> *That would however be exceptional. In any system outside there are variations caused by the*
> *season. Is the "baseline mean" (line 136-137) the average, or the seasonal trend? "pooled"*
> *assumed that all values were taken to calculate an averaged mean. The figure also indicate the*
> *trend line. Please explain how that was calculated.*

[Response to R2-5]

We thank the referee for pointing out this ambiguity. We agree that EC in outdoor systems can show seasonal variability, and we did not intend to assume that EC remains stable throughout the year. In the original manuscript, “baseline” referred to a descriptive pre-disturbance reference range rather than a fitted seasonal trend.

To clarify this point, we revised the text to specify that the reference range was calculated from all EC observations at all sites before completion of the excavation works. We also revised the Fig. 3 caption to state explicitly that the horizontal solid lines and shaded bands represent the pooled pre-disturbance mean and mean ± 1.96 SD range, respectively.

[Changes in manuscript]

We revised the relevant sentence in Sect. 3.2 as follows:

> Nevertheless, EC during June--August 2023 clearly exceeded the pre-disturbance reference range: values were above the pooled pre-disturbance mean and outside the mean ± 1.96 SD range calculated from all EC observations at all sites before the completion of the excavation works (7 July 2022 - 7 June 2023).

We also revised the Fig. 3 caption as follows:

> Horizontal solid lines and shaded bands indicate the pooled pre-disturbance mean and mean ± 1.96 SD range, respectively, calculated from all observations at all sites before completion of the excavation works (7 July 2022--7 June 2023).

> **[R2-6]** *Fig 3. Please consider adding the year to the date (x-axis).*

[Response to R2-6]

We thank the referee for the suggestion. We have added the year to the x-axis labels in Fig. 3 to improve clarity.

> **[R2-7]** *Line 138. A1 is at the same distance of the inlet as A0 (see fig 2). This line however states*
> *something else. Please explain. Also, please consider depth, and water flow. Were those different*
> *between the two points?*

[Response to R2-7]

We thank the referee for pointing this out. We agree that the original wording was ambiguous. Our intention was not to state that Site A1 was closer than Site A0 in terms of simple geometric distance from the inlet. Rather, we intended to indicate that A1 was more directly exposed to the main circulation pathway and sediment-laden inflow, whereas A0 was located in a more hydrodynamically sheltered part of the pond geometry.

The water depths at A1 and A0 were broadly similar, typically approximately 40-45 cm, although they varied seasonally during dry periods. Therefore, we do not interpret the stronger EC response at A1 as primarily reflecting a difference in depth, but rather as reflecting differences in hydrodynamic exposure associated with the pond geometry. We have revised the text to clarify this point.

[Changes in manuscript]

We revised the sentence in Sect. 3.2 as follows:

The largest increase was observed at Site A1, which was more directly exposed to the circulation pathway and sediment-laden inflow than Site A0, whereas the response at Site A0 was less pronounced, consistent with partial hydrodynamic shielding by the pond geometry rather than a difference in water depth (Fig. 2a).

> **[R2-8]** *Line 149. Please add reference to Wolfram here.*

[Response to R2-8]

We thank the referee for this suggestion. We have clarified the software environment used for the peak detection by adding the Mathematica version and Wolfram Research information in the relevant sentence.

[Changes in manuscript]

We revised the sentence in Sect. 3.2 as follows:

> Local maxima in the likelihood profile were identified using the FindPeaks function in Wolfram Language (Mathematica 12.0; Wolfram Research, Champaign, IL, USA).

> **[R2-9]** *Line 156: If O2 increases due to water flow, the water flow must have been very turbid.*
> *This seems unlikely. A more common explanation is O2 production (and consumption) in the*
> *water. The authors state that there was no plants growing, but it can be assumed a biofilm was*
> *present during the experiment as the water was filled with leaf litter and the photos indicate a*
> *living environment.*

[Response to R2-9]

We thank the referee for this helpful comment. We agree that the original wording attributed the DO increase too narrowly to inflow-related mixing. Our intention was not to imply that the DO increase necessarily required strong turbidity or sediment resuspension. In a shallow pond, inflow and pumping events may enhance air-water gas exchange and vertical exchange, but we agree that biological oxygen production and consumption, including processes associated with biofilms and organic matter, may also contribute to the observed DO dynamics. We have revised the text to avoid attributing the DO increase to a single mechanism. The revised wording now acknowledges both physical and biological contributions as possible explanations.

[Changes in manuscript]

We revised the sentence in Sect. 3.2 as follows:

> Dissolved oxygen (DO) tended to increase during the latter part of the post-disturbance regime relative to the same season in the previous year (Fig. 3c). This pattern may reflect multiple processes, including enhanced air--water gas exchange and vertical exchange associated with inflow/pumping events, as well as biological oxygen production and consumption. Over the same period, Eh exhibited an overall upward shift following the disturbance (Fig. 3d).

> **[R2-10]** *Line 160: Temperature is a great predictor for bio activity. Please consider moving the*
> *appendix to the main text.*

[Response to R2-10]

We thank the referee for this helpful suggestion. We agree that temperature is an important environmental variable because it can influence oxygen solubility and biological activity. We considered moving the temperature and pH time series to the main text. However, because the temperature record mainly provides seasonal context rather than an independent disturbance marker, we retained the figure in the Supplementary Information to keep the main figures focused on the EC, DO, and Eh dynamics. To address the referee's point, we revised the final sentence of Sect. 3.2 to explicitly acknowledge the relevance of temperature.

[Changes in manuscript]

We revised the final sentence of Sect. 3.2 as follows:

> Temperature, which can influence oxygen solubility and biological activity, showed typical seasonal cycles and vertical structure over the observation period, while pH ranged from 5.79 to 7.35 (mean 6.60) (Supplementary Fig. A1).

> **[R2-11]** *Line 257: Do these metrics come from the current paper, or from literature. Please*
> *add to the sentence to clarify. Also, please consider naming this 2 year period a two-*
> *year period. "Long-term" is an interpretation which will depend on the reader's*
> *perspective. Line 273 uses '21-month" as a good descriptor.*

[Response to R2-11]

We thank the referee for this helpful suggestion. We agree that "long-term" is subjective and that the duration of the dataset should be stated explicitly. We also agree that the origin of the two metrics should be clarified. These metrics are proposed and applied in the present study, rather than adopted from previous literature. We have therefore revised the sentence to explicitly state that the metrics are based on the co-located 21-month DO and Eh measurements in this study.

[Changes in manuscript]

We revised the sentence in Sect. 4.3 from:

Two complementary metrics emerge from co-located, long-term measurements.

to:

In this study, we propose two complementary metrics based on the co-located 21-month DO and Eh measurements.

> **[R2-12]** *Line 266-7: The conclusion in this last line has a self reference in it. DO will always only*
> *measure O2. Eh is the value for a wide range of redox active species, as explained in the lines*
> *before. Probably the text can end without this line. Or please rephrase your conclusion. For*
> *reference: see the standard works of the relationship between redox potential values and*
> *dominant pairs and hence the dominant idea that O2 can only be present in an active state at*
> *higher Eh.*

[Response to R2-12]

We thank the referee for this helpful comment. We agree that DO measures only O_2, whereas Eh integrates contributions from multiple redox-active species as a mixed potential. Our intention was not to imply that DO–Eh co-monitoring can identify dominant redox couples or directly resolve redox pathways. Rather, we intended to describe it as a screening-level approach for detecting departures from a site-specific DO–Eh baseline.

To avoid overstatement, we have rephrased the final sentence of Sect. 4.3 and the corresponding statement in the Conclusions. The revised text now emphasizes that DO–

Eh co-monitoring can indicate changes in effective redox conditions or in the relative contributions of redox-active processes, but that complementary measurements of redox-active species would be needed to resolve the underlying redox processes and dominant redox couples.

[Changes in manuscript]

We revised the final sentence of Sect. 4.3 as follows:

> DO-Eh co-monitoring is therefore best viewed not as a substitute for comprehensive geochemical characterization, but as a low-cost screening-level approach for detecting departures from a site-specific DO-Eh baseline that may indicate changes in effective redox conditions or in the relative contributions of redox-active processes.

We also revised the corresponding statement in the Conclusions as follows:

> Together, these results support the use of the ln[O_2]-Eh slope and baseline residuals as practical, screening-level indicators of departures from a site-specific redox baseline derived from co-located sensor measurements. These departures are consistent with disturbance-driven changes in effective redox conditions, although complementary measurements of redox-active species would be needed to resolve the underlying redox processes and dominant redox couples.