Dynamics of inorganic nitrogen cycling, redox conditions, and microbial community composition in a floodplain soil induced by a heavy rainfall after summer drought

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General response and changes

The manuscript was significantly shortened and improved in language and clarity. To do this, we used ChatGPT, which we personally trained for the past years with many manuscripts. All modifications, however, have been carefully checked by the authors.

Abstract - We revised the abstract substantially to improve clarity, reduce wordiness, and sharpen the main message. Particularly in response to Reviewer 1, we toned down statements implying that nitrogen transformation processes were directly quantified. We now explicitly state that nitrogen transformation rates were not measured and that the observed nitrate patterns should be interpreted as redox-associated changes in inorganic nitrogen pools rather than direct estimates of process rates. We also revised the microbial-community interpretation by avoiding the stronger claim that rewetting selected for "better adapted taxa"; instead, we now state that microbial community composition remained comparatively stable over the 14-day experiment, while extracellular enzyme activities responded rapidly to rewetting. In response to Reviewer 2, we also improved terminology consistency by using "irrigated" for the treatment and "^2H_2O/^2H enrichment" for the deuterium tracer throughout the abstract.

Introduction - We revised the Introduction to sharpen the motivation and define a more specific knowledge gap. In particular, we now emphasize that preferential flow, the Birch effect, redox-dependent nitrogen transformations, and microbial responses to drying–rewetting are well established individually, but are rarely assessed together under realistic field conditions with depth-resolved measurements of water movement, inorganic nitrogen pools, in situ redox potentials, microbial community composition, and extracellular enzyme activities. We also revised the study objectives to better reflect what was directly measured. Specifically, we no longer frame the study as directly quantifying a transition from

“pulse-driven” to “redox-driven” nitrogen turnover processes. Instead, we state that we evaluated how redox potentials developed during the experiment and how these dynamics related to changes in inorganic nitrogen pools. This revision addresses the reviewer’s concern that nitrogen process rates were not measured directly and that process-level interpretations should therefore be presented more cautiously.

Material & Methods - We have revised this section to improve clarity, consistency, and transparency, while retaining the detailed methodological description that Reviewer 2 considered useful.

First, we standardized terminology throughout the Methods section. The W treatment is now consistently referred to as the irrigated plots, and the D treatment as the dry control plots. We also harmonized the terminology for the hydrological tracer by using D_2O-labelled water, ^2H enrichment, and δ^2H signatures consistently.

Second, we added methodological details requested by the reviewers. In Sect. 2.2, we now explicitly state that the plastic foil covering plot D2, where the redox probes were installed, was removed by wind during the natural rainfall event from 26 to 27 July 2019. In Sect. 2.3.2, we clarified that daily irrigation was paused from T11 to T13 because of the natural rainfall event. In Sect. 2.3.3, we added information on the sampling positions: all cores were collected within the inner sampling area, at least 20 cm from plot borders, and later cores were taken at least 10 cm away from previous sampling locations. We also clarified that the initial shrinkage cracks were marked before the first irrigation and that samples were collected along these marked cracks.

Third, we revised the description of the redox measurements in Sect. 2.3.4. We now state explicitly that the redox sensors and reference electrodes were installed one day before the start of the experiment. We also added a sentence clarifying that redox potentials were monitored in one irrigated plot and one dry control plot only, and therefore provide high-resolution temporal information but spatially limited replication. This revision directly addresses the reviewer’s concern that redox measurements should not be overinterpreted as fully replicated plot-level measurements.

Fourth, we revised the water-mass-balance description in Sect. 2.4.2. The previous wording implied that hardly any water leached below 50 cm, which conflicted with our later interpretation of possible water and nitrate loss below the sampled profile.

We therefore reformulated this section to state that the water mass balance represents apparent water recovery within the sampled 0–50 cm soil profile, not a closed hydrological balance. We now explicitly note that evaporation and drainage below 50 cm could not be quantified directly from soil water contents alone and that potential losses below the sampled depth were evaluated indirectly using the δ^2H isotope mass balance of the labelled heavy-rainfall water.

Fifth, we clarified the use of reference evapotranspiration. Because the experimental plots had been harvested before the experiment, we now state that ET_0 is used as a meteorological reference for atmospheric evaporative demand, rather than as a direct estimate of plot-scale evapotranspiration.

Finally, we added information to Sect. 2.1 to clarify the present hydrological status of the study site. Although the site is geomorphologically part of the Ammer floodplain, the Ammer currently flows in a regulated riverbed, and regular overbank flooding of the investigated area is unlikely under present conditions. This clarification is relevant for the later interpretation of microbial adaptation to drying–rewetting and flooding-related redox fluctuations.

Results - We revised the Results section substantially to address the reviewers' concerns about readability, repetition, and overinterpretation. In particular, we removed methodological recaps from the beginning of several Results paragraphs and restructured each subsection so that it now starts with the main observed pattern before presenting quantitative details. This was done especially in Sect. 3.1, where the water and isotope mass balances are now described in terms of the main hydrological result: rapid deep infiltration after the simulated heavy rainfall event, followed by retention of subsequent moderate irrigation mainly in the topsoil.

We also revised Sect. 3.2 to avoid implying that nitrogen transformation rates were directly measured. The subsection title was changed from "Nitrogen Cycle" to "Inorganic nitrogen masses", and the text now consistently refers to nitrate and ammonium masses rather than "turnover" or "contents" where Figure 5 reports mass balances. We retained the key quantitative results but removed stronger process-level language. The revised text now describes observed changes in nitrate and ammonium pools and leaves mechanistic interpretation to the Discussion, where it is framed more cautiously.

In Sect. 3.3, we shortened the description of the microbial community results and focused on the main finding that bacterial community composition showed high

spatial variability but no clear treatment- or depth-related separation at either the DNA or RNA level. We also clarified that alpha diversity of the active community did not differ significantly between dry control and irrigated plots. The enzyme-activity results were rewritten to emphasize the contrasting response: extracellular enzyme activities, particularly β-glucosidase and exochitinase, responded significantly to irrigation, whereas bacterial community composition did not show a consistent response over the 14-day observation period.

We also revised terminology throughout the Results section. The treatments are now consistently referred to as “irrigated plots” and “dry control plots”, and the tracer terminology was harmonized as “D_2O-labelled water”, “^2H”, and “δ^2H”. Several figure captions were polished for clarity, and we note that treatment effects in Fig. 8 should be indicated with letters or asterisks where appropriate, as suggested by Reviewer 2.

Overall, these revisions make the Results section more concise, less repetitive with the Methods, and more closely aligned with the stated study objectives. The revised text now separates observed patterns from mechanistic interpretation more clearly, thereby addressing the concern that the previous version overinterpreted nitrogen-pool data as direct evidence for process-rate changes.

Discussion - We substantially revised the Discussion to address the reviewers’ concerns about overinterpretation and to improve the balance between interpretation and evidence. In particular, we removed the previous statement that nitrogen cycling “switched” from a pulse-driven regime to a redox-driven regime after approximately one week. We agree that such a process-level conclusion cannot be supported directly because gross nitrogen transformation rates were not measured. The revised Discussion now interprets the observed nitrate and ammonium patterns as changes in inorganic nitrogen pools that are associated with hydrological redistribution, rewetting, and redox dynamics, rather than as direct evidence for changes in process rates.

In Sect. 4.1, we retained the main hydrological interpretation that the simulated heavy rainfall event caused rapid deep infiltration through drought-induced shrinkage cracks. We clarified, however, that nitrate transfer below 50 cm is an indirect inference based on the decrease in nitrate mass in the upper profile, the rapid movement of D_2O-labelled water to 50 cm depth, and the absence of nitrate accumulation within the sampled subsoil. We also shortened the discussion of the

underlying aquifer and reframed it as a broader implication for nitrate transfer to deeper soil layers and groundwater, rather than as a direct result of this study.

In Sect. 4.2, we revised the nitrogen discussion to avoid inferring nitrification or denitrification rates from pool-size data. The section now distinguishes between the immediate ammonium increase in the 0–5 cm layer, which is consistent with a short-term Birch-effect response, and the later nitrate dynamics, which are discussed as redox-associated changes in nitrate pools. We explicitly state that nitrate and ammonium pool sizes integrate multiple simultaneous processes, including production, consumption, immobilization, and transport, and that gross transformation rates would be required to resolve these mechanisms quantitatively.

We also addressed the reviewer's concern about the limited spatial replication of the redox measurements. The revised Discussion now states that the redox records provide useful high-resolution temporal context but were obtained from one irrigated and one dry control plot only, so spatial extrapolation to all replicate plots must remain cautious. This limitation is now incorporated directly into the interpretation of nitrate dynamics.

We further revised the discussion of the increase in inorganic nitrogen masses in the dry control plots. Rather than presenting a single explanation, we now discuss this result as an example of the complexity of interpreting inorganic nitrogen pools under field conditions. Possible contributing factors include prolonged drought stress, microbial biomass turnover, upward redistribution of solutes by evaporation, and altered microclimatic conditions under the plastic cover. These mechanisms are explicitly presented as tentative.

In Sect. 4.3, we toned down the previous interpretation that rewetting selected for more adapted microbial taxa. The revised text now emphasizes that bacterial community composition did not shift detectably at either the DNA or RNA level, whereas extracellular enzyme activities responded clearly to irrigation. We therefore interpret the microbial data as evidence for rapid functional activation after rewetting, rather than for compositional restructuring or selection. This revision directly addresses the reviewer's concern that the previous microbial interpretation was stronger than the data allowed.

Overall, the revised Discussion now maintains the central message that heavy rainfall after drought can rapidly redistribute water and nitrate through preferential flow pathways, while presenting nitrogen-transformation and microbial-community

interpretations more cautiously and in closer alignment with the measurements performed.

Conclusions - We revised the Conclusions to make the final message shorter, clearer, and more focused on the main findings and their broader relevance. In response to the reviewer's concern about overinterpretation, we removed the previous statement that nitrogen cycling transitioned from a Birch-effect-driven phase to "normal redox-driven nitrogen cycling", because gross nitrogen transformation rates were not measured. The revised conclusion now states more cautiously that redox potentials declined after irrigation and were associated with later changes in nitrate pools, but that these patterns cannot be interpreted as direct estimates of nitrogen transformation rates.

We also revised the interpretation of the microbial results. The previous conclusion suggested that rewetting shifted the microbial community toward better adapted taxa. Because microbial community composition did not differ significantly between treatments at either the DNA or RNA level, we removed this claim. The revised conclusion now emphasizes that extracellular enzyme activities responded rapidly to irrigation, whereas microbial community composition remained stable over the 14-day observation period.

Finally, following the reviewer's suggestion to strengthen the broader relevance of the study, the revised conclusion now highlights that heavy rainfall after drought can create a brief but important window for nitrate displacement through preferential flow pathways. This process may bypass nitrogen retention in the biologically active topsoil and increase nitrate transfer to deeper soil layers or shallow groundwater. We also added a forward-looking statement that future work should combine tracer-based hydrology with direct measurements of gross nitrogen transformation rates to better disentangle transport processes from microbial nitrogen cycling during drought–rewetting events.

Response to Reviewer 1

General comments:

The authors present results from a field experiment examining water flow, nitrogen cycling, soil redox potential, and microbial community composition responses to initial wet up of dry soil and subsequent smaller irrigation events. The study utilizes advanced techniques

such as stable isotope labeling to track the movement of the initial pulse of water added and RNA extractions to characterize the active microbial community. The authors conclude that water movement down preferential flow paths (cracks in dry soil) can potentially lead to nitrate leaching into an underlying aquifer, that following a pulse of nitrogen mineralization after the initial wet up (Birch effect) that soil redox potential drives inorganic nitrogen cycling ("normal redox-driven nitrogen cycling"), and that rewetting shifts the microbial community toward "better adapted taxa". I found the manuscript to be lacking in compelling motivation (e.g., the knowledge gap identified was diffuse not specific) and that the findings were largely confirmatory of existing knowledge (e.g., water moves down preferential flow paths such as cracks in the soil, the Birch effect is short-lived).

We rephrased and strengthened the motivation for the study and made clear, that there is already a solid knowledge on individual of the studied topics. However, our study combines all these aspects to evaluate them together under field conditions (5th paragraph of the Introduction).

The presentation of results could be improved by framing a narrative around the research questions or study aims. As is, each paragraph of the results section reads as a comprehensive laundry list of results that the reader can get lost in. Also, some paragraphs start with a recap of the methods (e.g. Lines 323-325, 337-340, 376-379, 408-411) or embed results within the context of explaining the methodology (e.g., Lines 307-313, 350). Topic sentences that highlight the overall patterns as relevant to the main takeaways would help orient the reader.

We revised the results section according to the reviewer's suggestions. We shortened the manuscript substantially and deleted recaps. Where appropriate, we added a one-sentence summary of the main findings at the beginning of each Results subsection.

Specific comments:

I do not agree with the authors' conclusion that their data demonstrate processes switching from a "pulse-driven regime after rewetting to the 'usual' redox-driven nitrogen cycling regime governed by oxygen availability" (Lines 561-565). This is an over-simplification of the nitrogen cycling dynamics in the two weeks following wet-up. Interpreting patterns in nitrogen cycling process rates from ammonium and nitrate masses (note that "mass" is reported in Figure 5, not "content" as referred to in the text) is always rife with speculation because there are many different production and consumption processes that can be in play—and in this case, transport of nitrate (and to a lesser extent, ammonium) from overlaying soil can also factor in. While the extracellular enzyme activity response to initial wetting does support the interpretation that a wetting-induced increase in nitrogen mineralization led to the increase in ammonium mass in the top 10 cm of soil, I

do not find strong evidence that soil redox subsequently drove the observed ammonium and nitrate masses on T6/T8 and T14. The logic presented on Lines 542-560 to explain the increase in nitrate mass in the wet plots over time is that temporal variation in soil redox reflected changes in oxygen availability associated with daily irrigation events, and that nitrification led to nitrate production whenever soil redox was greater than +100 mV. Without any measurements of nitrification rates, this is all speculation from two sets of soil redox measurements in one wet plot (see comment below). Furthermore, both extracellular enzyme activity (a proxy for nitrogen mineralization or ammonium production rates) and ammonium mass remaining relatively constant from T0 through T14 in the wet plots, suggesting that rates of ammonium consumption (via microbial assimilation or nitrification) also remained relatively constant during this period. This suggests that nitrification rates did not ramp up after T0 to lead to the increased nitrate mass on T6. However, this is all speculation on my part because no process rates were actually measured.

We substantially revised the Discussion to address the reviewers' concerns about overinterpretation and to improve the balance between interpretation and evidence. In particular, we removed the previous statement that nitrogen cycling "switched" from a pulse-driven regime to a redox-driven regime after approximately one week. We agree that such a process-level conclusion cannot be supported directly because gross nitrogen transformation rates were not measured. The revised Discussion now interprets the observed nitrate and ammonium patterns as changes in inorganic nitrogen pools that are associated with hydrological redistribution, rewetting, and redox dynamics, rather than as direct evidence for changes in process rates.

We corrected ammonium/nitrate "content" to "mass" throughout the text.

The very curious increase in ammonium and nitrate mass in the dry plots between T0 and T14 highlights how difficult it is to infer controls on nitrogen cycling process rates simply from changes in inorganic nitrogen pool sizes. The authors offer some potential explanations (Lines 569-575), but none seem very convincing.

In the revised manuscript we highlight that this increase in ammonium and nitrate mass illustrates how difficult the interpretation of changes in the nitrogen pools are. However, we still give some tentative possible explanations.

I understand the technical reasons why the soil redox measurements occurred in very few plots, but this raises some concerns about the validity of drawing major conclusions from

the measurements. Soil redox measurements occurred only in three locations (two locations in one wet plot and one location in one dry plot), and the soil redox patterns differed greatly between the two locations in the one wet plot. Therefore, I have concerns about using these measurements to speculate about drivers of nitrogen cycling process rates across all three wet plots. Moreover, the reference electrodes were placed in the glyphosate and irrigation plot, suggesting that they may not have served as a good stable reference for determining the redox potential measured as the voltage difference between them and the redox probes placed in the wet and dry plots.

The revised Discussion now states that the redox records provide useful high-resolution temporal context but were obtained from one irrigated and one dry control plot only, so spatial extrapolation to all replicate plots must remain cautious. This limitation is now incorporated directly into the interpretation of nitrate dynamics.

Because of the different response of the redox probe in the irrigated plot vs. the dry plot, which timewise reflects the irrigation and the natural heavy rainfall event quite well, we are convinced that the redox measurements are reliable and in general reflect the development of the soil redox potential during the experiment. However, the direct link of the measured redox potential to nitrogen turnover processes is not made anymore.

I found it difficult to interpret the results without seeing data on soil moisture along the depth profiles. While the water mass balance and soil redox potential results were shown in Figures 3 and 4, respectively, and a description of the methods for determining gravimetric soil water content was described in Lines 210-214, I did not see these data reported in the manuscript.

We added a figure of soil water content in the Appendix of the revised manuscript.

Technical corrections:

Please use paragraph indents, and even line breaks between paragraphs, to make it easier for reviewers to read the manuscript. As is, each page was a solid block of text, and it made it difficult to orient to where a new line of thought was introduced in a new paragraph.

Paragraph formatting was adjusted

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Response to Reviewer 2

The authors present a well-constructed and thoroughly carried-out study on microbial responses to heavy rain following summer drought. The manuscript is well written, although perhaps a bit unnecessarily wordy and/or complicated in terms of sentence structure etc. This especially applies to the abstract and some places in the results and discussion sections (e.g., L408-411, L577-578). I do commend the authors on providing a very extensive and detailed material and methods section, which is unusual and definitely a section that merits thorough reporting. Finally, I would recommend a stronger and clearer message in the conclusions to highlight the relevance of this study from a broader perspective, i.e. put it into context.

Overall, this is a nice study and a great manuscript that should be of interest to EGUsphere readers and the wider science community. I would recommend publishing this manuscript after some minor revisions as listed below.

Detailed comments

Throughout manuscript: Please use consistent terminology for the treatments (e.g. W is sometimes referred to as "water" or "watered" and sometimes as "wet") and the deuterium label (sometimes D_2O, sometimes ^2H and sometimes deuterium).

We adjusted the terminology as suggested.

L138-139 Please add a note here about the accidental (I assume by wind) removal of the plastic cover on one of the plots (D2) in the latter natural precipitation event. I know it is mentioned later in the text (L362-363), but should be noted here for transparency.

I added the following sentence after the last sentence of the paragraph: "However, during the natural heavy rainfall event of 26 July to 27 July the plastic foil of plot D2, where the redox probes were installed, was removed by wind."

L152-158 Figure 1, if available it would be nice to see where within each plot the cores were taken on the 3 occasions. And even better, where noticeable drought cracks were located relative to those locations.

We do not have notes on where exactly on the plot each sample was taken. All samples were taken at least 20 cm away from the border of the respective plot to minimize boundary effects (compare with Figure 1). The holes that were created by the sampling were refilled with bentonite. Samples taken later were always taken with a distance of at least 10 cm from the previous core locations. The shrinkage cracks on each plot were marked before

the first irrigation. After irrigation, all samples of this study were taken at sites where shrinkage cracks were visible in the beginning of the study (compare section 2.3.3).

L198-199 When where the sensors installed? In other words, how much equilibration time was given prior to starting the experiments?

Due to time constraints, the redox sensors were installed on 15 July 2019, one day before the start of the experiment. We added that information in the Material and Methods section 2.3.4.

L233 Remove the comma before the end of the sentence

corrected

L234 The assumption that no water leached below 50 cm seems inappropriate and also contradicts statements made later in the text about water being lost below the sampling depth (e.g., L466-467, L475, L493-494). It should be possible to estimate the loss to deeper depths based on the deuterium labeling and estimated ET.

The reviewer is right. We revised the paragraph and rephrased that section: “The resulting water mass balance represents apparent water recovery within the sampled 0–50 cm soil profile. It does not constitute a closed hydrological balance, because water loss by evaporation and drainage below 50 cm depth could not be quantified directly from soil water content measurements alone.”

L248 Suggest referring to the section number (2.4.2) rather than the header (easier to read the sentence)

corrected

L309 You cannot follow the flow pathways, just measure the infiltration depth

We adapted the sentence following the suggestion.

L313 HRE was already defined earlier in the text

We deleted "(HRE)". We also rephrased the sentence in L 337: "While the HRE irrigation water at T0…"

L342 Some comment on the potential reason for the >100% recovery should be made

> 100 % recovery could result from slightly uneven irrigation intensity or confluence of the irrigation water at the shrinkage crack sites where the samples were taken.

Sentence inserted: "Recovery of more than 100 % result from slightly uneven irrigation intensity or confluence of the irrigation water at the shrinkage crack sites where the samples were taken."

L354 "in most cases" seems a bit inexact with only 2 locations and 3 depths at each, especially with W1 30 and 40 cm fluctuating much more than W2 and the 50 cm depth

We rephrased the sentence: "In the subsoil (30–50 cm below ground), the decline was slower and more variable; the time required to reach +200 mV ranged from one day at 30 cm depth in sensor 2 to six days at 30 cm depth in sensor 1."

L370-373 Figure 4, consider adding the timing of the natural rain events to the panels. Also, "in depths" should be "at depths" and "at experiment day" should be "on experiment day".

Figure modified accordingly. Language was corrected.

L401 It looks to me in Figure 5 as if there was an increase in nitrate both at 20-30 and 30-40 cm from WT0 to WT6.

We rephrased the sentence: "From WT0 to WT6, nitrate mass increased mainly in the upper 20 cm, while changes below 20 cm were small."

L419 Add "Violin plots of observed…"

corrected

L434 remove “short time frame” and replace with “14-days” (remove from the paranthesis) for more specificity

We rephrased the sentence: “This indicates that neither total nor active bacterial community composition changed consistently in response to irrigation over the 14-day observation period.”

L442 “significantly higher activities” if possible it would be good see this indicated by e.g. different letters or asterisks above the bars in Figure 8.

Figure was adapted

L475 my understanding is that no plants were growing on the plots during the experiment, if so it would be more appropriate to refer to evaporation loss only (no transpiration)

“The only factors affecting ET_O are climatic parameters. Consequently, ET_O is a climatic parameter and can be computed from weather data. ET_O expresses the evaporating power of the atmosphere at a specific location and time of the year and does not consider the crop characteristics and soil factors.” Allen et al. 1998 (see reference list of the paper).

We added a sentence for clarification of our interpretation of ETo: “Because the plots had been harvested before the experiment, water losses were likely driven mainly by evaporation and drainage below the sampled profile rather than by plant transpiration. The calculated ET_0 of 4.2 mm day^{-1} therefore represents atmospheric evaporative demand rather than plot-scale evapotranspiration.”

L494-500 Although this seems very plausible it does not directly add anything of relevance to the current study, other than the potential risk of nitrate contamination of groundwater, which would fit better in the conclusions as a framing of why this study is important from a larger perspective.

We don’t discuss this aspect in detail anymore as in the original paper but state in a more general way, that nitrate leaching by preferential flow is a potential threat for groundwater quality:

“The potential transfer of nitrate below the biologically active topsoil is relevant for groundwater quality, particularly in fine-textured floodplain soils where shrinkage cracks can bypass the soil matrix. The study site is underlain by a shallow tufa aquifer below the

upper clay-rich layer (Martin et al., 2020). If nitrate reaches such deeper zones repeatedly, its fate will depend on the local redox status and denitrification capacity of the subsurface. Therefore, heavy rainfall following drought may represent a short but important hydrological window during which nitrate can bypass retention and transformation in the upper soil profile."

L503 consider adding infiltration, as likely not all water was percolating

corrected

L509 Can you add references to some more recent studies observing this?

Unfortunately, we could not find more recent studies during the literature research. Any suggestion is acknowledged.

L517-518 I do not fully follow the reasoning here. Do you mean that the combined ammonium and nitrate distribution heterogeneities were not impacted by the cracks? That seems to be contradicted by earlier statements. Please clarify.

We rewrote the section and clarified our interpretation: "Coefficients of variation (Appendix H) showed that inorganic nitrogen distributions did not simply mirror the spatial pattern of the δ^2H-labeled water. While preferential flow clearly governed rapid water redistribution during the initial event, nitrate and ammonium masses were also shaped by sorption, biological transformations, and local soil heterogeneity. Thus, drought-induced cracks were important for rapid water and nitrate displacement, but they were not the only control on inorganic nitrogen pool distributions during the subsequent 14 days."

L533 "did not respond much" – much is a vague and relative term, consider replacing (also L609)

We changed the sentence to: "Below 5 cm depth, ammonium masses changed only moderately in irrigated plots."

L533-541 This section is a bit hard to follow. Consider reorganizing to a short list of the 3 alternatives and then explaining each in more detail, rather than having the explanation after each one.

We reorganized and shortened the section.

L551 Why does sensor 1 respond so differently compared to sensor 2? What subsurface structures or biogeochemical differences may have contributed to this discrepancy in responses? Some comment on this would be appropriate.

We added a sentence here: "The sometimes very marked differences in the sensors' responses can be caused, for example, by the highly uneven distribution of dry cracks or remaining root fragments from the harvested plants."

L554-555 Add reference to the figure supporting this statement.

Figure reference added.

L556-557 Why was the daily irrigation paused? I assume it was due to the natural precipitation events, but the reason should be noted in the materials and methods section.

Correct. We added the sentence "Daily irrigation was paused from T11 to T13 due to a natural heavy rainfall event on the evening of T10 and ongoing natural rain fall on T11 and T12." to section 2.3.2.

L559 Was total N measured in the soil? It would be a good complement for understanding the N mass balance. If not measured and soil samples are still available, I suggest adding this analysis.

Unfortunately, total N was not measured. No soil samples have been stored as back-up.

L567-569 This information should be moved to the materials and methods section.

We inserted this sentence in section 2.3.2 Plot treatment: "The dry control plots were covered with greenhouse foil between T10 and T14 to prevent wetting by natural rainfall."

L569-570 Add space between “activating” and “microbial” and before “although” (after the comma)

corrected

L595-596 On what do you base the statement that the microbial communities were not adapted to fluctuating water availability? The soil is on an arable floodplain that presumably undergoes such fluctuations somewhat regularly, so why would the microbes not be adapted to it?

We call the area a floodplain, because it shows a sediment record typical for floodplains (coarse sediments in alternating with clay horizons). Currently, the Ammer river is not a naturally flowing river anymore but flows in a controlled river bed. Therefore, the flooding of the whole “floodplain” does not occur regularly anymore, and the soil microbial community is not likely to be adapted to wet-dry-cycles.

We added this sentence to Section 2.1 Study site: “Although the site is geomorphologically part of the Ammer floodplain, the Ammer currently flows in a regulated riverbed. Regular overbank flooding of the investigated area is therefore unlikely under present conditions. This information is relevant for interpreting microbial responses to drying–rewetting and flooding-related redox fluctuations.”

L608 t is missing at the end of “effect”

corrected

L623 the red-brown color in b is not very clear to me, it looks more like the bottom part of the core in a

The core in “b” is still wet while the core in “a” is dry. Therefore, the brown color in “b” looks darker than the brown color in the top of the core in “a” on the picture. The brown color of the core in “b” resembled the brown color at the top of the core in “a”.

L628 for which time point were the cores analyzed for TOC? And for texture (L645)

These were cores from Day 10 (dry plots) and Day 14 (water plots). Information was added in the appendices.

L631 I assume this was a “planetary ball mill”, please specify manufacturer and model

Yes it was a planetary *ball* mill, corrected.

L634 suggest replacing “control” with “ascertain” or similar

corrected

L671 Table E1 (and where relevant in main text) – what was the difference in vegetation btw the experimental plots and the weather station used for estimating ET? Related to my comment above about whether only evaporation should be considered.

The weather station is located in a vineyard at a southern slope.

For the interpretation of ETo please see comment above.

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