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Response to reviewer 1

>Dear Editor,
>I have revised the manuscript entitled "Quantifying the impact of groundwater fauna and temperature on the ecosystem service of microbial carbon degradation" by Schmidt, Rütz and Marxsen. I found the work very interesting and timely. The study plausibly demonstrates the important ecosystem service provided by microbes and groundwater-dependent fauna, both in terms of carbon degradation and in preventing brownification of groundwater used for human consumption. I recommend acceptance after the authors have considered the points listed below.
>The introduction is well written and supported by robust and up-to-date literature. The rationale is clearly presented and the hypotheses are explicitly stated at the end of the section. Figure S1 is well constructed and informative.

Reply: We thank the reviewer for the comprehensive diligent review and constructive feedback.

>Although I do not consider myself fully qualified to evaluate every aspect of the modelling framework, I carefully followed the methodological steps as presented. The calculations appear internally consistent and the logical structure is coherent. The authors also provide a transparent discussion of the main assumptions and limitations of the model in the final section of the manuscript, which I found very appropriate and commendable. However, it might be helpful to frame the model more explicitly, already in the Methods or early Discussion, as a strongly parameterised proof-of-principle approach.

Reply: We agree with the reviewer that this point needs to be stressed and have included it in the abstract (former line 13 f.), in the introduction (former line 121 f. and 135), and in the "2 Study area and methods" section, former li. 143f..

> Given the number of assumptions and parameter choices, the results are best interpreted in qualitative and comparative terms among scenarios rather than as precise quantitative predictions. Making this perspective explicit earlier in the manuscript would further strengthen its clarity and robustness.

Reply: We completely agree with the reviewer and have additionally to the places named above, also included it in the first line of the discussion, former li. 319f., and picking up in the first line of the conclusion, former li. 530f.:

former li. 319f.: "*This is to our knowledge the first attempt at developing a **process-based proof-of-principle approach** model of the groundwater food web, and it is the first endeavour at comparing scenarios of excluding fauna from the system*."

former li. 530f.:

"*This first **proof-of-principle** study on quantifying the change in ecosystem services at scenarios of losing groundwater fauna, and of increased temperature, showed that the partial loss of ecosystem services (BOC degradation) due to lacking fauna increased remaining BOC up to 660-fold, based on the 3.5-year observation period."*

> The results are overall convincing and internally coherent. The model reproduces the measured time series reasonably well (Fig. 1b–e), and the agreement between simulated and observed dynamics supports its use for exploring scenarios. Figure 2 clearly shows that differences among scenarios are driven mainly by the presence or absence of fauna rather than by moderate temperature increases, since the "reference" and "+1.5 °C" lines largely overlap across zones. The strong divergence of the "no fauna" scenarios in the "M" and "P" zones convincingly supports the conclusion that faunal grazing substantially influences BOC dynamics and therefore the ecosystem service of carbon degradation.

Reply: We are glad for the feedback that the presentation was overall convincing.

>Some statements, however, would benefit from slightly more cautious wording (see specific comments below).
>I found the discussion particularly interesting where the authors provide a quantitative estimate of the loss of ecosystem services (in terms of BOC degradation) under scenarios combining fauna absence and increased temperature due to climate change. The estimates are convincing and I appreciate the effort, because although it is widely acknowledged that subterranean ecosystems provide important services (see, for instance, Mammola et al., 2026. Subterranean environments contribute to three-quarters of classified ecosystem services. Biol Rev Camb Philos Soc. 2026 Feb 10. doi: 10.1002/brv.70137), their quantitative evaluation is still limited.

Reply: We thank the reviewer for pointing out this recent publication and have added a reference to the Introduction, former li. 24:

"*Ecosystems fulfil functions that humans rely on (Koch et al, 2024b). Groundwater is no exception (Avramov et al., 2010; **Mammola et al., 2026**).*"

> In the final part of the manuscript, the authors openly discuss the weaknesses of the model and the study. This transparency is commendable and provides a solid basis for future refinement as new data become available. I found this attempt to model groundwater ecosystem services both courageous and valuable.

Reply: We appreciate the reviewer's feedback that our approach was convincing

>Specific comments

>Abstract

>No major comments.

>Introduction

>Line 24. I suggest expanding the references supporting the statement "Groundwater is no exception (Avramov et al., 2010)" by including the most recent work by Mammola et al. 2026 doi: 10.1002/brv.70137

Reply: Done as suggested; see also above

>Lines 42–44. The term "producers" may not be immediately clear. Consider revising to: "…producers (i.e. the basal microbial community including both autotrophs and heterotrophs sustained by imported organic matter)."

Reply: Done as suggested

>Line 44. Perhaps: "When even the producer biomass becomes substantial…".

Reply: We agree with the reviewer that there might be a temporal aspect to the development of a complex food web, as implied by "when". We were thinking rather of the spatial aspect, because we still lack clear indicators where to expect complex food webs and where not (e.g. Saccò et al. 2024). We have therefore added ", both on temporal and spatial scales," in former line 50 so that the sentence now reads "*Under which circumstances, **both on temporal and spatial (Saccò et al. 2024) scales**, which complexity of the food web emerges, is still largely unknown and has thus been near-impossible to parameterize*.", and we have added "and where" in former line 44 so that the sentence begins with: "***When and** where even the producer biomass becomes substantial, …* "

>In addition, regarding the term "grazers," I would suggest using a more general term, since groundwater fauna can include both grazers and deposit feeders that ingest sediments and digest the microbial biofilm attached to them.

Reply: We appreciate this important addition and have rephrased the sentence, and added a short explanation: "*When and where even the producer biomass becomes substantial, a further level may be added, i.e. predators preying on grazers **and deposit feeders** (Ercoli et al., 2019; Saccò et al., 2019). **In the following, we will encompass deposit feeders in the notion "grazers**"*."

>Lines 53–54. This sentence seems slightly disconnected from the surrounding text. It may benefit from relocation to improve the logical flow.

Reply: We see the reviewer's point and have moved the sentence to the following paragraph as the new last line.

>Line 111. Please consider adding: Vaccarelli et al. (2023, One Earth, 6, 1510–1522), which strongly supports your statement.

Reply: Done as suggested

>Line 115. You may consider citing: Iannella et al. (2020, Scientific Reports, 10, 19043) to support this point.

Reply: Done as suggested

>Line 128. Possibly: "in faunal composition (Di Lorenzo et al., 2025)?"

Reply: Thank you for pointing out this oversight – we cite two publications by Di Lorenzo et al. from 2025, and this is the one indicated as 2025a in the reference list; now corrected in the text.

>Results
>Lines 236–238. The statement that event-based recharge "contributed little" seems plausible, but this is not directly demonstrable from the figure alone. It would be safer to write "appeared to contribute little under the model assumptions."

Reply: Changed as suggested

>Lines 255–257. The interpretation that summer temperatures became "lethal" at +1.5 °C is mechanistic and not directly shown in the plots. It would be safer to state that fauna declined seasonally under elevated temperature, without explicitly attributing lethality unless this is independently supported.

Reply: Thank you for spotting this. Even if the interpretation was more substantiated, it should not appear within the results, so we deleted the half sentence about possible lethal effects.

>Discussion
>Lines 353–357. Please clarify this interpretation. The text states that early sudden dips in microbial dry mass occurred in the "A" and "R" zones and might reflect feeding by higher-than-average fauna biomass. However, in Fig. 1d the most pronounced temporary reductions to near-zero microbial dry mass appear in the "M" and "P" zones, and these coincide with peaks in fauna dry mass in Fig. 1e (as mentioned in lines 369–370). Could this be a mislabelling of zones, or am I overlooking a specific pattern in "A" and "R"?

Reply: This was misleading wording. Too much was left out for the sake of brevity. We added sentences and tried to lead more succinctly to the interpretation: "***In the "P" zone, dry mass rose to apparently the carrying capacity, dipped at the beginning of 1979 to low values and then rose again to apparently the carrying capacity. Fauna dry masses were lowest in***

the "P" zone (see below). In the "M" zone, the microbial dry mass rose very slightly to apparently the carrying capacity and then dropped at the end of 1978 to near-zero and never recovered. Also in the "A" and "R" zone, dry masses dropped to near-zero, already early in 1978, and never recovered. *The* ***fact that the dry masses*** *in the "A" and "R" zones* ***dropped so early****, might reflect moments of feeding by higher-than-average fauna biomass (see below)*."

>Lines 389–397. The interpretation that higher microbial dry mass in the "no fauna" scenarios is associated with reduced BOC degradation and attributed to a lack of rejuvenating grazing could be further strengthened. An additional explanation might relate to microbial community structure and activity. For example, Di Lorenzo et al. (2025, Biogeosciences, 22, 1237–1256) reported a correlation between groundwater-obligate crustacean abundance and low-nucleic-acid (LNA) bacterial cells, suggesting selective feeding interactions. Since LNA cells are generally less metabolically active, the absence of fauna could allow the accumulation of microbial biomass that is not functionally active in terms of carbon uptake. In this perspective, higher microbial dry mass but lower BOC degradation in the "no fauna" scenarios may reflect a shift towards less active microbial fractions rather than more effective processing. Even if the current model does not distinguish active and inactive pools, mentioning this conceptual perspective could provide a biologically grounded explanation.

Reply: These are very important points that we gladly expand upon in the discussion. We added the following sentences: "***In addition to quantitative effects, grazing might also qualitatively change the microbial community (Longnecker et al., 2009) and might thus influence functions, and in the consequence, ecosystem services. As an example, groundwater crustacean abundance correlated with low-nucleic-acid (LNA) bacterial cells (Di Lorenzo et al., 2025a). This may hint to selective feeding (Jochem et al., 2004). HNA cells are often more metabolically active (Longnecker et al. 2005) and might thus be the preferred food source in grazing, leaving over microbial biomass less functionally active which then degrades even less carbon than the same dry mass of an un-grazed community might have done***."

>Line 533. Please compare the BOC values here with those reported in line 492 and line 18 for consistency (6.6 time vs. 660-fold).

Reply: Thank you for spotting this oversight. Now corrected.

>Supplementary File

>Line 78. Please clarify what "L" means in "1419.5 mm year^{-1}, i.e. L m^{-2} year^{-1}."

Reply: We clarified as follows in the former L. 78: "… *the average precipitation in mm per year was 1419.5 mm year^{-1}, i.e. L m^{-2} year^{-1},* ***Liters per square meter and year (Kopáček et***

al., 2009; note that precipitation is either reported as mm in a rain gauge, or as volume per square meter)."

>Line 108. Should this read "10^6 cells mL^{-1}"? Please check that "cells" is not missing and verify the exponent (−6). This last point is important because I think it is 10^6 cell/mL

Reply: Thank you for spotting this writing error – of course it is 10^6 cells/mL

>Lines 109–111. It is not clear how dry mass per cell was derived (pg cell^{-1}? fg cell^{-1}?). This is not necessarily incorrect, but an important methodological step seems to be missing.

Reply: Indeed, this step was unfortunately missing from the explanations in Marxsen et al. (2021), and thus, was not tractable here. As in Marxsen et al. (2021), we had multiplied cell volumes with a known bacterial wet biomass density of 1.07 g/cm^{-3} (Bakken and Olsen, 1983: Cell, 45, 1188–1195) and multiplied by 0.3, assuming a dry matter content of 30% (Bakken and Olsen, 1983). The prokaryote cell numbers in the River Fulda floodplain had been reported for size classes. When multiplying the average volumes of bacterial cell size classes first listed in Marxsen (1982; Beiträge zur Naturkunde Osthessens - Schlitzer produktionsbiologische Studien, 51, 3–11) and used in Marxsen et al. (2021), yields between 0.016 pg for a <1 µm length rod and 1.9 pg for a > 2 µm cocci, or between 16 fg for a <1 µm length rod and 1926 fg for a > 2 µm cocci (the latter were rare):

dry mass

here: pg (10^-12 g)

Rods

<1 µm	**1-2 µm**	**2-3 µm**	**3-4 µm**	**>4 µm**
0.01605	0.0642	0.0963	0.1284	0.1926

Cocci

<0,5 µm	**0.5-1 µm**	**1-2 µmm**	**>2 µmm**
0.00963	0.08025	0.642	1.926

We have added this information in the SI, now SI3.

>Line 111. The average prokaryotic biomass of ~200 µg L^{-1} should be explicitly stated as dry mass to avoid confusion with carbon or COD.

Reply: Done as suggested.

>Lines 112–115. Please check this sentence for clarity. A closing parenthesis may be missing.

Reply: Thank you for spotting this oversight. Now corrected: “To calculate a budget from the different types of carbon compounds (prokaryote dry mass, fauna dry mass, DOM = dissolved organic matter, TOM = total organic matter), were translated to mol COD L-1.”

>Line 114. “Converted” may be more appropriate than “translated.”

Reply: Done as suggested.

>Lines 120–122. The reaction appears correct, but I wonder whether COD (acetate) should be expressed as 2 mol O_2 per mol of acetate, rather than “2 mol L^{-1}” in absolute terms. Should this not be linked to acetate concentration?

Reply: Indeed; Done as suggested.

>Lines 166–169. The adopted temperature scaling can be justified as a pragmatic modelling choice. However, it should be stated more clearly that it relies on a linear regression derived from optimal growth rates of a limited number of bacterial species over a warm temperature range (14–44 °C), and that applying it to typical groundwater temperatures, often below this range, represents an extrapolation beyond the original domain of validity. Explicitly stating this assumption would improve transparency.

Reply: We agree with the reviewer and have added the following sentences after the 14–44 °C range: “***Such temperature range is warmer than Middle European average groundwater temperatures, and thus, the derived regression might not apply for Middle European aquifers. Until further data are available, this is the best estimate at hand***.”

>Line 253. I believe you are referring to S5 rather than S3.

Reply: Yes, thank you for spotting this. Now corrected.

Response to reviewer 2

>The submitted manuscript aims at quantifying the change in ecosystem services, particularly carbon degradation, due to loss of groundwater fauna and increased groundwater temperature. While the topic is indeed very interesting and important, the presented study has multiple severe issues related to the soundness and robustness of the

chosen approach, regarding amongst others the vast number of underlying assumptions, which are in my opinion not sufficiently justified (see comments below).

Reply: We thank the reviewer for the comprehensive diligent review and feedback. We had tried to justify every decision where we made assumptions, and are now clarifying remaining open points.

>I therefore recommend to reject the current manuscript, although I think it could be resubmitted as a local case study on the relationship between groundwater fauna, microbiology and groundwater conditions under very specific hydrogeological conditions, with a more critical discussion of underlying modelling assumptions and limitations.

Reply: We regret the reviewer's discontent. We do not agree with the study being relevant only on the local scale. While we used data from a local study to parameterize where possible, most of the parameters, as the reviewer correctly notes (see above), had to be assumed from a scant global data base and at this stage would apply in most studies at any place, until we have better, regionally and structurally diversified, data sources.

>General comments:

>Soundness of scientific approach:

>I found that some of the information in the introduction is presented in an incomplete and biased manner. For example, line 100 states that Brielmann et al. (2009) found decreasing faunal diversity with increasing temperature. However, this study also found that faunal abundancy is not influenced by groundwater temperature. This finding is contradictive to the working hypothesis and modelling assumption, so please also comment on it.

Reply: Indeed, citing Brielmann et al. (2009) with its focus on diversity, when in fact, the focus in this modelling study is on dry mass, was not helpful and we thank the reviewer for spotting this inconsistency. We deleted the sentence and replaced it with the following: "In a meta-analysis, Vaccarelli et al. (2023) showed that all physiological response variables tested (stress response, mortality, and metabolism) exhibited statistically significant effects from climate change. A temperature of 20.2 °C was found to be lethal for half of the tested individuals of *N. inopinatus* after 25 days (Brielmann et al., 2011)". The subsequent examples from the literature should suffice to show the negative impact of temperature on groundwater fauna. No journal article, even no book, can be "complete" – there will always be some information missing. In our view, it is important to make succinct convincing arguments and we realize that citing Brielmann et al. (2009) was doing the opposite and thus, deleted the respective sentence, to let the subsequent sentences speak for themselves. We are happy to add more examples if these subsequent sentences are not deemed convincing enough.

>Just as critical in my opinion is the statement on modelling in line 120 "Since it is hard to derive the relevant data on the required resolution, […], one way to approach answers […] are models." Having representative, reliable and sufficient data is a hard prerequisite for any modelling task, otherwise one will end up with "garbage in, garbage out".

Reply: While in general we agree with the reviewer that a model needs to be based on sound assumptions and data, we do not agree with the insinuation that we fed the model with garbage. We certainly agree with the data having to be "representative, reliable and sufficient", but we are not sure what that means in the concrete case. The data are of course representative of a floodplain aquifer because nothing indicates that the studied area is too special to be representative. To address the question whether the data, and thus the model, are representative of the majority of aquifers we would need more data than we globally have.

We do not consider the data or the model to be unreliable but also do not quite see what reliable means in this context, and how it would be shown or even proven. We lack the data for a validation globally, thus, we do not see a way to quantify reliability at this stage.

No field data, and probably no lab data either, are ever "sufficient" because we will never have all the data in all spatial and temporal resolution to address every question. However, we need to talk about the data, and the models based on the incomplete data, even if they are deemed insufficient. Once one looks closely, one will find data gaps everywhere, regardless of how many thousands of pages are e.g. in the IPCC reports and the underlying publications. And yet we have to use these scant data bases to make decisions now.

> The same goes for the underlying modelling assumptions and simplifications, of with there are in my opinion too many in the presented work.

Reply: We are happy to replace some assumptions and simplifications with real data but are not aware of the respective studies having been performed yet. For the time being, the models required already now by authorities will necessarily have to be based on a multitude of assumptions and simplifications, and we provide here one example. We look forward to comparing our model results with the results from other models making different assumptions.

> While I did not have the time to go all through all of them in detail, I found the one of linear mortality of groundwater fauna with increasing groundwater temperature insufficiently justified.

Reply: We were also not happy with such a linear mortality (former li. 359f. SI): "However, we lack reliable relationships below 12°C. … . Therefore, we implemented the relationship only for temperatures above 12 °C and set the temperature-related mortality to 0 at temperatures below 12°C. This needs to be refined in future experiments." However, it seemed a large enough stretch deriving the parameters already for a linear model. We did not see a high enough data source to derive a higher order model. Once the experiments have been conducted, we look forward to integrating more appropriate models and parameters.

>Here again, findings from previous studies (e.g. Di Lorenzo et al., 2025) that indicate that fauna can exist at "higher" groundwater temperatures are actively dismissed.

We are at no point dismissing the fact that "fauna can exist at "higher" groundwater temperatures". Fauna in general can exist at higher temperatures if they are adapted to them, or if they are eurytherm, i.e. deal with a wide range of temperatures, which very few stygobites are. In most groundwater studies, organisms are not differentiated enough regarding their preferences toward the proportion of groundwater in their habitat. Stygoxenes, accidentally found in groundwater, and stygophiles, which can cope with groundwater even if it is not their preferred habitat (for the whole life cycle), usually have much wider tolerances towards temperature than stygobites (the competitive inhabitants of groundwater) do (Spengler & Hahn, 2018; Di Lorenzo et al., 2023). Depending on the aquifer, however, the community consists only of stygobites. It would thus be overly optimistic assuming the temperature tolerance of a stygophile, or even stygoxene, to apply to a fully stygobite community.

The more important question than: "is fauna found at a high temperature?" is, how long and how sustainable can fauna "exist" at an elevated temperature. In our opinion, "exist" implies the whole life span. But from a few field data points of fauna having been found at higher temperatures, it is impossible to know whether they can exist at the long-term under these conditions. "Issartel et al. (2005) reported that stygobitic amphipods of the cold-stenotherm species *Niphargus virei* that were maintained at an elevated temperature of 17 °C showed … an increased mortality rate as compared to specimens maintained at a temperature of 11 °C." (Avramov, 2013). That even Middle European groundwater fauna might dip into water nearly 22 °C (*Niphargus inopinatus*) or even 30 °C (*Proasellus cavaticus*) was shown in Avramov (2013) and Brielmann et al. (2011) and we are of course fully aware of this study:

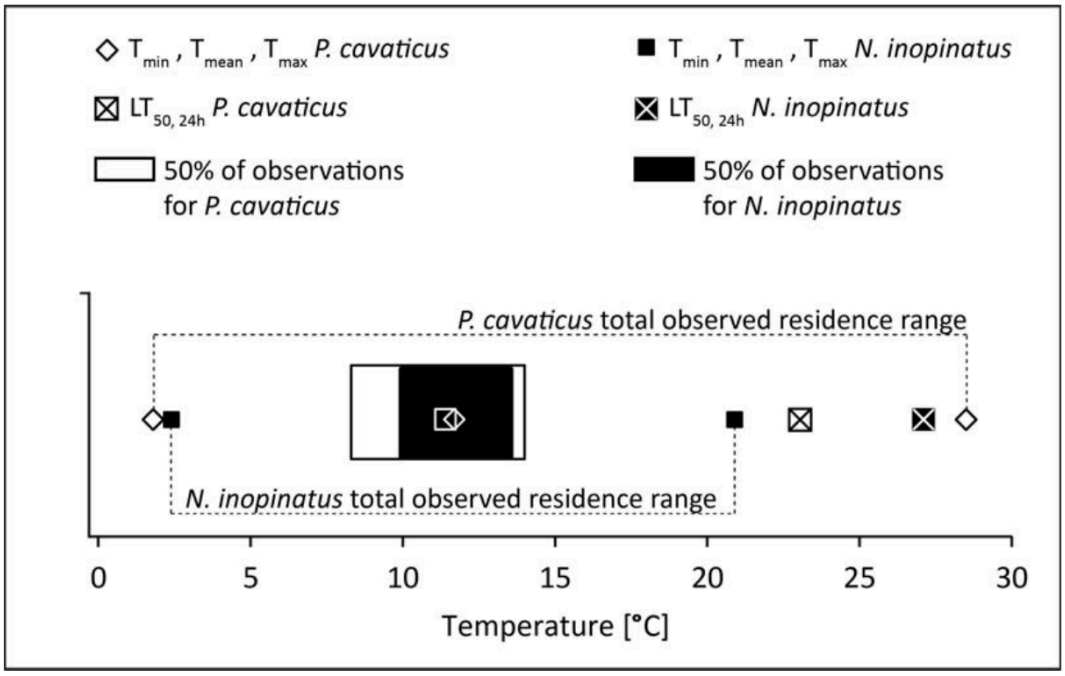


Figure 3: Preferred temperature ranges of the amphipod *N. inopinatus* (filled symbols) and the isopod *P. cavaticus* (empty symbols) in a temperature gradient ranging from 1.8 to 36 °C. The behaviour of each species was observed in a separate experiment. The dotted lines show the entire range of temperatures that were visited by either species. The boxes in the middle of the plot represent the interquartile range (Q_3-Q_1) and show the temperature range where 50% of the observations were made, *i.e.* the range of the most frequently visited temperatures. The LT_{50} values were assessed in temperature dose-response experiments in a separate study (see section 3.2.1).

(Brielmann et al. 2011)

But both tested taxa were found for 50% of the time at around 12 °C, the temperature of their natural habitat. They can thus survive for less than 50% of their time at higher OR lower temperatures, which is not the same as "existing" for the whole duration of their life. In our modelling study, we were interested in longer-term temperature regimes, not short-term survival.

Di Lorenzo et al. (2023) are also fully aware of this study by Brielmann et al.: "Groundwater species have a low tolerance to heat. Increased temperatures have been shown to cause stress and even death to several groundwater species. For example, the groundwater amphipod *N. inopinatus* is not able to withstand temperatures much higher than those of the GWT at its collection site (12°C) for a long time. In an experiment carried out by Brielman et al. (2011) in a chamber with a temperature gradient of 2-35°C, *N. inopinatus* was found positioned at temperatures between 8 and 16°C in 77% of the observations (one observation every 30 min for a total of 9 h), with a mean residence temperature of 11.7 +- 3.4°C."

Most stygobites have been shown to be stenotherm, i.e. need a narrow range of temperatures. If these organisms come from an Italian aquifer where average temperatures are 14 to 17 °C, it makes sense to conduct experiments at their ambient temperatures, as did Di Lorenzo et al. (2023). And thus, of course the organisms "exist" at temperatures higher than in the average

German aquifer. If, however, you use cyclopoids having grown at an ambient temperature of 12 °C which is typical for middle Europe (e.g. Germany), and submit them to experiments at 14 to 17 °C, this means a stress to the organisms which might compromise the intended test effect. In short: the experiments that Di Lorenzo et al. (2025) conducted are not directly transferable to an environment which is ca. 2 to 5 degrees colder. It seems that Tiziana Di Lorenzo, the main author of the study that you refer to, while suggesting clarfications, in general agrees with this, since she reviewed the paper as well (https://doi.org/10.5194/egusphere-2025-6523-RC1) and did not find fault with our decision of using parameters from a study more transferable to the Johannesaue than her own studies.

Diacyclops crassicaudis crassicaudis, one of the species that Di Lorenzo et al. (2025) used for their experiments, is characterized by them as occurring in the hyporheic zone which "frequently inhabits groundwater". It is thus an opportunist, and may be classed as a stygophile, i.e. a taxon using the groundwater occasionally, but belonging to the surface-near hyporheic zone. As mentioned above, stygophiles and stygoxenes are known to survive wider temperature ranges than stygobites. They do provide ecosystem services as well. But modelling the whole groundwater community with parameters only valid for a fraction of the community would be distortive. We have inserted now an explanation of the stygoxene, stygophile and stygobite classes in the introduction of the main paper (after former line 110) so that the reader is able to follow our reasoning:

"***Subsurface fauna consists of stygobites (i.e. organisms adapted to groundwater, e.g. rather stable temperatures), stygophiles (i.e. organisms coping with groundwater conditions but being home to surface environments such as the hyporheic zone, i.e. surface water sediments), and stygoxenes (i.e. accidental occurrences of taxa that are foreign to groundwater and likely do not reproduce in groundwater). The less exchange the groundwater experiences with the surface, the lower will be the proportion of stygoxenes and stygophiles, and the more dominant is the proportion of stygobites (Schmidt & Hahn 2012). Organisms adapted to the surface, i.e. stygoxenes and stygophiles, will cope with surface temperature ranges, while stygobites are not adapted to temperatures deviating from groundwater temperatures. Stygoxenes and stygophiles, however, will not cope with the food scarcity in groundwater, particularly away from exchange with the surface, and thus, do not play a role in the vast majority of groundwater systems. Here, the aim was to focus on stygobites as the main players in the vast majority of aquifers. Admittedly, the data from the Johannesaue within the narrow zone of river-near aquifers, might not be representative for the vast majority of aquifers. However, we lack data that are representative for those aquifers deciding on our drinking water resources, and thus, we decided to take the data from one of the most comprehensive study and made further assumptions relating to more secluded aquifers with a low range in temperatures and low adaptation of fauna to higher and lower temperatures.***"
We refer to this distinction within groundwater fauna in the discussion also, before former line 508: "***In particular, it is important to reflect the traits (Hose et al., 2022) of not only the common and rather well-studied stygoxenes and stygophiles, but also of stygobites, and the proportion with which they occur, in the model parameters. The data necessary for such assessment are lacking.***"

Some investigations have shown a higher tolerance towards temperatures than the model parameters that we used would imply: "… *N. rhenorhodanensis* … serious long-term effects have been observed: a temperature elevation by 6 °C (again resulting in a 17 °C treatment for

individuals naturally living at 11 °C), led to the death of 50% of the tested specimens within 3 months (Colson-Proch et al., 2010).” (Avramov, 2013).

Indeed, quite a few autecological investigations have been performed with the species *Niphargus rhenorhodanensis* because it occurs frequently. However, many individuals labelled as *N. rh.* differed widely genetically, and thus might in fact be different species (Brad et al. 2015). It is thus difficult to know for certain which autecological characteristics can be attributed to one species *Niphargus rhenorhodanensis*. This might be one explanation why this “species” has been called eurytopic:

„Als euryöke Grundwasserarten können z.B. die Amphipoden *Niphargus fontanus*, *Niphargus rhenorhodanensis* oder der Copepode *Phyllognathopus viguieri* bezeichnet werden (ISSARTEL et al. 2005).“ (DWA, 2007)

Google translation (22 March 2026): „Examples of eurytopic groundwater species include the amphipods *Niphargus fontanus*, *Niphargus rhenorhodanensis* or the copepod *Phyllognathopus viguieri* (ISSARTEL et al. 2005).“

This means that these “species” are known to survive at a wide range of environmental conditions. It is thus not surprising that it was found to be one of the “species” with the highest tolerance towards temperature (Issartel et al. 2005).

While *Niphargus rhenorhodanensis* is usually called stygobite, i.e. requiring groundwater, it was in fact found near the surface and feeding within ditches (Dhomps-Avenas & Mathieu, 1983), i.e. it should be labelled as a stygophile species. Living near the surface instead of deep in the aquifers like the true stygobites, may explain why *N. rh.* showed this relatively wide tolerance of temperatures cited above. Based on the high and well-distributed biomasses it reaches (however, not in Germany expect for Lörrach; DWA 2007), it is justified to use its tolerance towards temperature in models, but using only its tolerance as a given for all fauna would exclude the true stygobites which require more narrow temperature ranges, such as *Niphargus vireis*. Here, we do not want to rely on well-studied stygophile taxa which are adapted to the surface, but want to come closer to a realistic scenario of not only stygophiles, but also stygobites.

>Instead the modelling is based on results from one single study (Brielmann et al., 2011) that was conducted under controlled conditions in a laboratory on just two groundwater species. It is neither clear how far this experiment reflects in-situ aquifer conditions, nor if those two species were actually found at the study site of the manuscript.

Reply: Brielmann et al. (2011) took great care to conduct the laboratory experiment under conditions that reflect *in situ* aquifer conditions: “Jeder Behälter enthielt Grundwasser und etwas natürliches Brunnensediment. Alle Platten wurden abgedeckt, um den Wasserverlust durch Evaporation gering zu halten. Verdunstetes Wasser wurde durch Grundwasser ersetzt. Zu Versuchsbeginn wurden die Tiere kontinuierlich an die verschiedenen Temperaturen akklimatisiert. Die Temperatur- Tox-Tests wurden dynamisch ausgewertet, d. h. nach 24 h und 48 h, wie es für Tox-Tests üblich ist, und zusätzlich über einen weiteren Zeitraum von mehreren Wochen, um der verringerten Stoffwechselaktivität von Grundwasserorganismen Rechnung zu tragen. “

Translation by google translate (22 March 2026):

“Each container held groundwater and some natural well sediment. All plates were covered to minimize water loss through evaporation. Evaporated water was replaced with groundwater. At the start of the experiment, the animals were continuously acclimatized to the different temperatures. The temperature toxicity tests were evaluated dynamically, i.e., after 24 h and 48 h, as is standard for toxicity tests, and additionally over a further period of several weeks to account for the reduced metabolic activity of groundwater organisms.”

We are happy to replace the parameters from Brielmann et al. (2011) by a range of more appropriate values from a range of field studies reflecting a range of organisms, but we are not aware of such studies. We look forward to clarification on details of such studies.

Since we lack the species-wise data from the Johannesaue study after Siegfried Husmann’s untimely death, in fact, we cannot ascertain whether exactly these two species studied by Brielmann et al. (2011) had been indeed present in the Johannesaue as well. We assumed that the parameters derived from Brielmann et al. (2011) from stygobites would be representative enough of those organisms present in the Johannesaue, until we have better data. We have added the following sentence in the Methods section after former line 103: “*While we do not know whether these two species were among those present in the River Fulda floodplain, until we have better data, we assumed the parameters from Brielmann et al. (2011) to be representative enough for a modelling study*.”

>On the same note: there are several recent studies on invertebrate groundwater fauna, which either contradict or relativize the findings presented in the manuscript (e.g. Koch et al., 2021, 2024a, 2024b; Noethen et al., 2024; Becher et al., 2022), and are currently not cited in the manuscript (or the SI).

Reply: We could not detect where these studies “either contradict or relativize the findings presented in the manuscript” and are happy for hints in what way exactly they do. In the meantime, we have made sure to include these valuable contributions, as detailed below for every single contribution.

Where Koch et al. (2021) did not find “clear spatial patterns with respect to land use and other anthropogenic impacts, in particular with respect to groundwater temperature“, this might be due to the study design which mixed various anthropogenic and abiotic factors so that none of the factors can be extracted as a sole influencer. Koch et al. (2021) come to that conclusion as well: “The lack of a statistically significant correlation might also be related to the low number of wells (n = 8 in the forested area) and individuals (n = 46).”

Temperature changes with groundwater and well depth, depending on season, depth, and further factors (Benz et al., 2024). Fauna has been observed to move actively vertically in wells: “move progressively downward ahead of the water column” (Stumpp & Hose, 2013). Where there are vertical changes in temperature, fauna will move accordingly to the preferred level of temperature, as observed in a temperature gradient (Brielmann et al. 2011). It is thus paramount to correlate fauna to the whole range of temperature in the wells throughout the year. It is not sound to correlate fauna which survives for decades, to only one temperature measured at one particular day and expect correlations. This does not change the fact, however, that fauna experiences temperature as life-threatening – the question is how to

sample the exact environment of the individual. This is easier in laboratory experiments, which, however, introduce further and other vagaries as we all know. After all, as Koch et al. (2021) cite: “Moreover, it is assumed that groundwater fauna can usually cope well with short-term changes in physical–chemical parameters (Griebler et al., 2016)”. The stress is here on “short-term”. Organisms can survive most poisons for short moments – however, in this study we are not interested in surviving for 5 minutes, but for a life-time.

To discuss the findings of the important contribution by Koch et al. (2021), we have added the sentence “*On the regional and landscape scale, clear correlations between faunal associations and temperature have so far been missing, which is mostly due to the number of influencing factors (dissolved oxygen, geochemical properties, varied toxicants, land use and development over time for all such factors, and geological background, size of catchment) in relation to the often low numbers of samples (Koch et al. 2021*).“ after the former li. 96.

Koch et al. (2024a) is an important contribution, and we added the following sentence at the end of the first paragraph of the introduction, after former line 30: “*Where groundwater communities decline or vanish, this may compromise an aquifer’s ecosystem and its function, leading to deteriorating groundwater health (Koch et al., 2024a).”*

We changed (here: bold italics) the before-last two sentences of the first paragraph of the discussion, after former line 324 as follows: „*Our knowledge of the groundwater ecosystems’ processes and dependencies is still patchy* ***because field studies are usually conducted in hydrogeologically and hydrochemically heterogeneous aquifers with low numbers of monitoring wells, with sampling too rare in time so that local influences and site-specific parameters linked to variations in topography and geology superimpose effects from single variables such as temperature and oxygen (Koch et al., 2024a)***. Thus, the choice and development of appropriate parameters is crucial *but virtually impossible based on the available field and laboratory data without far-fetched assumptions*.”
Even if we took the lack of correlation between temperature and faunal variables in Koch et al. (2024a) at face value, we would ignore what the same authors write in the conclusion: “Transferability of local observations to a larger scale is very limited due to small-scale heterogeneities in hydrogeological conditions and the superimposition of site-specific effects.”
And in the following sentence, the same authors state: “Noticeable environmental changes for wells in Baden-Württemberg were often linked to changes in the dissolved oxygen content, well water temperature and sediment content.” They do not become clearer than this, but indeed at no point in Koch et al. (2024a) does it become obvious that temperature can be believed to have a minor, or even no, influence on fauna at all.
The lack in transferability applies to our study as well, of course. It appears that we had not made that clear enough. We have therefore added the following sentence as the new second sentence in the conclusion, in former line 532:
“*The variables and parameters used in this modelling exercise will have to be confirmed and adapted by more detailed field studies to ascertain transferability of the model.*”

We cannot see where Koch et al. (2024b) indicate that fauna does not respond to temperature, on the contrary this publication contains the statement: “Additionally, anthropogenic climate change poses a significant threat to groundwater ecosystems … due to … increased temperatures”.
We have, however, added the citation to the very first sentence of the introduction.

We have added a reference to Noethen et al. (2024) in the third-before-last paragraph of the introduction: "*In extreme cases, in summary, a temperature increase of a few °C may be lethal to groundwater fauna (Vaccarelli et al., 2023). Groundwater fauna is thus threatened from extinction* ***(Noethen et al., 2024)****, like, globally, a quarter of freshwater fauna (Sayer et al., 2025).*"

Becher et al. (2022) write: "Stygobionts are ectotherms with narrow thermal tolerance ranges … Brielmann et al. (2011) found *Niphargus inopinatus* to be more tolerant to temperature elevations than isopod *Proasellus cavaticus* (Fig. 3a). LT50 values of both species decreased the longer the test lasted (Brielmann et al., 2011). These results are in agreement with the observed narrow thermal tolerance breadths that apply to two *P. cavaticus* morphospecies (*Proasellus* n. sp. 1 and *Proasellus* n. sp. 2) reported in Mermillod-Blondin et al. (2013). The unaffected physiological activities of *D. belgicus* may be due to adaptation to groundwater temperatures of 14–15.9 ◦C in a Mediterranean shallow aquifer and the wide geographical range of the species (Di Lorenzo and Galassi, 2017)." This is in line with our assumptions and discussions, and we therefore added a reference in the third sentence of the introduction.

>Robustness of the approach:

>Throughout the entire manuscript a critical reflection or discussion on the mentioned underlying modelling assumptions is missing.

Reply: We are not sure at what points the reviewer is missing the critical reflection on assumptions, as we are mentioning e.g. in the introduction: "Since it is hard to derive the relevant data sets on the required temporal and spatial resolution, while controlling for different environmental settings globally, one way to approach answers to these questions are models. Here, we present a model, as a proof-of-principle approach, based on data from one of the most comprehensive data sets on a groundwater ecosystem (Husmann and Marxsen, 1988; Marxsen et al., 2021)." I.e. we are fully aware that a 40 year old data set which was performed at a time when molecular methods were not common in ecology, cannot be state-of-the-art – and yet, unfortunately, it remains one of the most comprehensive, longest-running data set. It is not for lack of trying to acquire funding for extending data bases to improve data. However, unfortunately, not enough groundwater studies are being funded and thus, data are sorely missing.

Further, we indicate in former line 519:

"This study was based on a plethora of assumptions most of which might trigger discussions. Also, here, only average compartment carbon concentrations were modelled. It is relevant to focus on the whole range of likely conditions, i.e. including minima and maxima. However, because of error propagation, this is not a straightforward process and requires diligent development. The present study aims to serve as a starting point for future developments."

> **In my opinion, it is not sufficient to simply state that models reflect the field data "satisfactorily", "well" or "appropriately" (e.g. lines 13, 232, 329, 387). Error metrics**

are shown in Fig. 2, but not discussed in the main text. Likewise, the number of data points ("N") is shown, but also not critically reflected upon. In particular, for groundwater fauna, the number of data points is very low (between 1 and 4). This raises severe concerns about the robustness of the approach.

We have added the following sentence in former Li. 219: "*The error metrics in Figs. 2, 4, and 5 all indicate a worse-than-desired fit*".

In the discussion, we added in former Li. 508: "*The error metrics between measured data and model fits were not satisfactory which was to be expected with the scant data base (low N of measured data). We hope that this lack in fit prompts the necessary investigations and improvements on the model in more comprehensive studies following on this proof-of-principle study. In particular, it is important to reflect the traits (Hose et al., 2022) of not only the common and rather well-studied stygoxenes, but also of stygophiles and stygobites, and the proportion with which they occur, in the model parameters, to show robustness of the approach. The data necessary for such assessment are lacking.*"

These sentences in addition discuss the too-low N and robustness. We are happy to see, or be involved in, studies that improve the poor data base.

>The extrapolation to global scale (section 4.8) is absolutely absurd from a hydrogeological perspective. You cannot extrapolate findings from a few wells in a local floodplain aquifer to the entirety of global groundwater, whilst ignoring basic fundamental needs of invertebrates for e.g. pore space and dissolved oxygen.

Reply: We appreciate the reviewer's comment on this bold extrapolation. We are of course aware that we were making a huge number of assumptions here and we comment on that in the main paper and in the SI. Also, we agree with Koch et al. (2024a) statement: "Transferability of local observations to a larger scale is very limited due to small-scale heterogeneities in hydrogeological conditions and the superimposition of site-specific effects."

However, where more elaborate data are missing, it is not uncommon to make such assumptions when upscaling to the global scale, e.g. for climate projections as communicated by the IPCC etc. Such assumptions are always difficult to accept when one knows the caveats and details. Since this global upscaling is not the main point of this contribution, we deleted it from the paper for the sake of clarity and for the sake of being more concise and moving more material from the SI to the main paper. We agree with the reviewer that such extrapolation would be improved by more elaboration on parameters and variables which was out of scope for this contribution – the reviewer already commented on the length and extent of the SI which would rather multiply when incorporating all necessary information for a global extrapolation.

> **Even within Germany, a large portion of shallow groundwater does not contain enough dissolved oxygen to sustain invertebrate life (see e.g. Kunkel et al. 2004), correspondingly many sampled wells were simply not inhabited by groundwater fauna (e.g. Koch et al., 2024a).**

We cannot find where Kunkel et al. (2004) specify which amount of dissolved oxygen sustains invertebrate life. They do say: “Im Allgemeinen liegt die Konzentration des im Grundwasser gelösten Sauerstoffs zwischen 2 und 8 mg O2/l.”

Google translation (30 March 2026): „Generally, the concentration of dissolved oxygen in groundwater is between 2 and 8 mg O2/l.“

That is a concentration that stygobites have no trouble with, and even most stygophiles can deal with (Notenboom et al. 1992; Malard et al. 1999; Mösslacher et al. 1998; Datry et al. 2005). Hahn (2006) states that fauna requires dissolved oxygen above 1 mg / L. Such a state is present in most aquifers. In a recent compilation, Marmonier et al. (2023) state: “An overview of adaptive strategies of groundwater crustaceans to withstand hypoxia and food deprivation (Hervant and Renault, 2002; Hervant and Malard, 2019) suggests that many species are probably able to experience highly variable oxygen concentrations”. It becomes thus clear that the importance of dissolved oxygen for groundwater fauna is often overrated, but the importance of temperature is often ignored.

With the average faunal density that we used for upscaling, we of course took into account the fact that (with the usual methods), fauna is not found in every sample (e.g. Koch et al., 2024a) – which might mean that fauna does not exist at the place where the sample comes from or that the method was not appropriate for the scale at which fauna occurs. This is yet another insecurity in field data that we cannot clarify with the existing data. However, until we have better data, we have to base models on the existing data.

>Also, the mentioned implications for drinking water supply and treatment are too general and vague in my opinion, and again they are mostly not referenced.

Reply: we referenced the UNESCO aims in the introduction:

“These functions are one prerequisite for sustainable drinking water production, which is one of the 17 goals (goal 6) of the 2030 Agenda for Sustainable Development (United Nations General Assembly, 2017).”

We added a reference to Schmidt et al. (2024) in the sentence at the end of the second paragraph of the introduction: “*However, we urgently need to quantify the impacts of temperature on groundwater ecosystem services since we may have to future-proof drinking water production if groundwater ecosystems change their functionality due to increasing temperatures and changing climates and land uses* ***(Schmidt et al., 2024)***.”

We added two references in the sentence “*In drinking water production, it is advantageous to have raw water with low organic substances* ***(Moona et al., 2021)*** *so that little respiration (which would reduce the dissolved oxygen) takes place in the pipes on the way to the customers* ***(Kekes et al., 2023)***.

In the sentence “*Thus, high raw water quality, with low levels of carbon, begins in the aquifers with a community of active organisms, degrading the recharged carbon and ensuring high oxic state throughout the drinking water production*.” we added the references ***Schubert et al 2002, Fowler & Smets, 2017***.

>Not all groundwater is drinking water, and vice-versa. Before highlighting such aspects in the conclusions, robust background information on this have to be shown and referenced in the manuscript.

We certainly agree that "Not all groundwater is drinking water, and vice-versa." However, we are losing more and more of those groundwater reserves previously used for drinking water production so that we must shift to new groundwater resources for drinking water production. Thus, groundwater previously not used for drinking water production might have to be used for it in the future, due to increasing population, increasing water demands, increasing pollution of existing drinking water reservoirs, etc.

>Clarity:

>The manuscript is built around just two figures: one showing the fit of the hybrid model to the measured data, another showing the scenario analysis. While these figures are presented in a clear way, there is almost no information about the actual site and the used data in the main manuscript.

We have moved some information from the SI to the main paper, see also your last comment, but I think that we agree that we cannot move the whole SI to the main paper. We also do not see which parts of the SI could be deleted without the contribution suffering in clarity, but we are of course open to suggestions. In the meantime, Figure S2 has been incorporated as Fig. 1 in the main paper, and SI 1 has been deleted since it thus was obsolete. The remaining SI sections were renumbered accordingly. We also moved the two last figures of the SI into the main paper, as the new figures 4 and 5. This reduces the SI to 27 pages i.e. less than a third of the length of the SI in Koch et al. (2024a).

>The introduction lacks a clear structure leading to the hypotheses, and contains several aspects such as drinking water pipes, piscivorous fish and bark beetles, which seem unrelated to the actual topic.

Reply: We are sorry that the reviewer misses a clear structure and are happy to see recommendations how to improve the structure. Meanwhile we have improved the structure where we saw fit.

In this contribution, we stress the importance of groundwater fauna to drinking water production, UN goal 6 of the 2030 Agenda for Sustainable Development (United Nations General Assembly, 2017)" aim which involves drinking water pipes. We therefore do not think that it is irrelevant to refer to the installation of pipes during drinking water production, where the ecological features within pipes may influence tap water quality.

Where we mention piscivorous fish and bark beetles, it is to refer to ecological phenomena which we assume to apply to groundwater, as well, or have effects on groundwater, until proven otherwise.

>The results and discussion contain many references to the different zones R, M, P, and A in the study area, which are neither explained, nor justified in the main manuscript.

Reply: The four zones are explained in the introduction, former lines 123 f: "Marxsen et al. (2021) grouped the wells in the River Fulda floodplain into four groups: one group combining wells near the river Fulda and in intense exchange with the river ("R"), one group covering wells that were characterized by a plume of a carbon source from the near-by village ("P"), one group characterized by agriculture with elevated nitrogen and phosphorus concentrations ("A"), and a group representing wells of a mixing zone being situated in-between the other three zones and combining characteristics of all zones ("M")."

We are open to recommendations how better to inform the reader on the four zones, and have in the meantime copied most of this information also to the Methods section, since it needs to be placed near the - now - figure 1: "*One group among the four combined river-near wells which were in intense exchange with the river ("R"), one group covered wells dipping into a carbon plume from a near-by village ("P"), one group was characterized by agriculture which showed in high nitrogen and phosphorus concentrations ("A"), and the fourth group represented wells of a mixing zone ("M")*."

We have now moved the map which was in the SI, to be the new Figure 1 in the main manuscript and refer to it with this sentence.

>Also, I found it impossible to follow the description and discussion of the results without cross-reading the lengthy (29 pages) SI on a second screen.

Reply: Ecological studies are so complex that the usual length of a research article cannot do justice to all the details of the methods and necessary background information. Many papers are less than 20 pages long, with often more than 20 pages of SI. One of them is Koch et al. 2024a that you drew our attention to:

Koch, F., Blum, P., Korbel, K., and Menberg, K.: Global overview on groundwater fauna, Ecohydrology, 17, e2607, https://doi.org/10.1002/eco.2607, 2024a. (**86 pages of SI**)

Further important papers with extensive SI of more than 20 pages e.g. in the field of groundwater, hydrogeological patterns, and ecology include e.g.:

Benz, S. A., Irvine, D. J., Rau, G. C., Bayer, P., Menberg, K., Blum, P., Jamieson, R. C., Griebler, C., and Kurylyk, B. L.: Global groundwater warming due to climate change, Nat. Geosci., 1–7, https://doi.org/10.1038/s41561-024-01453-x, 2024. (**38 pages of SI**)

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Melkonian, C., Fillinger, L., Atashgahi, S., da Rocha, U. N., Kuiper, E., Olivier, B., Braster, M., Gottstein, W., Helmus, R., Parsons, J. R., Smidt, H., van der Waals, M., Gerritse, J., Brandt, B. W., Röling, W. F. M., Molenaar, D., and van Spanning, R. J. M.: High biodiversity in a benzene-degrading nitrate-reducing culture is sustained by a few primary consumers, Communications Biology, 4, 1–12, https://doi.org/10.1038/s42003-021-01948-y, 2021. (**62 pages SI**)

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We have also moved some information from the SI to the main paper (see above), but I think that we agree that we cannot move the whole SI to the main paper. We also do not see which parts of the SI could be deleted without the contribution suffering in clarity, but we are of course open to suggestions. In the meantime, Figure S2 has been incorporated as Fig. 1 in the main paper, and SI 1 has been deleted since it thus became obsolete. We also moved the two last figures into the main paper, to become Figures 4 and 5. This reduces the SI to 27 pages i.e. less than a third of the length of the SI in Koch et al. (2024a).

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Response to the editor

We thank the editor for checking the two rather contrasting reviews in detail and for giving us the opportunity to make the best possible compromise.

>Both reviewers highlight that the topic of the manuscript is timely and relevant. But while reviewer 1 is overall positive, reviewer 2 is more critical and sees a mismatch between the scale at which the model is presented (applicable beyond the study site) and how is parameterized and validated, using mostly data from a local case study.

We thank the editor for this summary.

>I suggest that the authors revise their manuscript, after which I will send it out again for review. In addition, below I have added some additional comments and suggestions related to the concerns of reviewer 2, which might help to further address these concerns. Note that I am not an expert on the topic as the reviewers are, so I am trying to do this from the 'outsider's perspective'.

Thank you very much for all the effort on our manuscript.

>One way to clarify the qualitative and comparative nature of the study would be to add a subtitle including the phrase 'proof-of-principle'. E.g. simply, a colon followed by 'a proof-of-principle approach'.

We have changed the title according to the suggestion to "Quantifying the impact of groundwater fauna and temperature on the ecosystem service of microbial carbon degradation*; a proof-of-principle approach*" in line 2f.

>I agree with reviewer 2 that a more solid discussion of error metrics is warranted (more than just a single sentence as the authors propose in their response), especially for those cases where the number of data points is higher than 4.

Regarding the discussion of the error metrics see also the respective comments to rev. 2. In particular, major changes include a new paragraph at the beginning of the – now – section "4.8 Necessary steps for further development" (li. 632).

In particular, we have now added in – now – line 309 ff. in the results: "*In particular, for the fauna data for the groups river-near "R" and agriculturally-influenced "A" there were only data available from 1 or 2 dates, respectively, so that the error metrics could not even be calculated*".

>I also agree with this reviewer that section 4.8 weakens the manuscript – and support the reply by the authors to remove this section.

The former section 4.8 was removed, as suggested by reviewer 2 and the editor.

> In addition, I think the discussion on the sensitivity analyses can be broadened by also including the uncertainties on the parameterizations. Yield and k1 may be the both sensitive parameters, but how does that link to the processes which inclusion is most uncertain to begin with? (as now touched upon in section 4.9) That may be especially relevant when applying the model at other locations, as the authors ultimately aim for.

In the discussion, we added in – now – li. 634 ff.: "*For the comparisons in the fauna data for the groups river-near "R" and agriculturally-influenced "A", for which data were only available from 1 or 2 dates, respectively, the error metrics could not even be calculated. The overall low number of fauna data are the weakest point in this data set. This weakens of course the conclusions drawn from the modelling results for fauna. However, the field data show a wide variability which seems adequate when compared to field data from e.g. a 60 km^2 field site (Schmidt et al., 2007), a similar size field site in the River Isar valley (Brielmann et al. 2009) or from a region-scale investigation in Western Czechia (Schmidt et al. 2024). Thus, we believe that the range of fauna abundances would have been not much larger with more frequent sampling data available. The trends over time cannot be evaluated with the in situ data set, but the modelled trends reflect what has been measured in monthly data e.g. in Schmidt et al. (2007). We thus assume that the sampled data and the modelled time course are representative for what was really happening, until more comprehensive data show the opposite*".

Uncertainties in parameterization within the sensitivity analysis were not mentioned in the manuscript so far, which was indeed an oversight. We thank the editor for pointing this out. To discuss them we changed the sentence in li 345 ff. (changes in italics) to
"To demonstrate the influence these parameters indeed had on the model, *and thus to narrow down uncertainty when using particular values for parameters,* we performed two additional runs *each* for the *two* most influential parameter, with a minimum and maximum value each tested for sensitivity, respectively (see model parameters in "parameter.xlsx" in the "gtm" (groundwater trophic model) repository on github: https://github.com/suisch/gtm_River_Fulda_floodplain.)."

Also, we added and changed a few words in the respective results section in the – now – line 370 ff. so that it now reads "Since microbial yield on carbon *and k1* proved so sensitive, we ran the reference scenario *(recent temperature, fauna present)* with the minimum (0.0001; Fig. *4*) and maximum (0.8; Fig. *4*) microbial yield tested in the sensitivity analysis, *and the minimum (0.000001; Fig. 5) and maximum k1 (0.001; Fig. 5), to evaluate uncertainty."*

We added table S3 and the following sentences after the new line 372 ff.: "*The fitted linear model trends for these extreme scenarios and the reference scenario for comparison are listed in Supporting Information S10, Table S3."*

And we added in now line 384 ff.
"Both the minimum k1, i.e. minimum partitioning of detritus into biodegradable organic carbon, and the maximum yield, i.e. efficiency in transforming the taken-up BOC into microbial biomass, resulted in up to 100 times less BOC left over, depending on the group (Table S3). High k1 and low microbial yield increased left-over BOC by a factor of up to 33, depending on the group (Table S3)."

Sentences and half sentences were added in the discussion in section 4.6 beginning with – now – li. 574 ff., including discussion of the new diagrams, so that it now reads (new text in bold):
"4.6 Sensitivity analysis
The two parameters showing higher sensitivity in one group each than other parameters were the microbial growth parameter yield and the k1 (proportion with which detritus split

into biologically oxidizable carbon). It is thus paramount to derive these two parameters carefully, to exclude artefacts, since we are showing here that changing microbial yield may change the outcome of the model considerably for BOC. *After the study period, minimum k1 and maximum yield had reduced the BOC left over from the feeding network, depending on the group, while high k1 and low microbial yield had largely the inverse effect. There is thus high uncertainty for BOC, depending on these two most sensitive parameters. The uncertainty introduced from k1 was high, particularly for the mixing "M" zone and extremely high k1 values. The excess BOC was apparently not used by the food web and would be imported into drinking water production facilities, requiring more treatment than where the food web uses BOC efficiently.*

The uncertainty from the tested range of the microbial yield was more complex and the four zones differed strikingly. The uncertainty was low for the groups "R" and "A", meaning that different microbial taxa or different states of the taxa with a varying yield would not have led to high differences in remaining BOC. Changing the microbial community, e.g. by biostimulation or bioaugmentation would not have influenced the degradation much in these two zones. For the plume zone "P", all three scenarios reached a minimum for BOC between early 1978 and mid 1979 from which both extreme scenarios picked up earlier than the reference scenario. All three scenarios would thus have led to a quite efficient use of BOC within the first year.

However, the low yield with high substrate use, and thus possibly steep biomass increase within the first months might have led to increased grazing by fauna which would, in turn, have kept a check on the increase of microbial biomass compared with the low yield and the reference scenario, leaving more BOC unused. In contrast, the high yield and thus low substrate use led to the latest minimum in BOC. The then low microbial biomass would have meant that fauna had less to graze than in the reference scenario, and the microbial biomass increased with a delay compared to the low yield scenario. The reference scenario kept the BOC at a minimum value for the longest time. Microbial biomass was using virtually all BOC introduced from detritus for the longest time, probably without losing too much biomass to grazing. This reference scenario would thus be the preferred scenario when trying to have good quality raw water for drinking water production.

In the mixing "M" zone as well, low yield with high substrate use, led to the fastest decrease in BOC, while high yield and thus low BOC use, led to the slowest decrease in BOC. After minimum values between 1978 and 1979, BOC concentrations again increased fastest with the low yield scenario, like in the plume "P" zone, probably for similar reasons. Here, however, the most effective scenario for keeping BOC low was the high yield scenario. The coinciding high substrate use under the "M" zone circumstances of the relationship between start concentrations of microbial biomass and BOC might have led to microbial biomass remaining low, but being just enough to sustain fauna which rejuvenated microbial growth optimally, depleting the resulting BOC concentrations. In follow up studies, the food web will be followed for a longer period, to check whether there are longer amplitudes in the growth and decline of BOC and microbial dry mass, than visible in the study period.

It needs to be noted though that particularly facultative oligotrophs are known to exhibit different growth parameters when facing different substrate concentrations (Ishida et al., 1982). This will be hard to parameterize representatively *for an entire prokaryote community*

and questions how important exact microbial growth parameters can *really* be, *and which uncertainty will remain even if parameterization has improved beyond this proof-of-principle study. It is hard to tell which process, physical degradation of complex organic molecules to become microbially palatable sources, or microbial efficiency (yield) is the most important. Because the growth parameters depend on substrate concentrations (Ishida et al., 1982; see above), an interaction between the two is more than likely. The form of the interaction will determine whether uncertainty will be a sum of the uncertainties of the two most sensitive parameters, or more or less than that***."**

>I also agree with reviewer 2 that the introduction could be written more clearly and think it can be more clearly 'funnelled' from the broad problem to the specific research question. One example is L. 24-37 which goes back and forth between ecosystem services and food webs, and the role of temperature is suddenly introduced in L. 34-35, which then only comes back in L.77. My suggestion would be to start with ecosystem services, then proceed with the role that the food web plays in performing these ecosystem services. I'd only introduce the role of increasing temperature from L.77 onwards.

We appreciate very much the clarification on how to improve the introduction. This clarification enables us to provide more details on our reasoning. Our approach had been to provide a first paragraph in the introduction which already lines out where the introduction will ultimately lead to, which required introducing the concept of dependence on temperature already early on, then go into all points raised in the first paragraph in detail and ultimately also introduce temperature in more detail. This approach does not seem to convince and therefore we now followed the editor's recommendation and moved the last two sentences of the first paragraph to the – now – line 75 ff. We also moved explaining ecosystem functions to line 72 ff., beginning with the former first two sentences of the introduction.

We deleted the mention of "and presumably function." from now line 125.

We replaced the beginning of the introduction by the following two paragraphs, beginning with li. 25
"*Groundwater, i.e. sensu lato the water beneath the surface, is alive. Microorganisms are found almost everywhere in groundwater, adapted to different conditions. They produce biomass which may be the food source for other organisms such as protozoa and fauna.*

Subsurface fauna consists of stygobites (i.e. organisms adapted to groundwater, e.g. rather stable temperatures), stygophiles (i.e. organisms coping with groundwater conditions but being home to surface environments such as the hyporheic zone, i.e. surface water sediments), and stygoxenes (i.e. accidental occurrences of taxa that are foreign to groundwater and likely do not reproduce in groundwater). The less exchange the groundwater experiences with the surface, the lower will be the proportion of stygoxenes and stygophiles, and the more dominant is the proportion of stygobites (Schmidt & Hahn 2012). Stygoxenes and stygophiles, however, will not cope with the food scarcity in groundwater, particularly where there is little exchange with the surface, and thus, they do not play a role in most groundwater systems. Here, the aim was to focus on stygobites as the main players in most aquifers."

Thus, this introduction clarifies what groundwater fauna actually is.

This is followed by the introduction to groundwater food webs beginning in line 38.

The role that the food web plays in performing these ecosystem services begins in line 73.

The role of temperature for abiotic conditions is now introduced first in line 80, and the role for organisms is elucidated in the paragraphs beginning in line 110. We hope that it is now more straightforward to follow the train of thought.