Comments on Reviewer2

1. Lack of research question, hypothesis, and conceptual focus

We have revised the manuscript to clearly articulate the larger goal of the research initiative that this study is part of. Specifically, we now state that the study aims to determine the natural state of the aquifer, providing the baseline hydrogeochemical and microbiological characteristics of the aquifer system prior to large-scale water abstraction.

We have clarified that the research question focuses on whether microbial community structure, i.e. “microbial fingerprint”, combined with selected chemical and isotopic tracers, can provide insights into groundwater origin, age, and connectivity within a complex buried valley aquifer system. The hypothesis is that microbial community structures, when integrated with multitracer hydrogeochemical approaches, offer a novel tool for characterizing flow dynamics and aquifer connectivity. These additions explain why this specific suite of tracers and microbiological approaches was selected: to evaluate their applicability and compatibility for aquifer characterization and to support sustainable groundwater management. We believe these revisions address the concern that the study previously appeared as a catalogue of measurements and strengthen the interpretation and relevance of the results.

2. Insufficient treatment and interpretation of noble gas, CFC/SF6, and 3H/3He data

The tracer dataset contains internal inconsistencies that remain unexplained:

•Ne concentrations exceed air-saturated water (ASW) values by 121–427%, which is far above typical excess-air ranges expected for such aquifers.

Yes, this is an unusual feature. 7 of 10 He samples were measured in duplicates. Duplicates result in identical data. Because of the large 4He concentrations the amount of gas was split in the measurement system to achieve the proper range for calibration of the signals. This resplitting reduces also the amount of Ne in the device and results in slightly larger errors. While typical error for Ne is about 1% for concentrations near equilibrium, the errors increase to about 1% of value received for the He concentration. I.e., sample "R56 bedrock borehole" with the highest 4He concentration of 1E-1ccSTP/kg created an error for Ne of about 1E-4ccSTP/kg which is exceed by far any plausible concentration. Larger errors for Ne the He concentration beyond these indicate some problems with sampling. The Ne concentrations should not be overrated.

- Some CFC and SF6 values also exceed ASW by large margins.

For the interpretation of CFC and SF6 concentration the amount of excess air is critical. This is usually derived from Ne concentration. Here, this method fails completely and CFC and SF6 data should not be used. Also, SF6 is in general produced in the rock matrix with large rates and should be applied rated here. We have added these notions to the text.

> - Dissolved O2 values reach several hundred percent of ASW (23–401%), including values incompatible with waters sampled at depths up to 160 m.

There were errors in the Table 2. We had mistakenly used the oxygen saturation percentage values for the two “supersaturated” readings of dissolved oxygen. The concentration (mg/l) values are for KUU19 **5.65** and for HÄJY11 **3.14**. The confusion is from the fact that the readings from the YSI meter are displayed both as the concentration and as saturation percentage values for dissolved oxygen and from these two sites, the DO% was marked down to field notes. See also discussion about the high Ne excess.

> - Tritium values are inconsistently reported, and negative 3He* values appear to have been omitted without comment. When applying the unfractionated air (UA) model to the published data, similar 3H/3He results can be reproduced; however, this approach also yields negative 3He* values, which seem to have been excluded from the table without explanation.

Tritiogenic 3He cannot be clearly separated from other 3He sources if 4He concentrations are as large as 5E-4ccSTP/kg. Here data are neglected. Only for samples NOPPA15, KUU19 and HARJA10 3H-3He-age could be derived. We will add a clarifying figure on the relationship between the 3He/4He and the 20Ne/4He ratios in groundwater from the study site in the manuscript.

> - *Such large oversaturations in O2, Ne, CFC, and SF6 could stem from sampling artefacts, air contamination, or analytical issues. If real, they require detailed mechanistic discussion. As presented, the tracer dataset cannot be reliably interpreted.*

See the above discussion. We agree with the reviewer that there were major issues with residence time indicators, and we give explanations to these in the manuscript and advise on careful assessment before using these in the future.

3. Lack of methodological transparency

More detailed information has been added to the Materials and Methods section regarding the field sampling, as well as the calculation of tritiogenic and terrigenic Helium.

4. Insufficient integration between microbiological and tracer datasets

The redox conditions as well as all samples being hypoxic (there were two incorrect values for DO in Table 2.) at each sampling site are now incorporated into the microbiology discussion, with reflection to the potential cycling of specific elements (C, S, Fe, N). However, as there is no knowledge on the speciation of iron and only total sulphur and SO4 have been measured (sulphite concentrations are lacking), in addition to the limits on making conclusions on microbial metabolic activity from the taxonomy, this somewhat hinders the discussion. Nevertheless, we have now combined the microbial diversity data (Shannon H') with the microbial community structure figures and plotted the relative abundances of known sulphate reducers in samples with the sulphur isotope data to show the relationship between the samples that hosted significant proportions of sulphate-reducers (e.g., *Desulfovibrio*, *Desulfosporosinus*, *Desulfurivibrio*) and the enrichment of the δ^{34}S values.

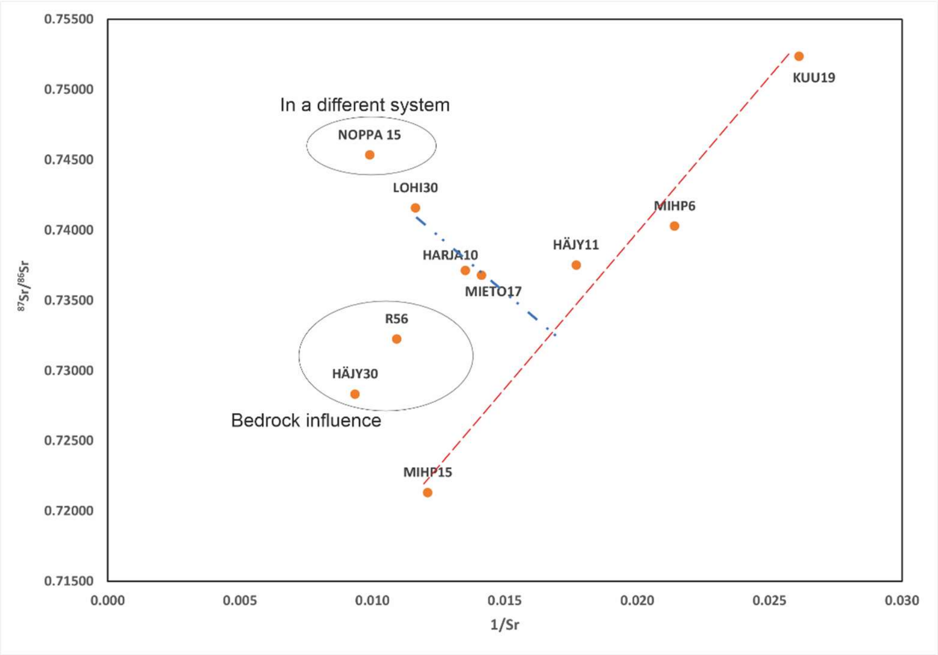
The reviewer's notion on the air contamination can be somewhat relevant when considering the DO measurements. These were done in field conditions, pumping the fluids from the aquifer to the surface, and the measurement, although done immediately from the pumped fluids, some oxygen from atmosphere may have dissolved to the fluid. However, as the DO values show hypoxic conditions and most samples are also on the reductive side, we concluded that the groundwater sampled here is most likely oxygen-depleted. Regarding the notion in the reviewer's Figure 3 about the anaerobic fungi, there are several publications that describe diverse fungal communities in hypoxic or anoxic groundwaters in deep terrestrial subsurface as well as subseafloor crust and sediments (Sohlberg et al. 2015, Drake and Ivarsson 2018, Purkamo et al. 2018, Inkinen et al. 2019, Velez et al. 2022). Nevertheless, despite the limited understanding of fungal metabolic capabilities in groundwater systems (Retter et al. 2024), most taxa we identified belong to saprotrophic lineages that are typically adapted to oligotrophic conditions, may be originating from surface as they are distinct from the deeper bedrock groundwater communities such as those described for example in Sohlberg et al. (2015) and Purkamo et al. (2018).

5. Limited site characterization

Interpreting groundwater flow patterns in this complex, multi-layered aquifer system—partly leaky and partly confined—is challenging outside the recharge zones, which are controlled by bedrock topography. There are two publications describing the conceptual model of the aquifer system referenced (Rashid et al. 2022 and another that has just been published, Åberg et al. 2026 that we can add to the manuscript), and two more on the pipeline that provide more detailed information on the site. We have now incorporated details on the suspected flow patterns to the Figure 1 and added the references to the papers to the site description part. These have also been incorporated to the discussion in more detail.

6. Figures and tables require substantial revision

We thank the reviewer for providing the guiding figures as we have now a more comprehensive discussion on the end-members based on strontium analyses and the binary mixing trends.



The sample point NOPPA15 is located north of the buried valley site, and the water sample represents a different geochemical environment that just happens to look like a continuation of the blue mixing line. However, it is most unlikely that there is a hydraulic connection between NOPPA15 and the other sampling sites. R56 and HÄJY30 show non-conservative behaviour outside the two mixing lines that suggest three endmembers. The two “outliers” are most likely explained by bedrock groundwater influence in HÄJY30, a groundwater well that has no screen, and R56 being a bedrock groundwater well. This seems to create an additional endmember to the three portrayed in the diagram. In a diagram with the reciprocal of the Sr concentration a binary mixing line draws as a straight line. Here the two dotted lines (red and blue) represent binary mixing trends. The samples near the red dotted mixing line (MIHP15, HÄJY11, MIHP6 and KUU19), represent influence of the Paloluoma buried valley modern/young groundwater. The sample points MIHP15 and MIHP6 are located fairly near the Kyrönjoki valley, but the flow of groundwater in the Paloluoma valley to the north has a volume that overrides the influence from the

Kyrönjoki direction (Fig .1 in the manuscript). The remaining samples on the blue mixing line (LOHI30, HARJA10 and MIETO17) represent the Kyrönjoki valley, south of the buried valley system where there is northbound groundwater flow (figure 1). The mixing line does not clearly represent this south-north direction, due to hydraulic connections west of LOHI30.

As this study was conducted with the purpose of extracting information about the aquifer system prior to extensive groundwater extraction, all the isotope geochemistry results can be considered as end-members.

7. Interpretation remains descriptive

Without a guiding question or conceptual framework, the manuscript does not progress beyond description. A hydrogeological model relating the measured variables to flow, recharge, mixing, or redox evolution would greatly improve interpretive strength. Some figures and questions are added here which might stimulate some interesting scientific discussion.

We thank the reviewer for raising this important point. While building a full numerical or analytical hydrogeological model is beyond the scope of the present study, we agree that the manuscript benefits from a clearer conceptual framework. We have therefore improved the description of aims of the study in the introduction: establishing baseline hydrogeochemical and microbiological properties for future monitoring, determining water origin and potential mixing using isotopes, and assessing groundwater residence time and recharge periods. We also included the need to evaluate the potential of multitracer approach, including the microbial community profiling as one tool to the introduction. We have modified the figure 1 as mentioned above, reorganized and rewrtitten the results and discussion to improve the interpretation and incorporated some additional/improved figures.

Additional suggestion:

We have now had additional help for interpretation and the practices in describing the methods from the expert that made the noble gas analyses in the first place, Jürgen Sültenfuss from University of Bremen. He is listed now as a coauthor.