The reviewer’s comment is written in bold. The reply of the authors is written in non-bold.

The data description paper from Bogena et al. (2025) presents a comprehensive dataset of water quantity and quality measurements in the Wüstenbach catchment, part of the TERENO network, and a neighboring reference catchment. Data from these catchments is especially interesting, as they are designed as a paired-catchment study with a >3 year calibration time before forest interventions (i.e., clear-cut) in the Wüstenbach catchment. Therefore, the data have great potential for further investigation on the impact of forest loss on catchment-scale water and solute fluxes, which is a highly relevant topic. To support such analysis, the authors present ways to estimate solute fluxes from grab sample concentration data. The manuscript is well written, and I have no doubt that it will be a valuable contribution to the readers of ESSD. Particularly, I appreciate that all original and processed data are made publicly available.

We sincerely thank the reviewer for the positive overall evaluation of the manuscript and for recognizing the value of the presented data set. We greatly appreciate the time and effort devoted to reviewing our work. In the forthcoming revision, we will carefully consider each of your suggestions and implement the necessary changes.

The only major point I have is that there is no detailed data on the quality of the high-frequency concentration measurements via the optical TriOS sensor. Spectral absorbance data, obtained from optical sensors, need calibration and additional quality checks (such as calibration with grab sample data), which should be provided if the data is to be published.

We agree with the reviewer that the data from the optical TriOS sensor should be calibrated against grab sample data. This calibration had already been performed in our analysis, but we inadvertently omitted a description of the procedure and the outlier detection from the manuscript. In the revision, we will add a dedicated section detailing the sensor calibration process and report the resulting measurement accuracy.

Please see my one major and some minor comments below:

Major:
Chapter 3.3 (starting in line 231): As mentioned above, I appreciate the additional provision of high-frequency concentration measurements, which clearly strengthens the manuscript. However, information on the quality control procedure and its results (calibration, outlier detection and comparison to grab sample data) should be provided so that readers of ESSD can trust and better understand the data.

See comment above.

Minor:
L60-63: with serious consequences for the water quality of rivers (Musolff et al., 2024), reservoirs (Kong et al., 2022), and groundwater (Winter et al., 2025). I recommend adding these two citations.

Thank you for these helpful citation suggestions; we will include them in the revised manuscript.

L194-195: Why linear? Why not other methods like cubic spline interpolation or similar? Is linear reliable enough? I suggest to indicate periods of interpolated Q in Figure 2 or to put an additional figure with those periods into the SI, so readers can see that this does not severely affect the Q time series.

The original discharge time series contained data gaps of varying lengths, ranging from a few minutes to several days, with highly variable distributions. After mutual gap-filling with scaled runoff data from the other stations, the remaining gaps were relatively short. We therefore consider linear interpolation of the described ratios to be sufficiently reliable. However, owing to their short duration, these gaps cannot be meaningfully visualized. Nonetheless, we recognize that some data users may wish to examine the gaps and evaluate the impact of the applied methodology in greater detail. To support this, we will provide two additional data tables for WU14 and WU17 in the data supplement, indicating where gaps have been filled, and will include a corresponding explanatory note.

Figure 4: Is there a reason the x-axes are log10-transformed and the y-axes are not? I suggest log10-transforming the y-axes as well, in line with commonly applied power law C-Q relationships.

Thank you for this helpful suggestion; we will revise the figure accordingly.

Figure 5: I suggest adding the R^2, NSE, etc. directly in the Figure. This would make a first visual assessment easier.

Thank you for this suggestion; but we think that you are referring to Figure A5. We will revise this figure accordingly.

L397: As Figure A5 appears to be quite relevant for the presented results, I suggest adding it to the main manuscript.

Thank you for this suggestion; we will add the figure to the main manuscript.

L462-476: I think the analysis is sufficient for the purpose presented. However, I would add a sentence to inform readers that nitrate export patterns at the long-term (analyzed via low-frequency data) and the event scale (analyzed via high-frequency data) can considerably diverge, because of different mechanisms that dominate at different time scales (Winter et al., 2024).

Thank you for this suggestion. We will add the following sentence to the manuscript: *'It should be noted that nitrate export patterns at long-term scales (analyzed using low-frequency data) and at event scales (analyzed using high-frequency data) can differ substantially, as different mechanisms dominate at different temporal scales (Winter et al., 2024).'*

L484: How much is this mean value influenced by the drought in 2018-2020? I could imagine that this lowers the mean substantially, while other years might have been just as wet as 2024…? In this light, how plausible is an explanation purely based on the wetness state of the riparian zone? Couldn't it be that the decrease in nitrate concentrations might have contributed to this pattern as well, similar to what was argued in Musolff et al. (2017)?

We believe there may have been a slight misunderstanding, as the statement refers to the export of dissolved iron rather than nitrate. While nitrate concentrations generally exhibit a decreasing trend, dissolved iron shows an extraordinary concentration peak in 2024, which was substantially higher than in both drought and normal wet years.

L491-492: I suggest citing Škerlep et al. (2023) here, who also found simultaneous increases of Mn and Fe(II) and related them to changes in catchment wetness and related redox conditions.

Thank you for this citation suggestion; we will include it in the revised manuscript.

L505: Cl- not CL-

Thank you; we will revise the figure accordingly.

Figure 9: It would make it even clearer if the reference catchment were directly indicated as such in the legend, but I leave this to the discretion of the authors

Thank you for this suggestion; we will revise the figure accordingly.

References:

Musolff, A., Selle, B., Büttner, O., Opitz, M., and Tittel, J.: Unexpected release of phosphate and organic carbon to streams linked to declining nitrogen depositions, Global change biology, 23, 1891–1901, 2017.

Musolff, A., Tarasova, L., Rinke, K., and Ledesma, J. L. J.: Forest Dieback Alters Nutrient Pathways in a Temperate Headwater Catchment, Hydrological Processes, 38, e15308, https://doi.org/10.1002/hyp.15308, 2024.

Škerlep, M., Nehzati, S., Sponseller, R. A., Persson, P., Laudon, H., and Kritzberg, E. S.: Differential Trends in Iron Concentrations of Boreal Streams Linked to Catchment Characteristics, Global Biogeochemical Cycles, 37, e2022GB007484, https://doi.org/10.1029/2022GB007484, 2023.

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Winter, C., Müller, S., Kattenborn, T., Stahl, K., Szillat, K., Weiler, M., and Schnabel, F.: Forest Dieback in Drinking Water Protection Areas—A Hidden Threat to Water Quality, Earth's Future, 13, e2025EF006078, https://doi.org/10.1029/2025EF006078, 2025.