This study investigates seasonal eutrophication patterns in rivers from 133 monitoring sites in Germany. It's a novel framework with large national dataset and ecologically meaningful clustering and clear management relevance .Here, five reproducible archetypes of seasonal eutrophication across German rivers were introduced. The results provided evidence that nutrient stoichiometry differs systematically among seasonal eutrophication regimes, linking the river-network structure (lake proximity and river length) to seasonal eutrophication dynamics.

My comments (revisions needed) are primarily interpretative rather than analytical:

- Reconsider strong claims of grazing control with more cautious language.
- Reconsider claims of N limitation with "consistent with" or "suggestive of" wording.
- Reconsider claims of riparian shading effects with more cautious inference.
- It needs to be clear that these mechanisms were inferred from patterns rather than directly measured.

RC2.00

We thank the reviewer for the positive and constructive comments. Please find below detailed responses on the individual aspects.

Below, more detailed comments are listed:

- Abstract:

Line 6 : Archetypal pattern or cluster? In Line 9, you mention cluster. Be consistent as this is interchangeably used throughout the manuscript and is confusing.

Also, could you briefly explain these 5 cluste4rs/patter in the abstract.

RC2.01

We agree that readability and consistency in the wording could be improved. We will carefully review the manuscript and homogenize the wording. We thank the reviewer for suggesting brief explanations of the clusters in the abstract. We will implement this in the revised version of the manuscript.

- Line 87-88: Reference ?

RC2.02

We thank the reviewer for drawing our attention to the missing reference, which we will add in the revised version of the manuscript.

- K-means assumptions deserve discussion

Can you justify use of K-means assumptions? Why not hierarchical clustering or dynamic time warping clustering? Seasonal trajectories often violate these assumptions.

RC2.03

We thank the reviewer for this comment. *k*-means was chosen because its use of Euclidean distance is consistent with our objective of identifying archetypal seasonal patterns that differ in both timing and magnitude. Note that we took particular care of the temporal alignment by the interpolation to reference days. This is important for our research question because differences in the timing of seasonal features, such as earlier versus later spring peaks or persistently high versus seasonally restricted $\alpha_{realized}$, are themselves ecologically meaningful and should contribute to the distinction between clusters. One of the main aspects of *k*-means clustering is the predefined number of clusters. To identify the optimal cluster number, we used the elbow plot, resulting in 5 clusters.

We recognize that Euclidean distance is calculated between the each seasonal cycle with the cluster centroid, which favors relatively compact clusters, although transitions between seasonal patterns in the underlying data may also be continuous.

We also considered the alternatives suggested by the reviewer. Hierarchical clustering would still require a distance metric and, if based on Euclidean distance, would retain the same definition of similarity as used here. Dynamic time warping, in contrast, is designed to align temporally shifted trajectories. Thus, this would be less appropriate for our objective because it reduces inference of peak timing central to our interpretation, for example the distinction between spring and summer peaks.

We will add these considerations to the Methods/Discussion to better justify the choice of *k*-means and acknowledge its limitations.

- Grazing is not measured and this issue appeared in the abstract and introduction.

The Methods confirm that no grazing measurements exist, no zooplankton data exist, no mussel abundance data exist. Therefore any later claim of "explained by grazing" is questionable. The Methods only support statements such as: "consistent with increased grazing pressure." This needs more clarification.

RC2.04

We agree that some formulations regarding grazing might be too confident and potentially inappropriate. We thank the reviewer for pointing this out. We will carefully review all parts of the manuscript where we argue that grazing is the main explanations for the observed patterns. When appropriate, we will change the current wording to more careful and scientifically robust formulations.

- There are n = 9 for the station-level Spearman correlations.

This is a very small sample size. With only nine monthly points, statistical power is low, confidence intervals are large, therefore significance testing becomes unreliable. I would recommend acknowledging this limitation more explicitly. Were correlations interpreted as exploratory indicators of potential controls."

RC2.05

We thank the reviewer for this comment. We will point out this limitation in more detail in the discussion section of the revised manuscript.

- "We used the thresholds TN:TP = 20 and 50 established in lakes."

The introduction already notes that rivers differ from lakes. So why should lake thresholds apply to rivers?

RC2.06

Marine, lake, and river algal communities differ remarkably little in their average molar biomass N:P ratios, with an average N:P of 16 in ocean phytoplankton (Redfield, 1958), 20 in lake phytoplankton (Sterner et al., 2008), and 17 in stream benthic algae (Hillebrand & Sommer, 1999). In other words, average algal biomass N:P ratios are highly comparable across aquatic ecosystems. Since no river TN:TP thresholds analogous to those established for lakes exist, this similarity makes it very likely that lake TN:TP thresholds are applicable to rivers.

References:

A. C. Redfield. The biological control of chemical factors in the environment. American scientist, 46 (3):230A–221, 1958.

R. W. Sterner, T. Andersen, J. J. Elser, D. O. Hessen, J. M. Hood, E. McCauley, and J. Urabe. Scale-dependent carbon: nitrogen: phosphorus seston stoichiometry in marine and freshwaters. Limnology and Oceanography, 53(3):1169–1180, 2008. doi: 10.4319/lo.2008.53.3.1169.

H. Hillebrand and U. Sommer. The nutrient stoichiometry of benthic microalgal growth: Redfield proportions are optimal. Limnology and Oceanography, 44(2):440–446, 1999. doi: 10.4319/lo.1999.44.2.0440.

- Cluster A interpretation needs caution

Cluster A is central to the paper. However, only three stations have catchment characteristics available later becomes important. Some conclusions involving land use and population density may therefore be weak for Cluster A. I would avoid drawing strong land-use conclusions from this cluster.

RC2.07

We agree that we cannot draw conclusions about clusters from land use data due to low data availability. However, we note that the discussion of cluster A is exclusively based on nutrient stoichiometry (available at 9/13 stations) and river network properties (L_{river}, d_{lake}, τ_{lakes}, available at 13/13 stations), not on land use and population density. To avoid misunderstandings, we propose to reformulate L300-301, where we note the low population density in the three cluster A catchments with available data.

- The Results often refer to N deficiency, P deficiency, co-deficiency based solely on TN:TP thresholds. However, TN:TP ratios indicate potential nutrient limitation, not actual limitation.

RC2.08

We agree that the current version of the manuscript may contain misleading formulations regarding the interpretation of nutrient stoichiometry. We will carefully check the manuscript for the terms "limitation" and "deficiency" and individually decide whether the wording is appropriate or not. If applicable, we will change the wording to "imbalance" or "potential deficiency". We also plan to critically review the management implications of the manuscript regarding nutrient stoichiometry (See Reviewer 1, RC1.08).

- The Results support strong spring production followed by summer losses but not "summer losses are caused by grazing."

This section contains valuable information but is difficult to read as there are many comparisons. Readers may lose track so consider a short synthesis sentence:

RC2.09

We thank the reviewer for this comment. We will review the manuscript in terms of general readability. Regarding the comment on grazing, please see RC2.04.

Cluster C: rαrealized, PAR = 0.85 . General observation: gives very clear signal and not very common in environmental field data. This is interesting!

RC2.10

We thank the reviewer for acknowledging our finding. For a more detailed response on $r_{\alpha\_realized,PAR}$, please see RC2.12.

Looks there is uneven sample sizes (stations in Cluster A, B , c). If one cluster is very small, some conclusions become less robust.

Catchment characteristics are available for only 58 stations so Land-use interpretations rely on less than half the dataset.

RC2.11

We agree that this is an important limitation of the study. We will highlight it more explicitly in the discussion.

- PAR , discharge, temperature are variables that are seasonally covarying. A high PAR correlation may partly reflect seasonality itself. Was it partial correlations or multivariate analyses that were considered:

RC2.12

We are aware of the potentially very strong co-correlations between PAR, T_{water}, and Q. Due to the limited coverage of Q, we avoided analyses addressing all three predictors at once. However, we checked the influence of co-correlations by computing partial correlation coefficients between $\alpha_{realized}$ and the three predictors with one of the other predictors being the covariate in each case (Figure 1).

The results of this analysis strongly support our interpretation that PAR is the main control of $\alpha_{realized}$ in cluster C. Whether T_{water} or Q is the covariate, the median partial correlation coefficient between alpha and PAR in cluster C is exceeding 0.8 (Figure 1a+c), whereas partial correlation coefficients are much smaller if PAR is the covariate (Figure 1b+d). We propose to add Figure 1 to the Supplementary Material of the manuscript.

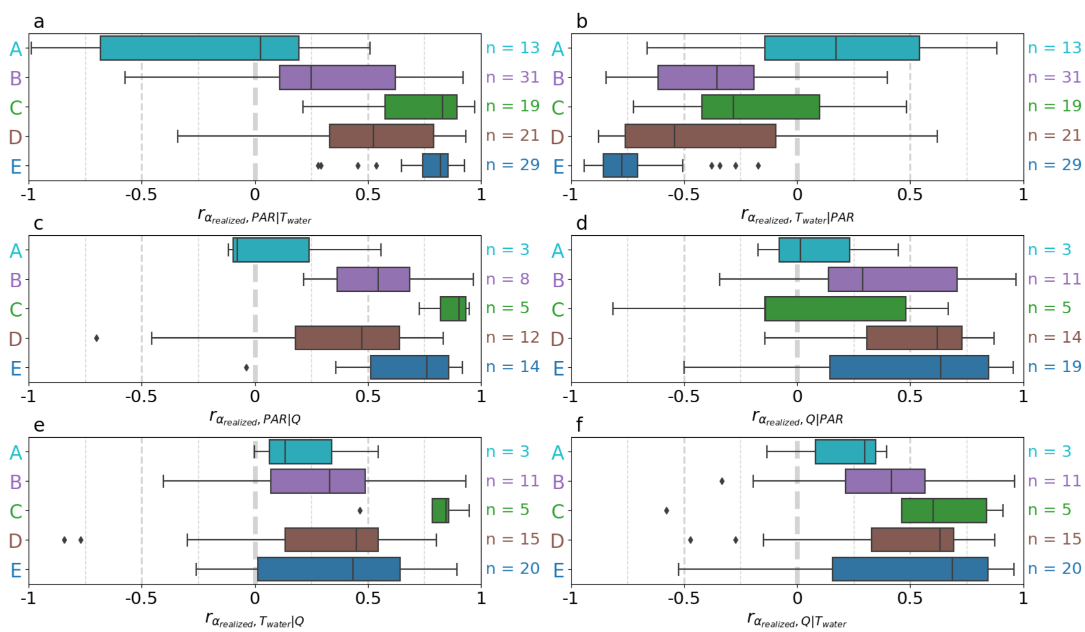

Figure 1: Cluster-wise distribution of the station-wise partial Spearman rank correlation coefficients at single stations between $\alpha_{realized}$ and (a) PAR with the covariate T_{water}, (b) T_{water} with the covariate PAR, (c) PAR with the covariate Q, (d) Q with the covariate PAR, (e) T_{water} with the covariate Q, and (f) Q and the covariate T_{water}.

- This claim of “summer declines were caused by grazing” result do not directly demonstrate it. No grazing data are presented; interpretations should consider this.

RC2.13

➔ See RC2.04.

- TN:TP ratios do not prove nutrient limitation.

RC2.14

➔ See RC2.08.

- Riparian shading was inferred but not measured!

RC2.15:

We agree that this is an important limitation. We will carefully review the manuscript for any potentially misleading formulation and explicitly note this limitation in the discussion.

- Only 58 stations had catchment data.

RC2.16:

➔ See RC2.11.

- The conclusion currently contains no acknowledgement of uncertainty. Future work better to consider ..???

RC2.17:

We agree that a Discussion subsection summarizing all limitations would substantially improve the manuscript. We therefore propose a new subsection addressing

- Limitations of the $\alpha_{realized}$ metric (see response to Reviewer 1, RC1.07)
- Lacking data of phytoplankton community composition (related to $\alpha_{realized}$, Reviewer 1)
- Lacking grazing data (RC2.04)
- Lacking data or measurements of shading (RC2.15)
- Limited and partly imbalanced sample sizes (discharge and catchment characteristics data, RC2.11; correlation analysis, RC2.05).

When appropriate, we will also suggest future works on the respective limitations.