

Response to Referee #1 Comments

I thank the referee for the careful reading of the manuscript and for the constructive and insightful comments. The suggestions have helped improve the clarity of the interpretation and the presentation of the results. All comments are addressed below in a point-by-point manner.

1. Interpretation of the common mode (PC1)

The most important conceptual issue concerns the interpretation of PC1. The manuscript states that removing PC1 reveals limited residual inter-basin structure, implying that synchronization is largely explained by a single large-scale/regional signal. However, PC1 is defined on annual aggregated reservoir anomalies, not on external forcing. This means that it represents the dominant covariance structure of the dataset, not necessarily a physically unique regional climate mode. The concern is that the manuscript risks overstating the physical interpretation of a purely statistical construct.

It is my recommendation that the authors clarify that PC1 represents the leading empirical mode of co-variability in reservoir storage and not a uniquely identifiable physical driver. Consider tempering conclusions about single dominant regional mode unless linked to independent climatic evidence.

I thank the referee for this important comment. I agree that PC1, as used here, is an empirical mode of co-variability derived from the reservoir-storage metrics, and not a uniquely identifiable physical driver.

Changes in manuscript:

In the Abstract, I replaced:

"the dominance of a common large-scale signal"

by:

"the dominance of a common empirical mode of co-variability"

In the Abstract, I replaced:

" that this change is largely captured by a single dominant regional common mode (PC1) in the annual reservoir metrics."

by:

" that this change is largely captured by a single dominant empirical mode of co-variability (PC1) in the annual reservoir metrics."

In the Abstract, I replaced:

"the leading regional mode of variability"

by:

"the leading mode of co-variability"

In the Introduction, I replaced:

"the dominance of a regional-scale common mode of variability that acts coherently across basins"

by:

"the dominance of a common empirical mode of co-variability across basins"

In the Introduction, I replaced:

"from the imprint of a common large-scale signal acting simultaneously on otherwise independent basins"

by:

"from the imprint of a dominant empirical mode of co-variability acting simultaneously on otherwise independent basins"

In the subsection "Common-mode removal and residual correlations", I added the following sentence after replacing:

"A dominant regional mode (PC1) was then estimated..."

by:

"A dominant empirical mode of co-variability (PC1) was then estimated..."

Added text:

"PC1 is interpreted here as an empirical mode of co-variability in the reservoir-storage metrics and not as direct evidence of a uniquely identifiable climatic or operational driver."

In the subsection "Summary of Observational Findings", I replaced:

"a single dominant regional common mode"

by:

"a single dominant empirical mode of co-variability"

In the Discussion, I replaced:

"regional-scale common signal"

by:

"dominant empirical mode of co-variability"

In the Conclusions, I replaced:

"single dominant regional mode of variability"

by:

"single dominant empirical mode of co-variability"

2. Causality vs statistical co-variability

The discussion carefully avoids casual claims, which is appropriate. However, some phrasing (e.g. line 159, explained primarily by a single regional-scale signal shared across reservoirs) implicitly suggests causality. I recommend that the authors distinguish more clearly between statistical common mode (observed covariance structure) and physical forcing mechanics (not directly analyzed here). The existence of a dominant PC1 is well demonstrated, but its physical interpretation remains unsolved, which the authors do indicate is not the goal of this manuscript. The PC1 should not be referred to as a "regional signal" without supporting climatic or operational evidence.

I thank the referee for this important clarification. I agree that some formulations could be interpreted as implying a physical causal mechanism, whereas the present study is limited to statistical co-variability in the reservoir-storage metrics.

Changes in manuscript:

In the Abstract, I replaced:

"dominated by a common large-scale signal"

by:

"dominated by a common empirical mode of co-variability"

In the subsection "Common-mode removal and residual correlations", I replaced:

"a single regional-scale signal shared across reservoirs"

by:

"a dominant empirical mode of co-variability shared across reservoirs"

In the subsection "Common-mode removal and residual correlations", I replaced:

"dominant regional signal"

by:

"dominant empirical mode of co-variability"

In the subsection "Common-mode removal and residual correlations", I replaced:

"a single dominant regional common mode"

by:

"a single dominant empirical mode of co-variability"

In the subsection "Common-mode removal (PC1) and residual inter-basin structure", I replaced:

"a shared regional signal"

by:

"a shared empirical mode of co-variability"

In the Conclusions, I replaced:

"identified common mode"

by:

"identified empirical mode of co-variability"

In the Conclusions, I replaced:

"shared large-scale signal"

by:

"shared dominant empirical mode of co-variability"

In the Discussion, I replaced:

"one dominant regional component"

by:

"one dominant empirical component"

In the Discussion, I replaced:

"strengthened regional mode"

by:

"strengthened empirical mode of co-variability"

In the final paragraph of the Discussion, I replaced:

"single dominant regional factor"

by:

"single dominant empirical mode of co-variability"

3. Choice of temporal breakpoint (1986)

The split into pre and post 1986 periods is described as exploratory. The results are strongly dependent on this segmentation, and no formal change point detection is provided. The breakpoint may appear somewhat ad hoc despite justification. The authors should either include a simple change point analysis or emphasize that results are robust to nearby split years (if tested).

I thank the referee for this important methodological comment. I agree that the selected breakpoint should not be interpreted as a formally determined change point, but rather as a representative split within a broader transition period.

Changes in manuscript:

In the Methods section, I replaced:

"The resulting conclusions are qualitative in nature and do not rely on the choice of a specific transition year."

by:

"The resulting conclusions are qualitative in nature and do not rely on the exact choice of a specific transition year."

In the Methods section, immediately after the previous sentence, I added:

"The qualitative increase in inter-basin synchronization is preserved for nearby split years within the mid-1980s window (not shown)."

4. Correlation as the primary metric.

Pearson correlation is appropriate for linear co-variability, but reservoir dynamics are often nonlinear and threshold driven, and correlation can be inflated by shared trends or persistence. Although PC1 removal addresses this, the manuscript would benefit from discussion of robustness to Spearman correlation or rank-based measures.

I thank the referee for this useful comment. Pearson correlation was chosen as a simple and interpretable measure of linear interannual co-variability in the annual reservoir metrics.

Changes in manuscript:

In the Methods section, I added the following sentence after:

"Pearson correlation was chosen because the annual metrics are continuous variables and the primary goal is to measure linear co-variability at interannual time scales."

Added text:

"Pearson correlation captures linear dependence and does not fully characterize possible nonlinear or rank-based relationships between reservoirs. The results are therefore interpreted specifically in terms of linear interannual synchronization."

In the Discussion, I added the following sentence after:

"The present work intentionally refrains from causal attribution."

Added text:

"Similarly, the correlation analysis is intended as a characterization of large-scale linear co-variability and does not exclude the presence of additional nonlinear dependence structures not resolved by the present approach."

5. Interpretation of increasing synchronization

The manuscript concludes that increasing synchronization is largely due to a dominant common mode. While the PC1 results support this, the following nuance is important. The "removal of PC1 reduces correlations", line 214, does not necessarily imply the absence of multi-basin coupling or the absence of higher-order shared structure.

I thank the referee for this important clarification. I agree that the reduction of inter-basin correlations after removal of PC1 does not imply the absence of additional higher-order dependence structure or more complex forms of coupling between basins.

Changes in manuscript:

In the subsection "Summary of Observational Findings", I replaced:

"additional inter-basin structure beyond this dominant mode appears limited"

by:

"additional inter-basin structure beyond this dominant mode appears weaker within the annual pairwise correlation framework used here"

In the subsection "Summary of Observational Findings", I replaced:

"the residual inter-basin correlations are substantially weaker"

by:

"the residual inter-basin correlations are substantially weaker within the present pairwise correlation framework"

In the Conclusions, I replaced:

"there is limited evidence for additional inter-basin structure beyond this dominant mode"

by:

"additional inter-basin structure beyond this dominant mode appears weaker within the annual pairwise correlation framework used here"

6. Hydrological interpretation of reservoir storage

The manuscript treats reservoir storage percentage as a proxy for hydrological state, which is valid, but this is strongly influenced by operational decisions and may not directly reflect natural hydrological coherence. I recommend adding a brief discussion of management driven synchronization (e.g., policy, allocation rules) vs climate-driven synchronization. This is especially relevant given the strong post 1980 signal.

I thank the referee for this important point. I agree that reservoir storage reflects not only hydroclimatic variability, but also operational decisions and management practices acting on the system.

Changes in manuscript:

In the Discussion section, I added the following paragraph after:

"From a water-management perspective, this result remains highly relevant."

Added text:

"Reservoir storage is not a purely natural hydrological variable. It integrates hydroclimatic inputs with operational decisions, allocation rules, and infrastructure constraints. The synchronization identified here therefore reflects the behavior of the managed reservoir system, rather than natural hydrological coherence alone. In this context, coordinated responses may arise from both shared external forcing and management practices acting across multiple basins."

7. Missing linkage to external forcing

The manuscript explicitly avoids external datasets, which is acceptable. However, the interpretation of a regional mode would be strengthened by at least referencing known climatic shifts that impact Catalonia (e.g., Mediterranean drought regimes). Even without analysis, a short contextual paragraph would improve interpretability.

I thank the referee for this helpful suggestion. I agree that a broader hydroclimatic context improves the interpretation of the results, even without explicit attribution analysis.

Changes in manuscript:

In the Discussion section, I added the following paragraph before:

"The present work intentionally refrains from causal attribution."

Added text:

"This interpretation can be placed in the broader context of Mediterranean hydroclimatic variability, where large-scale drought patterns, atmospheric circulation anomalies, and changes in evaporative demand have been widely documented [\(Lionello 2012, Vicente 2014\)](#). While the present analysis does not explicitly incorporate external climatic datasets, such processes provide a plausible background for the emergence of coherent inter-basin behavior and large-scale empirical co-variability at regional scale."

Additional corrections/suggestions

The Data section (beginning on line 60) needs elaboration. It would benefit from a location map of the reservoirs and river basins. What are the main reservoirs (line 62)? Did you limit based on capacity? Are the reservoirs similar in capacity or are some very small while others are very large? A table would be helpful which lists the 10 reservoirs, the river basin, capacity, and basic attributes mentioned throughout the text.

In line 86, did you confirm that these large fluctuations may not have been attributed to a large storm event?

Are annual metrics acceptable (section 4)? Are any of the reservoirs in alpine climate regions where snowmelt may contribute to reservoir storage, impacting the timing of averaging metrics? Provide justification for the calendar year.

Technical corrections

The citations are currently not in parentheses but part of the text.

Add labels to the components of each multi-panel figure (e.g., a, b, c).

I thank the referee for these useful suggestions, which helped improve the clarity of the data description and the methodological discussion.

Changes in manuscript:

In the Data section, I replaced:

"The study focuses on ten major reservoirs distributed across several river basins, including the Ter, Llobregat, Ebre, Muga, Foix, Gai\`{a}, and Riudecanyes basins."

by:

"The study focuses on ten reservoirs for which long-term daily storage-percentage records are available in the ACA compilations, providing coverage across several river basins in Catalonia."

In the Data section, I added a new summary table listing the analyzed reservoirs, basin affiliation, and temporal coverage of the available records.

In the Preprocessing section, I added the following paragraph after:

"This criterion allows for rapid but realistic variations during intense hydrological or operational events, while excluding unrealistically abrupt jumps incompatible with reservoir storage dynamics."

Added text:

"Because no independent inflow or precipitation records are used, individual flagged variations are not attributed to specific causes, and extreme hydrological or operational events cannot be formally excluded. However, the very small fraction of corrected observations limits the influence of this procedure on the derived annual metrics."

At the end of the section "Derived Annual Metrics", I added:

"Calendar-year aggregation was chosen to provide a uniform interannual unit across all reservoirs and to facilitate comparison at multi-decadal scales. This approach is appropriate for the present objective, which focuses on long-term co-variability rather than seasonal dynamics. Possible seasonal effects, including snowmelt contributions in headwater basins, are therefore not resolved explicitly and are instead integrated into the annual metrics."

Citation formatting was corrected throughout the manuscript to ensure consistency with the journal style. In addition, labels (a, b, c, ..) were added to the multi-panel figures to improve clarity and readability.