

In this manuscript, the authors introduce a spatial interpolation framework termed *Spatial Pattern Regression* (SPR), applied over North America. The method consists of extracting spatial patterns from Regional Climate Model (RCM) simulations using Principal Component Analysis (PCA), and subsequently using these patterns within a spatial regression framework to derive gridded meteorological fields from station observations.

The manuscript addresses an important topic for the hydrological and climatological communities. The increasing availability of RCM simulations indeed provides promising opportunities to improve the spatial interpolation of meteorological observations, especially in data-sparse regions. Overall, the manuscript is clearly written and the validation is well conducted. However, several methodological clarifications and additional analyses are required before publication can be considered.

Major comments

1) Fair comparison between SPR and benchmark interpolation methods

The tested approaches are not evaluated under equivalent settings. Specifically, IDW, OK, and KED are directly applied to station observations Z_d , whereas SPR is applied to anomalies relative to the RCM background field:

$$Z'_d = Z_d - Z_{d,\text{grid}} \quad Z'_d = Z_d - Z_{d,\text{grid}}$$

(or potentially transformed ratios).

This makes the comparison difficult to interpret. It is possible that IDW, OK, or KED applied to the same anomaly fields could perform similarly or even outperform SPR. I therefore strongly recommend evaluating all interpolation methods within the same anomaly-based framework.

In addition, the manuscript should investigate the sensitivity of SPR to the definition of anomalies:

- additive anomalies:

$$Z'_d = Z_d - Z_{d,\text{grid}} \quad Z'_d = Z_d - Z_{d,\text{grid}}$$

- multiplicative anomalies:

$$Z'_d = \frac{Z_d}{Z_{d,\text{grid}}} \quad Z'_d = \frac{Z_d}{Z_{d,\text{grid}}}$$

The additive formulation is generally more appropriate for temperature, while multiplicative or relative anomalies are often preferable for precipitation due to its heteroscedastic nature.

At present, the conclusions regarding the superiority of SPR remain insufficiently supported.

2) Robustness to station sampling and validation strategy

For both the synthetic and real-data experiments, the results may be highly sensitive to the specific sampling of training stations, particularly under low station-density scenarios.

I recommend using a K-fold cross-validation framework (or repeated random subsampling) in which several independent training/validation splits are generated. Performance metrics should then be averaged across folds:

$$\overline{M} = \frac{1}{K} \sum_{k=1}^K M_k$$

This would provide more robust and statistically reliable conclusions.

3) Ground truth used for SSIM and spatial field evaluation

The manuscript should clarify what is used as the “ground truth” for the SSIM calculations and spatial field comparisons.

If the reference field originates from the RCM itself, this may introduce an important methodological bias. It is well known that RCMs — especially at resolutions around 10–15 km — tend to smooth spatial variability, particularly for precipitation extremes and localized convective structures.

Consequently, methods reproducing smoother spatial fields (such as SPR) may be artificially favored over approaches preserving higher local variability (e.g., KED).

The authors should discuss this limitation explicitly and evaluate how sensitive their conclusions are to the choice of reference dataset.

4) Discussion on RCM spatial smoothing and convection-permitting models

The discussion section should explicitly acknowledge that conventional RCMs at approximately 11 km resolution strongly smooth the spatial variability of meteorological fields, especially precipitation.

The use of convection-permitting regional climate models (CP-RCMs), typically at 1–4 km resolution, represents a promising avenue for improving the realism of spatial structures used within SPR.

This limitation is particularly important because SPR directly exploits RCM-derived spatial patterns; therefore, any structural bias in the RCM may propagate into the interpolated fields.

Minor comments

5) Lines 16–17

The manuscript separates “spatial heterogeneity” and “extremes”, although these concepts are closely related. In this context, “variability” appears more appropriate than “heterogeneity”.

I suggest replacing:

“spatial heterogeneity and extremes at fine spatial and temporal resolution”

with:

“the fine spatio-temporal variability of meteorological fields, especially during extreme events”.

6) Lines 22–24

Meteorological radar products provide gridded precipitation estimates but do not fully belong to the three categories listed by the authors. Radar datasets can be used directly, without relying on station observations or reanalysis frameworks.

This category should therefore be acknowledged separately.

7) Lines 23–24 — Missing references

Several widely used products and modeling systems should be cited when discussing interpolation, reanalysis, and climate-model-based datasets.

Examples include:

Spatial interpolation products

- E-OBS

Reanalysis products

- CERRA-Land

Regional climate models / NWP systems

- WRF
- AROME
- COSMO

8) Lines 36–37 — Climatological products from station observations

Climatological fields are also frequently derived directly from station observations. The manuscript should acknowledge datasets/methods such as:

- PRISM
- SPAZM
- AGPD

Relevant reference:

- Hofstra et al. (2008), *New daily precipitation and temperature datasets for Europe*, International Journal of Climatology, DOI: 10.1002/joc.8662.

9) Lines 38–39 — Temporal scale of extracted spatial structures

If I understand correctly, the spatial structures extracted from the RCM are climatological patterns rather than daily evolving fields. This point should be clarified explicitly.

Regarding Equation (1), does:

$Z_{n,p}^{\mathrm{grid}} Z_{n,p,\mathrm{grid}}$

represent all daily RCM fields? If so, the indexing notation should probably be revised (e.g., $1:n1:n1:n$) to better reflect the temporal dimension.

The manuscript would benefit from a clearer distinction between:

- climatological spatial structures,
- daily anomalies,
- temporally evolving PCA modes.

10) Lines 40–41 — Classification of SPR

SPR can only be classified as a hybrid between spatial interpolation and reanalysis if the RCM predictors vary dynamically over time (e.g., daily or sub-daily evolving structures).

If SPR only relies on climatological information extracted from the RCM, then the method is conceptually closer to a spatial interpolation framework constrained by climatological covariates.

Please clarify this distinction.

11) Figure 1

Please add the station locations to Figure 1(b). This would greatly improve the interpretation of the spatial interpolation setup and station density.

12) Lines 74–75 — “Does not require overlap”

The statement that the method “does not require overlap” is true for precipitation but more questionable for temperature because of long-term warming trends.

For temperature, inconsistencies between climatological periods may introduce systematic biases.

This issue should be discussed, together with potential solutions such as:

- detrending,
 - anomaly standardization,
 - quantile mapping,
 - temporal homogenization.
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13) Lines 105–110 — Covariance structure vs spatial patterns

The manuscript states that SPR exploits the covariance structure. However, the method actually exploits much richer information: spatial patterns extracted from the RCM.

Using covariance structure alone would correspond to statements such as:

observations separated by 20 km are correlated, whereas correlation becomes negligible at 100 km.

SPR instead leverages organized spatial modes derived from the RCM. This is potentially more informative, but also riskier because any regional structural bias in the RCM may directly influence the interpolation.

This limitation should be explicitly discussed.

14) Lines 122–123 — Distributional assumptions

The manuscript states that SPR does not make distributional assumptions. This is not entirely correct.

The regression residuals implicitly assume:

$$\varepsilon \sim N(0, \sigma^2) \quad \text{where } \varepsilon \sim \mathcal{N}(0, \sigma^2)$$

with residuals being independent and identically distributed (IID).

Therefore, SPR still relies on classical regression assumptions, even if the predictors themselves are derived empirically.

15) Line 209 — Sensitivity to transformation choice

The sensitivity of the transformation step should be evaluated.

In precipitation interpolation, square-root transformations are frequently used at the daily timescale to stabilize variance and reduce skewness. It would therefore be useful to compare the selected transformation against the classical square-root transformation approach (e.g., Erdin et al.).

16) Line 237 — Use of KGE

I believe the Kling–Gupta Efficiency (KGE) would be more informative than RMSE alone.

KGE explicitly evaluates:

- correlation,
- bias,
- variability ratio,

through:

$$\mathrm{KGE} = 1 - \sqrt{(r-1)^2 + (\beta-1)^2 + (\gamma-1)^2}$$

$\mathrm{KGE} = 1 - \sqrt{(r-1)^2 + (\beta-1)^2 + (\gamma-1)^2}$

where:

- r is the correlation,
- β the mean bias ratio,
- γ the variability ratio.

In the present context, the variability component is particularly relevant because it directly evaluates biases in spatial variability.

17) Table 3

I recommend replacing Table 3 with boxplots (or violin plots). This would make the distribution of RMSE values much easier to interpret visually and would better highlight:

- dispersion,
- outliers,
- robustness across experiments.

