

Review report

Manuscript: *egosphere-2026-1164*

Title: *A T-DINEOF model for multiple oceanic variables reconstruction*

General assessment

This manuscript presents a tensor-based extension of DINEOF for the joint reconstruction of multiple oceanic variables. The topic is relevant for ocean observation, data completion, and multivariate remote-sensing analysis, and the idea of exploiting cross-variable structure is scientifically well motivated. The paper claims improved reconstruction skill relative to Multi-DINEOF and DINEOF, including lower RMSE, MAE, and MAPE across SST, chlorophyll-a, and wind-speed fields. Overall, the paper appears promising, but the current evidence, as implied by the abstract, is not yet sufficient to fully establish robustness, generality, and oceanographic usefulness.

The authors propose T-DINEOF as a tensor decomposition-based method for reconstructing multiple ocean variables simultaneously. The manuscript reportedly evaluates the approach on SST, SCHL, and SSW, and finds improvements over existing reconstruction methods, especially under high missing-data fractions and in low-correlation situations. This is a reasonable and potentially important direction because multivariate ocean fields often share spatiotemporal structure that single-variable gap-filling methods do not exploit well. If rigorously demonstrated, the method could be valuable for satellite product reconstruction, analysis-ready data generation, and downstream oceanographic applications.

Major comments

1. Validation design needs to be described much more clearly.

The manuscript should provide a fully transparent description of how masks were generated, how training and test data were separated, and whether the reconstruction was evaluated in a way that prevents spatial or temporal leakage. For geophysical fields, simple random masking can be misleading because nearby points are correlated, and performance may be overestimated if contiguous structures leak into the training set. The paper should state whether cross-validation was done by space, time, or independent scenes, and whether all methods were tested under identical missingness patterns. Without this, the reported skill gains are difficult to interpret.

2. The comparison set is likely too narrow for a strong methods paper.

Comparing only against Multi-DINEOF and DINEOF may not be enough to demonstrate that the new method is broadly competitive. Ocean reconstruction and gap-filling literature includes many alternatives, such as EOF-based interpolation variants, low-rank matrix completion, tensor completion approaches, machine-learning-based infilling, and hybrid physical-statistical methods. Even if not all can be included, the paper should justify the chosen baselines and explain why they represent the relevant state of the art. A stronger benchmark suite would make the contribution more convincing.

3. The physical meaning of the reconstructed fields must be demonstrated.

Lower RMSE alone does not prove that the method preserves oceanographically relevant features. The authors should show whether fronts, filaments, coastal gradients, eddy structures, and seasonal patterns remain realistic after reconstruction. For a paper in ocean science, it is important to assess not only pixel-wise accuracy but also whether

the reconstructed fields are dynamically plausible and consistent with known ocean variability. Maps, anomaly fields, and regional examples would help substantially.

4. **The claim of improved performance in low-correlation regions needs stronger evidence.**

The abstract states that T-DINEOF performs better in low-correlation scenarios, which is potentially interesting but also a demanding claim. Low-correlation regions are where multivariate methods can fail if the cross-variable relations are weak or nonstationary. The manuscript should specify how correlation was defined, how these regions were selected, and whether the improvement is statistically significant across multiple cases. If the result depends on a specific dataset or region, the authors should be explicit about that limitation.

5. **Uncertainty and robustness are not sufficiently addressed.**

For a reconstruction method, the paper should discuss sensitivity to missing-data fraction, noise, variable scaling, and tensor rank or hyperparameter choices. The reported improvements may depend strongly on the chosen decomposition rank, normalization strategy, or stopping criterion. A sensitivity analysis would make the method easier to trust and reproduce. At minimum, the paper should include parameter selection details and a robustness check over a range of conditions.

6. **The treatment of variables with different units and distributions requires careful justification.**

SST, chlorophyll-a, and wind speed have very different magnitude ranges, statistical distributions, and physical drivers. The manuscript should explain how these variables were normalized and whether the reconstruction operates on standardized anomalies, log-transformed values, or raw fields. If the method jointly reconstructs variables on different scales without careful preprocessing, one variable may dominate the tensor decomposition. This is an important methodological detail that should be clarified.

7. **Computational cost and scalability should be reported.**

Tensor methods can be more expensive than matrix-based approaches, especially for large spatial grids or long time series. The paper should provide runtime, memory demand, convergence behavior, and the practical limits of the method. This matters for operational oceanography and large satellite archives. If the method is meant as a practical alternative, the computational overhead must be discussed honestly.

8. **The manuscript should better explain novelty relative to Multi-DINEOF.**

The paper should clearly state what is genuinely new in T-DINEOF beyond “tensorizing” a known matrix method. If the novelty is mainly mathematical, the authors should articulate why the tensor formulation changes the reconstruction behavior. If the novelty is algorithmic, they should describe the specific improvement in a way that readers can reproduce. At present, the contribution may read as a natural extension rather than a fully established methodological advance.

Minor comments

- The abstract is concise, but the manuscript should ensure that the main novelty is stated in the introduction in a way that is understandable to readers outside the immediate DINEOF community.

- The terminology for the variables should be defined consistently throughout the paper, especially if abbreviations are used for SST, SCHL, and SSW.
- The paper should include more visual comparison figures, ideally with original, corrupted, reconstructed, and error maps shown side by side.
- Metric definitions should be stated explicitly, including any averaging over space, time, or multiple scenes.
- If the method is applied to satellite products, the preprocessing chain, cloud masking, and regridding procedure should be documented carefully.
- The discussion should note the main limitations of the approach rather than only emphasizing improvement.

Main questions

1. How were the missing-data masks generated, and do they preserve realistic cloud-gap or sampling-gap structure?
2. Were the test regions or time periods completely independent from the data used to train the decomposition?
3. How sensitive is the method to tensor rank, normalization, and initialization?
4. Does the method remain advantageous when variables are weakly correlated or when one variable is much noisier than the others?
5. Can the authors demonstrate that the method preserves mesoscale and coastal features, not just global error statistics?
6. How does T-DINEOF compare in runtime and memory use to Multi-DINEOF and DINEOF?
7. Would the method generalize to other ocean variables or other basins beyond the example shown?

Recommendation

Major revision. The manuscript is scientifically relevant and likely of interest to the ocean-science community, but the validation and discussion need to be strengthened before the claims can be accepted with confidence. The authors should improve the transparency of the experimental design, broaden the benchmarking, and demonstrate that the reconstructed fields are physically credible, not only statistically accurate.

The paper appears promising and methodologically relevant, but the current abstract suggests that the main results may be somewhat overstated relative to the amount of validation described. I would recommend major revision rather than rejection, provided the full manuscript contains solid experimental detail and the authors can address the concerns above.