

Review of “The advantages of data assimilation in parametric space rather than classic grid space”

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Overall assessment

The revised manuscript has improved relative to the previous version. The authors have clarified that the study concerns a linear-Gaussian analysis step rather than a full nonlinear or cycling data assimilation system, and they now place the work more appropriately in the context of non-Gaussian, displacement-aware, and feature-informed data assimilation methods. Most importantly, the authors added a fourth experiment in which the methods start from noisy gridded fields and parameters are subsequently fitted. This is the right direction and directly addresses the main concern raised in my previous review.

However, I do not yet find the revision sufficient for publication. The first three experiments are useful as tutorial experiments, and I do not object to them being idealized or favorable to the parametric representation, provided this is made explicit. They illustrate the expected coordinate effect: Gaussian updates behave better in coordinates aligned with the dominant uncertainty. The fourth experiment is therefore central, because it is the only experiment that can test whether the parametric approach remains advantageous after accounting for the inverse step from gridded fields to parameters. In the current manuscript, this experiment is still too under-documented and too lightly integrated into the methodology and results.

Recommendation

Major revisions.

The work could become publishable as a useful conceptual and didactic contribution, but the revised manuscript needs a clearer and more reproducible presentation of Experiment #4, quantitative diagnostics in addition to the figures, cleaner notation, and more moderate claims.

Brief summary of the manuscript

The manuscript considers a one-dimensional Gaussian eddy profile described by amplitude, position, and radius parameters. It compares data assimilation performed in the three-dimensional parameter space with data assimilation performed in the corresponding gridded representation of the eddy. The main result is that gridded linear-Gaussian updates can smear or distort the eddy when position or radius uncertainty is present, whereas parameter-space updates preserve the prescribed coherent structure.

This result is plausible and useful to illustrate. It is also largely expected, since averaging displaced or differently scaled structures in grid space produces smearing, while averaging directly

in position/radius/amplitude coordinates preserves the parametric shape. The manuscript should therefore be framed more as a controlled demonstration of a representation effect than as a broadly validated new data assimilation scheme.

Major comments

1. The first three experiments are acceptable as tutorial cases, but their role should be stated more clearly

In the first three experiments, uncertainty is generated in parameter space and then mapped to grid space. This means that the parameter-space method receives the uncertainty in the coordinates in which it is simple and approximately Gaussian, while the grid-space method receives the nonlinear push-forward of that uncertainty. For position and radius perturbations, this setup naturally produces the smearing seen in grid space.

I do not view this as a fatal flaw. These experiments are useful didactic examples. However, the manuscript should state explicitly that they are tutorial experiments designed to illustrate the mechanism, not balanced end-to-end comparisons of practical DA workflows. The broader claims should not rely on these experiments alone.

2. Experiment #4 is the key addition, but it is not yet reproducible

The new Experiment #4 is the most important part of the revision. It attempts to start from noisy gridded fields, fit parameters, estimate parameter-space covariances, and then perform the parametric analysis. This is the correct response to the main asymmetry in the original manuscript.

At present, however, the experiment is not described in enough detail to reproduce or assess it. The manuscript should specify the fitting objective, the optimizer or fitting method, the initialization, whether constraints such as positive amplitude and positive radius are imposed, how many noise realizations are used, and how the parameter covariance matrices are estimated from the fitting procedure. The manuscript states that non-negligible correlations between fitted parameters arise, but the actual covariance or correlation matrices are not shown. These should be reported, at least in a small table or appendix.

As written, Experiment #4 reads like a useful but preliminary add-on. It needs to be integrated into the methodology and results as the main test of whether the parametric approach remains useful once the inverse mapping from grid space to parameter space is included.

3. Quantitative diagnostics are needed in addition to the figures

The figures are helpful and should remain. They communicate the qualitative mechanism clearly. However, the conclusions cannot rest mainly on visual comparison of curves.

The manuscript should report quantitative diagnostics for each experiment. At minimum, this should include analyzed or fitted amplitude, position, and radius values; errors relative to a prescribed truth or reference state; profile errors such as RMSE or relative L^2 error; and uncertainty estimates. If EKE is retained as a physical diagnostic, the formula and computation should be given explicitly, together with quantitative EKE errors.

For Experiment #4, these diagnostics should preferably be reported over several noise realizations. A single plotted realization is not enough to establish robustness.

4. The notation remains confusing

The notation is still a major obstacle to understanding the method. The same symbols are used for parameter-space states, gridded states, observations, ensembles, and generic DA variables. In particular, x , y , X , Y , B , and R change meaning depending on the space under discussion. This makes it difficult to verify what is being assimilated and which covariance matrix belongs to which space.

I strongly recommend introducing separate notation for parameter states and gridded fields. For example, use $\theta = (a, p, r)$ for the parameter vector and $u = h(\theta)$ for the gridded profile. Then define separate background, observation, and analysis variables in each space, such as $\theta^b, \theta^a, z_\theta$ and u^b, u^a, z_u . The cost functions and Kalman gains should also be written separately for parameter space and grid space. This would make the comparison much easier to audit.

The EnOI update should also be clarified. It is currently unclear whether the update is written for ensembles, ensemble means, or random variables. If the equation is for ensemble means, barred variables should be used. If it is for ensemble members, that should be stated explicitly.

5. Several claims should be narrowed or better supported

Some claims remain too broad for the evidence presented. The abstract should not state that classic gridded DA generally fails for coherent structures. The appropriate claim is narrower: linear-Gaussian gridded updates without displacement correction can smear or distort coherent structures when displacement or shape uncertainty dominates.

The computational-cost claim should also remain conditional. In the synthetic setting, the parameter space is much smaller than the grid space, but practical applications require detection, fitting, covariance estimation, and possibly multi-feature association. Without timing or complexity evidence, the manuscript should only claim potential cost reduction under favorable conditions.

The statement that the parametric approach does not require a large ensemble, while grid-space DA requires at least 25 members, is not supported by the experiments shown in the manuscript. The described experiments use a much larger ensemble, and no ensemble-size sensitivity analysis is presented. This claim should either be supported or removed.

Finally, the statement that a machine-learning model is required for parameter evolution is too strong. Machine learning may be one possible approach, but stochastic parameter models, reduced-order models, tracking-based models, or hybrid models could also be used.

Minor comments

The manuscript still needs careful editing. Examples include “without the the forecast step”, “we don’t focus not”, “Copyright statement. TEXT”, “The Figure”, “The Table”, “correspond to”, “gives same values”, and “Gaussian profiled fitting”. Some DOI strings also appear malformed, for example with duplicated <https://doi.org/>. The language should be revised throughout for precision and fluency.

The parameter constraints should also be clarified. In particular, if radius uncertainty is modeled with an unconstrained Gaussian distribution, negative or near-zero radii may occur. This is physically problematic even if the formula depends on r^2 . The authors should state whether parameters are constrained, truncated, transformed, or sampled without constraints.

Conclusion

The authors have made a good-faith and relevant revision. The added fourth experiment is the right response to the main methodological concern, and the first three experiments remain useful as tutorial illustrations. However, the fourth experiment is not yet sufficiently documented or quantitatively supported, and the notation and presentation still make the method difficult to verify.

I therefore recommend major revisions. The manuscript could become a useful contribution if Experiment #4 is made reproducible and central to the argument, quantitative metrics are added, the notation is cleaned up, and the claims are kept appropriately limited.