Response to reviewer #1

Feb.17, 2025

Response to the general comments

First, we would like to thank the reviewer for his time spent on this paper and the editor's continuous effort on searching for reviewers. As we can see from the MS records in the NPG submission system, the editor has reached out more than 20 potential reviewers and most of them refused to review this paper. Aside from our great gratitude to the editor and the reviewer, we believe the reason why this happened might be close to what's mentioned in the first paragraph of the referee report, that this paper has used mathematics more advanced than the level that the majority of the DA community in geophysics are familiar with. While we agree that difficult techniques may cause reader's anxiety, we do not think this should be a direct reason that this paper in its current form should not be considered by any DA-focused journal in geophysics, especially when the technique is useful.

Since this is a scientific paper and NPG is a scientific journal, whether this paper should be considered by NPG and the DA-community, could be determined by a simple fact: has this paper made an explicit and innovative point that could be helpful to the data assimilation community in geophysics? Next we express our own opinion on whether this paper has explicit and innovative points and whether it would be helpful.

1) Does this paper make an explicit and innovative point?

The main point of this paper is that when you use a diffeomorphism (a mathematical displacement map) T to displace/correct a physical field S, the corrected/displaced field should NOT simply be $S \circ T$ (or $S \circ T^{-1}$). For instance, suppose T maps the point $p = (1, 3)$ to the point $q = (2, 5)$, in which $(1, 3)$ and $(2, 5)$ are coordinates of p and q respectively on a plane. Also suppose that the original field S is a density field on the plane that has value $S(p) = 3\text{g}/\text{m}^3$. Then the corrected/displaced field S^{new} does NOT necessarily have the value $S(q) = 3\text{g}/\text{m}^3$. We pointed out this explicitly and have very detailed discussion of this point in section 3.1 using the 2007 paper of Professor Sai Ravela et.al as a comparison. In our opinion, this is a very important gap that has been missing in all past papers in the DA-community since 2007, including the two papers mentioned by the reviewer [1,2]. We did not cite and discuss all the past papers because the same conclusion can be easily reached if the reader truly understand what we want to convey in section 3.1 of the current manuscript.

Let's look at [1] for example. In equation (2) of [1], it is clearly written that the residue error term is:

$$J_r = \sum \frac{[y(i,j) - x(i+a, j+b)]^2}{\sigma_o^2}, \tag{1}$$

in which y, x are observed field and background estimate respectively. The method proposed in [1] aims to minimize $J_r +$ (other penalty terms). Clearly, aside from the other constraints, the authors of [1] would like the corrected background estimate $x(i+a, j+b)$ to be as close as possible to the observed field $y(i,j)$. Here $x(i+a, j+b)$ is exactly the aforementioned $(S \circ T)(i,j)$ for $T(i,j) = (i+a, j+b)$. Thus, the J_r term in equation (2) of [1] can be equivalently rewritten as

$$J_r = \sum_{i,j} \frac{[y(i,j) - (x \circ T)(i,j)]^2}{\sigma_o^2} \tag{2}$$

or more compactly,

$$J_r = \|y - x \circ T\|_{\sigma_o}^2 \tag{3}$$

Eq.(3) can be directly compared with the first penalty term in Eq.(1) of our manuscript: $\|S_1 - T^{\#} S_2\|^2$. **In fact, $S \circ T$ is used in almost all (with one exception as we know which builds their method based on optimal transport theory [3]) research papers in geophysics which studies the displacement of physical fields.** In case the reviewer is interested, we leave the discussion of [2] to you as an exercise. Therefore our main point is that, we think it is a mistake to use $S \circ T$ to represent the corrected/displaced field for all types of physical fields S. We propose to use $T^{\#} S$ to replace $S \circ T$ in this manuscript. The explicit definition of $T^{\#}$ is based on the concept of differential forms, which might be unfamiliar to the majority of the DA-community. Thus technically, we can not find a way to introduce our method without mentioning any mathematics beyond Calculus III as required by the reviewer. And we do not think this should be a barrier of publishing this paper. If this paper has nice and important points and some mathematics is necessary to understand this paper, why not learn the mathematics? Indeed, differential form is practically needed for writing a code for the algorithm in this manuscript, and for improving other methods on the same subject presented by other researchers.

The reviewer also mentioned another option that is to submit this paper to an applied math or computer science journal. In fact, we have tried so. But the major value of this paper is not in its mathematical complication, but in that it provides new insights to the real DA-community in geophysics, instead to the applied math community. And we were told by an editor in some CS journal, only a very small group of people in CS cares about displacement error in DA. Thus, after having tried several applied math/CS journals, we have accepted the fact this paper may not be proper for an applied math/CS journal.

In my impression, NPG is an open-minded interdiscinplinary journal which welcomes all papers that bring new ideas to the geophysics community. I have seen some paper in NPG that discusses about the connection between Chern class and geophysics. Chern class is a much more advanced mathematical concept than differential forms. Thus we think maybe submitting this paper to NPG would be the best option.

2) Would the main point of this paper be helpful to the practical DA-community of geophysics?

The advantage of $T^{\#}S$, in contrast to $S \circ T$, can be briefly summarized as the following two points:

- $T^{\#}S$ would conserve certain physical quantities for any smooth and invertible map T;
- The correctly defined $T^{\#}S$ would not introduce conflict when two different fields are corrected/displaced using the same T.

To elaborate on the physical conservation property, suppose that the domain $\mathcal{D}$ is 3-dimensional and S is a density field on $\mathcal{D}$, and

$$m = \int_{\mathcal{D}} S dxdydz \tag{4}$$

is the total mass. Let $T : \mathcal{D} \to \mathcal{D}$ be any smooth and invertible displacement map on the domain. T would induce the corrected/displaced field $S^{\text{new}} = T^{\#}S$. Correspondingly we have the new mass

$$m^{\text{new}} = \int_{\mathcal{D}} S^{\text{new}} dxdydz. \tag{5}$$

Then we always have that $m = m^{\text{new}}$. The definition of $T^{\#}$ only relies on T and the type of S (i.e. a density field). It does not rely on the value of S. However, $S \circ T$ would not enjoy this property. We believe it would not be hard for the reviewer to find such an example himself/herself.

What's more important is the second advantage. Still we elaborate it using a specific example. Consider an OSSE using some reanalysis data as the "truth" and we are interested in the surface 2 dimensional velocity field $vel = (u, v)$ and the corresponding vorticity field $\omega = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y}$. To simplify the notation we denote by $Y_1 = vel^{\text{true}}$, $Y_2 = \omega^{\text{true}}$. And $X_1 = vel$ and $X_2 = \omega$ are the background estimates. For any displacement map T, it could induce a displaced velocity field $X_1^{\text{new}} = T^{\#}X_1$. X_1^{new} would further induce a new vorticity field, denoted by $vor(X_1^{\text{new}})$ to be distinguished from $X_2^{\text{new}} = T^{\#}X_2$. A natural question immediately arises, is X_2^{new} equal to $vor(X_1^{\text{new}})$? The answer is yes if $T^{\#}$ is correctly defined. However, the traditional $S \circ T$ does not have this property. To make it crystal clear, if $X_1^{\text{new}} = X_1 \circ T$, it would be unknown whether $vor(X_1^{\text{new}}) = X_2 \circ T$. We think this is a very important point for data assimilation practitioners, including ourselves, when they/we want to directly estimate/adjust the displacement of a vortex. In our numerical experiment, it is assumed that the full vorticity field ω and the full water height field h are observed, while the buoyancy field Θ and the velocity field (u, v) are fully unobserved. Imagine that you have used some method and the data h^o, h, ω^o and ω to estimate a displacement map T. The next question is that, given T and Θ and (u, v), how would you estimate the corresponding adjusted (u, v)? Would you use $(u \circ T, v \circ T)$ for the new velocity field? Given that $(u \circ T, v \circ T)$ is close to the truth $(u^{\text{true}}, v^{\text{true}})$, how do you know the vorticity field calculated from $(u \circ T, v \circ T)$ is close to the true vorticity field? Logically, the vorticity field calculated from $(u \circ T, v \circ T)$ can be far away from the true vorticity field. This is shown in Fig.5 of our manuscript. Of course you can make this up by adding more penalty terms to the cost function. But things can be very complicated and it is likely that you would not get a meaningful displacement map T if you add too many penalty terms. In sum, the correct definition of $T^{\#}$, in our opinion, is a clean and simple solution to avoid such conflicts between different types of physical fields. And it is a fundamental element in displacement estimation for DA practitioners.

With all the above, I am trying to explain, based on our scientific interpretation instead of on our standpoint, that this paper perfectly match the scope of NPG and the need of DA researchers and practitioners. We introduce new mathematical techniques to the DA community because those techniques are directly helpful to the final algorithm. We also believe that many of the researchers/practitioners in DA community think differently from the reviewer. For instance Fuqing Zhang, the first author's Ph.D advisor, had always been open-minded to applying new mathematical techniques in DA. However, we will respect the decision of the reviewer and the editor.

Response to the specific comments

1, *"The idea of working from the manifold of physically-allowable fields is interesting. "*

Reply: We thank the reviewer for his/her interest. "Working from the manifold of physically-allowable fields" is not the perspective of this manuscript. The reviewer is talking about a perspective of Dr. Vladimir Arnold. We are not experts on this subject. The method of our paper aims to find a displacement map T which transforms the background estimate S_2 to the observed truth S_1. Here S_1 and S_2 are not generated from the perfect law of physics, but could be disturbed by human activity, some random events, or even the model error due to coding. Hence we are not assuming that S_1 and S_2 lie on the manifold of the physically-allowable fields. Also, the displacement flow we estimate do not have to be divergence free. According to our knowledge, there is no perfect theory which defines the displacement error between two arbitrary fields of differential forms. What we propose in this manuscript is an empirical method. The solution is shown to exist uniquely when the fields are smooth. It can be used to identify the displacement error empirically. It can be applied to more general situations. But it is not a mathematical theory. However, we understand that there might be some connection between Arnold's perspective and what we propose in this manuscript. We will keep this in mind.

2, *"Please consider more recent advances in the field alignment literature. In particular, I recommend reviewing the following works and related references. Please compare and contrast your method with methods like these (e.g., the physics-based penalty terms used to prevent grossly unphysical alignment fields)"*

Reply: We have explained this in our response to the general comments. Basically, all the past papers have the same mistake. The field alignment paper of Professor Sai Ravela et.al in 2007 is one of the earliest paper that studies this subject. All the following papers, including [1,2], made the same assumption that $T^{\#}S = S \circ T$. This assumption is not explicitly proposed in the past papers but this conclusion can be easily deduced from their definition of the penalty terms. We discussed about the disadvantage of $S \circ T$ in section 3.1 and made our suggestion on how to improve the past methods using $T^{\#}$. We think there is no need to cite and discuss all papers in the past. That would be a heavy burden to the readers as well.

3, *"The experiment presented in the manuscript (Section 3.2) is inadequate because only a single time-step DA outcome is shown. Please perform some cycled OSSEs that demonstrate that your framework results in root-mean-square-errors that are consistently better than a standard DA method like the EnKF. Even perfect model OSSEs will do. "*

Reply: With one time-step DA, it is already clear that plain EnKF fails and morphed EnKF could reduce the failure of EnKF. However, this suggestion will be taken in our next paper.

4, *"To convince your audience that your framework is superior to other alignment estimation approaches, please perform cycled DA OSSEs with an alternative approach. In my experience, the FAT-type method is not difficult to implement and try."*

Reply: The key point in this paper is that $T^{\#}S$ is superior to $S \circ T$, which has already been demonstrated in Fig.5. In all the FAT-type methods, after you choose the definition of $T^{\#}S$, either in a way proposed in our manuscript or simply using the traditional $S \circ T$, the next step is to choose your penalty terms for your cost function. Different methods have different choices. Despite $T^{\#}$, we are not claiming that our cost function is superior. What we propose in this manuscript is the most simple cost function. It has only two terms, the residue error term $\|S_1 - T^{\#}S_2\|^2$ and the smoothness constraint on the flow field $\|T\|^2$. You can add more terms to make your estimates more stable. Hence we are not suggesting the readers to completely give up the past methods. The readers can improve the past methods by taking our point into account. For instance, as suggested in section 3.1, the field alignment method of Professor Sai Ravela et.al can be improved using our definition of $T^{\#}$. Suppose there are two methods, one has the correct $T^{\#}$ but simple penalty terms (for instance the method in our manuscript), another one has the wrong $T^{\#}$ but more complicated penalty terms. How would you draw your conclusion based on the results? If the previous method wins, would you conclude that $T^{\#}$ is superior or the penalty terms are better? Thus, we think a fair experiment is to use the same penalty terms but different $T^{\#}$. This has already been done in the experiment of Fig.5. If the reviewer wants us to use the penalty terms of Ravela et.al 2007 and the correct $T^{\#}$, and compare the new method with the one proposed in Ravela et.al 2007, then this is beyond the responsibility of the current manuscript.

5, *"Please provide estimates for the computational cost/complexity of your framework, and whether your framework is embarrassingly parallel. A key difficulty in practical DA is the high dimensionality (>10^7) of the state space. If your framework's computational cost/complexity does not scale linearly with dimensionality or is inherently serial (i.e., impossible to parallelize), it cannot be used for practical DA. "*

Reply: The computational aspects of this method is what we are working on right now. As already pointed out in the response of the previous comment, the choice of $T^{\#}$ and choice of penalty terms are two separated things. You can use the $T^{\#}$ in our paper while using the cost function from another paper (with the correct $T^{\#}S$). The definition of $T^{\#}S$ is straightforward and it would keep the computational cost at the same level as if you use the plain $S \circ T$. Thus logically, what the reviewer concerns is not whether our method can be practically used, but whether all FAT-type methods can be used practically. I believe the answer is yes. But there are many other work to do. For now I can only assure the reviewer that using the correct $T^{\#}S$ would not increase the order of the computational cost when using plain $S \circ T$.

6, *"Your in-text reference citations are incorrectly formatted. I believe this is an issue with your citation command in LaTeX or your chosen template."*

Reply: Indeed, when several papers are cited at the same place, I should put them in parentheses. Thank you for pointing it out.

7, *" Please display your algorithms using a text Table, not a print screen. "*

Reply: This is embarrassing. At first I displayed my algorithms using a text table. But then I found a guideline of NPG submission which has some requirement on the algorithm. I do not remember what they required. But I recall that they wrote in the end that if the requirements could not be met, then display your algorithm in a figure. I can not find the same guideline anymore. I will carefully check their requirements again if I were to submit it or revise it.

8, *“ Please check to see if you have defined every symbol you used. For example, the epsilon symbol on used in Line 21 is not defined and sv in (2) is not defined. ”*

Reply: We thank the reviewer for pointing this out. This will be fixed in the next revision or submission.

9, *“ It is unclear to me how (1) leads to (2). Please derive (2) an/or provide a reference that connects (1) and (2). ”*

Reply: Basically, for a given vector field v on the domain, the first term in (2) means how fast S_2 can approach S_1 if S_2 is displaced along the vector field v. Thus the optimization problem in (2) searches for a vector field v, so that it is the most “efficient” direction for S_2 to approach S_1 at the initial time. Once v is found at the initial time, then S_2 should be adjusted along v a little bit. And then search for another v. And this should be done iteratively until some stopping criterion is met. In our numerical experiment we did not provide a stopping criterion but simply set up the number of iteration steps. (2) essentially is a gradient descent method which provides a suboptimal solution to (1).

10, *“ L95: Please define the Hodge star operator. ”*

Reply: This can be found on Wikipedia and many standard differential geometry textbook. I personally do not think it is proper to write a lengthy introduction of some commonly known mathematical concepts. Further more, we think the readers should be approached by mathematics step by step. First we wanted to use plain language to convince the reader that $T^{\#}$ is important. Once the reader gets interested, they will find their own way to learn detailed mathematics. But it looks like we did not succeed implementing this strategy, based on the general comments of the reviewer and the MS records.

The definition of Hodge star operator starts with Riemannian metric. The Riemanian metric introduces the rule of inner product on the tangent space of each point. It further induce the inner product of the tensor fields or differential forms. To define Hodge star operator, first you need to choose an orthogonal basis $\{e_1(p), e_2(p), ..., e_n(p)\}$ for the tangent spaces of each point p. Then you express your differential form ω using the dual basis $\{e_1^*(p), ..., e_n^*(p)\}$: $\omega(p) = \sum_{i_1,...,i_k} a_{i_1...i_k}(p) e_{i_1}^*(p) \wedge \cdots \wedge e_{i_k}^*(p)$, in which $a_{i_1,...,i_k}$ are coefficients depending on p. Then the Hodge star operator $*$ is defined as:

$$*(e_{i_1}^* \wedge \cdots \wedge e_{i_k}^*) = \pm e_{j_1}^* \wedge \cdots \wedge e_{j_{n-k}}^* \tag{6}$$

where the set $\{i_1, ..., i_k, j_1, ..., j_{n-k}\}$ is equal to the set $\{1, ..., n\}$. The sign depends on n and k and the order of $i_1, ..., i_k, j_1, ..., j_{n-k}$. $*$ is a linear operator thus this gives the definition of $*\omega$. You need to prove that this definition does not depend on the choice of basis. This operator will be needed when we handle the displacement of physical fields on the Earth surface (a 2 dimensional manifold). The definition of Hodge star operator is beyond the scope of Calculus III.

11, *" Fig 3 and 4: Please swap the second and third rows. "*

Reply: This suggestion will be considered. Thanks.

References:

[1] Stratman, D. R., and C. K. Potvin, 2022: Testing the Feature Alignment Technique (FAT) in an Ensemble-Based Data Assimilation and Forecast System with Multiple-Storm Scenarios. Mon. Wea. Rev., 150, 2033–2054, https://doi.org/10.1175/MWR-D-21-0289.1.

[2] Ying, Y., 2019: A Multiscale Alignment Method for Ensemble Filtering with Displacement Errors. Mon. Wea. Rev., 147, 4553–4565, https://doi.org/10.1175/MWR-D-19-0170.1.

[3] Bocquet, M., Vanderbecken, P.J., Farchi, A., Dumont Le Brazidec, J., Roustan, Y.: Bridging classical data assimilation and optimal transport: the 3d-var case. Nonlinear Processes in Geophysics 31(3), 335–357 (2024) https://doi.org/10.5194/npg-31-335-2024