

COMMENTS TO THE AUTHORS

Thank you for this valuable feedback. Your comments will help make our manuscript clearer and more scientifically rigorous. We agree that the manuscript contains imprecise mathematical definitions and formulations that prevent the reproducibility and reliability of our work. These issues were difficult to notice without taking a step back from the manuscript. We provide a specific response to each comment below and will revise the manuscript accordingly.

A. SPECIFIC COMMENTS

1) The Mathematical description of the methods and/or computational tools applied in this work has to be much more elaborated and clarified to provide a consistent, complete, and firm Mathematical basis for the results presented. All scalar fields, vector fields, functions, and parameters (especially those related to DIVAnd and SVD methods) have to be firmly defined in strict Mathematical terms, while their designation should be carefully assigned upon first appearance and consistently used throughout the entire manuscript. The physical meaning of any quantity has to be clearly stated. To give an example, introducing Eq.(1), the authors mention that (Ins 184-187, pg 8):

"Therefore based on a cost function, it computes a scalar variable $J(\phi)$ that depends on the difference between the variable $\phi(x_j)$, the observations d_j and the spatial/temporal regularity of the field.

In this sentence, although $J(\phi)$ is denoted as a function, it is referred as a "variable" which is dependent on another "variable" $\phi(x_j)$, which is actually however, a function (of x_j). At the same time, none of the functions and/or variables mentioned in the text is assigned to the actual physical quantities; at a minimum, **the authors should clearly state what physical quantities, fields, or functions in their study, are assigned to the general designation** -especially $\phi(x_j)$ - introduced in the manuscript equations. They also have to clearly specify **what the "observations parameter" d_j represents** (vector velocity, velocity magnitude, direction, velocity components, other. ?).

Thank you for this remark. It is true that the original manuscript lacked clarity concerning the mathematical definitions. To this end, we propose a revised version of the considered paragraph. Additionally, the full text will be revised to ensure that mathematical formulations are clear and well defined.

Original text:

Therefore, based on a cost function, it computes a scalar variable $J(\phi)$ that depends on the difference between the variable $\phi(x_j)$, the observations d_j , and the spatial/temporal regularity of the field.

In this cost function, ϕ is the ocean field, and N_d is the number of observations for the locations x_j ; ϕ_b is the background estimate, which is a first guess of the interpolated field (here ϕ_b is set to zero), and the spatial/temporal regularity is defined using the following norm:

Revised text:

Therefore, based on a cost function, DIVAnd derives a continuous ocean field $\phi(\mathbf{x})$ (where the vector $\mathbf{x}$ represents the geographical position, i.e., longitude and latitude) from a series of N_d discrete

measurements d_i (where $i = 1 \dots N_d$) at the position $\mathbf{x}_i$. To derive a tracer field ϕ , the method minimizes equation 1 with the norm defined in equation 2.s (Eq.1 & Eq.2, revised manuscript).

In this equation, J is a functional that computes the difference between $\phi(\mathbf{x}_i)$ and the corresponding discrete measurements d_i . Moreover, J also minimizes the norm (Eqn 2) of the difference between ϕ and a background estimate ϕ_b , which is a first guess of the interpolated field. For example, if the method aims to derive salinity, the background estimate could be salinity averaged over space and time. Thus, the method ensures that the field is relatively close to the observations and to a first guess. All these requirements are formalized via a cost function: (equation 1)

$$J[\phi] = \sum_{j=1}^{N_d} u_j [d_j - \phi(x_j)]^2 + \|\phi - \phi_b\|^2$$

equation 1

Additionally, they have to explain what is the "*spatial/temporal regularity of the field*", where or how it is involved in Eq.(1), and what field is referred to.

Additional text for “spatial/temporal regularity of the field”:

At the same time, the derived field ϕ is spatially constrained by defining a particular norm (equation 2). This norm penalizes abrupt spatial variations by controlling the relative importance of the Laplacian, gradient and field value. This norm is thus an extension of the commonly used L2 norm. It will ensure that the field does not create abrupt variations, unrealistic for surface currents, which we call the spatial regularity/coherence of the field.

Unfortunately, the notation in the original manuscript was confusing as we used $|\phi|^2$ instead of $\|\phi\|^2$ to denote the norm. This is fixed in the revised manuscript.

NOTE: The first manuscript did not show the vector form of DIVAnd. It only includes the case for scalars with a reference for the vector case. In the revised manuscript, we now also included the formulation for vector fields. We decided to extend Section 3.2 after the subsequent comments, removing doubts regarding the mathematical formulation of the method. For more information, see Barth et al. (2021) 10.1007/s10236-020-01432-x.

Concluding, the meaning of *all* equations have to be clarified, providing firm Mathematical and physical description of any field, function, or parameter involved, with special emphasis given to Eqs.(1) and (2). Moreover, attention has to be given in the different designation between *scalar* and *vector* quantities, that has to be clearly visible in the text (see further the technical comment 11).

J is a functional (and not simply a function) and this should have been clarified in the original manuscript, as it takes as an argument a function and not just a value at a fixed position. In the cost function $J[\phi(\mathbf{x})]$, ϕ is the vector field corresponding to the surface currents, N_d is the number of discrete observations d , ϕ_b is the background estimate, which is a first guess of the interpolated field (here ϕ_b is set to zero), and μ is a free parameter that weights the different observations.

The norm (equation 2) controls the spatial/temporal regularity through the Laplacian, the gradient, and the field values. The coefficients α_0 , α_1 and α_2 are parameters that controls the distance over which the observations are interpolated.

The following parameters are used in the equations 1 & 2 of the manuscript.

$$\alpha_0 = 1 / c$$

$$\alpha_1 = 2 L^2 / c$$

$$\alpha_2 = L^4 / c$$

$$c = 4 \pi L^2$$

$$\mu = 1 / \varepsilon^2$$

where L is the correlation length-scale and ε^2 is the error variance of the observations relative to the error variance of the background estimate. The parameter μ is also called signal-to-noise ratio.

See Brasseur et al. (1996) and Barth et al. (2014) for a justification and derivation of these parameters.

2) SVD is invoked in sub-section 4.4, without any prior reference in section 3 (Materials and Methods). The method must be introduced and firmly described in a short paragraph, along with the relevant notation, in Section 3. Additionally, the authors must comment on the choice of SVD (instead of other relevant methods, perhaps more widely applied, such as PCA, or others), specify the underlying matrix type?, comment on the resulting EOFs spectrum, mention how many independent principal components resulted from their analysis and why they choose only the first EOF, how they treated degeneracy issues (especially in light of the very small proportion, 8.37%, explained by this leading mode), and finally, provide appropriate citations. Furthermore, and importantly, they must explain how they applied a method originally intended to decompose scalar fields, to a vector field [the surface velocity current $\vec{v} \rightarrow = u(\lambda, \varphi) \cdot \vec{i} + v(\lambda, \varphi) \cdot \vec{j}$]

Finally, note that even the introductory description of SVD in sub-section 4.4 is vague and not well-posed, as the authors state that (ln. 419, pg. 21):

" *we decomposed the Ionian Sea (corresponding to 108 months of the DIVAnd result) into modes with orthogonal functions*".

However, the Ionian Sea by itself is not a field, and therefore, it can not be decomposed into anything through SVD. Additionally, the meaning of the sentence in the parentheses is insufficient, as the Ionian Sea is not "*corresponding to 108 months of the DIVAnd results*", but the velocity field $\vec{v} \rightarrow(\lambda, \varphi)$ does so (provided that proper Mathematical description and notation has been given). This point represents just one of many similar insufficiencies and deficiencies in the Mathematical description and notation of the applied methods (DIVAnd and SVD) throughout the manuscript.

1) PCA vs SVD

The Singular Value Decomposition is used to make a Principal component analysis (PCA). We should have also mentioned PCA/EOF (Empirical Orthogonal Functions) in addition to SVD in the original manuscript to clarify that our approach is not an alternative to PCA. In fact, PCA can be computed either as an eigenvector decomposition or an SVD. The SVD is preferred because it has better numerical stability, but they are mathematically equivalent approaches.

We agree on that point, as the SVD techniques are matrix decompositions and need a firm mathematical

definition. The full description of the technique was added in the material and method section, and additional precisions were added to the results section. The answers to the specific questions are given here and will be introduced carefully in the text.

2) Mathematical description

The decomposition is performed on the matrix $\mathbf{X}_a$ (*space, time*) and is mathematically corresponding to:

$$\mathbf{X}_a = \mathbf{U} \mathbf{S} \mathbf{V}^T$$

where $\mathbf{U}$ is an orthonormal matrix (*space* $\times$ *k*) whose columns represent the **principal component (PC) time series**, describing the temporal evolution of each mode. $\mathbf{S}$ is a diagonal matrix (*k* $\times$ *k*) containing the **singular values**, ranked in decreasing order, which represent the amplitude (or explained variance) of each mode. $\mathbf{V}$ is an orthonormal matrix (*time* $\times$ *k*) whose columns correspond to the **empirical orthogonal functions (EOFs)**, representing the spatial patterns associated with each mode.

From this decomposition, we will analyze the dominant spatial modes (**EOFs**) and their associated temporal evolution (**PCs**), which will help identify seasonality and recurring patterns within the selected subdomain.

3) Adaptation of the method for the velocity field

Indeed, PCAs are adapted for two dimensional matrices, and not for three dimensional vector fields. We have to first: transform the arrays into a matrix (as described below), then do the analysis, and last, retrieve the full dimensions to obtain the SVD modes.

The surface velocity fields are described by vectors defined at a specific position and time and are decomposed into two components: zonal (*u*) and meridional (*v*). These components are represented in two 3D arrays whose elements are $u_{i,j,k}$ and $v_{i,j,k}$ where *i*, *j*, and *k* are the longitude, latitude, and time indices.

To combine them into a single matrix suitable for decomposition, we first collapse (or flatten) the longitude and latitude dimensions of each array into a single spatial dimension, removing at the same time the grid points corresponding to the land, retaining thus only valid ocean values. Mathematically, if *l* is an index of the flattened vector, the corresponding indices on the grid of the *u* (or *v*) velocity is a function of *l*, written as *i(l)* and *j(l)*. The singular value decomposition is insensitive to the exact ordering of the elements. The following 2D matrix is then formed for *u*, whose *l*, *k* elements are given by:

$$\mathbf{X}_{u_{l,k}} = u_{i(l),j(l),k}$$

Similarly for *v*:

$$\mathbf{X}_{v_{l,k}} = v_{i(l),j(l),k}$$

We concatenate both matrices $\mathbf{X}_u$ and $\mathbf{X}_v$ into a single combined matrix for each time step, and stack these vectors across time to form the final data matrix, with dimensions **valid ocean points (for *u* + *v*) $\times$ time**.

$$\mathbf{X}_a = (\mathbf{X}_u ; \mathbf{X}_v)$$

From this, we apply the SVD decomposition as shown earlier. We will carry the same reversed steps to retrieve the full field in the form of zonal and meridional velocities defined along longitude, latitude, time.

4) Subsample the DIVAnd results.

To make the EOF analysis focused on the Ionian Sea, we have sub-sampled the velocities corresponding to the region situated between longitude 13°E to 23°E and the latitude from 34°N to 40°N. We call this domain the Ionian Sea in the manuscript, but it rather corresponds to the central Ionian Sea. We can reformulate saying: “We defined a subsample of the Mediterranean Sea velocities corresponding to the longitude 13°E to 23°E and the latitude 34°N to 40°N, which is analogous to the central Ionian Sea. This subsampled dataset was decomposed with the SVD method described in the material and methods, over the full period, corresponding to 108 months.”

5) The number of computed modes, choice of the first one and degeneracy issue

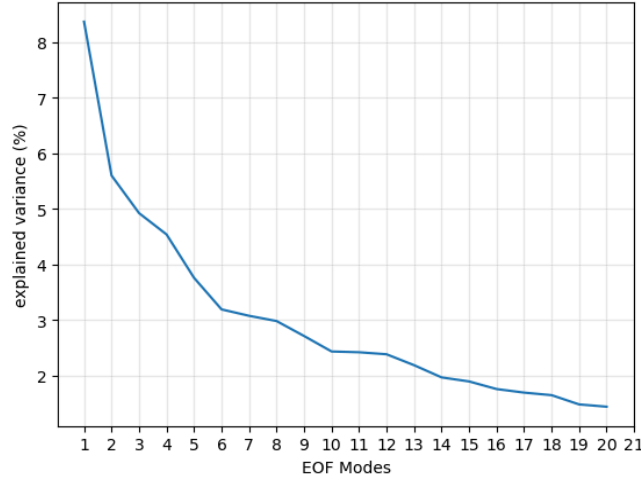


Figure 1: Variance of the 20 first EOF modes

The number of computed modes is equal to the number of time steps within the analysis. For these 9 years period, we have one result per month, corresponding to 12*9 (=108) modes. For these modes, only the first two explain more than 5% of the variance and decrease below 2% after the 14th mode, showing the high variability of the chosen domain.

The choice of the first EOF mode is a logical choice because it shows the sought pattern inside: the Twin Gyres. It allows us to investigate its amplitude through time and to detect temporal patterns related to seasons and interannual variations.

The degeneracy concern in EOF/SVD analysis occurs when two neighboring modes have similar eigenvalues and can no longer disentangle the spatial and temporal patterns (North et al., 1982).

Based on the “North’s Rule of Thumb, we verified the degeneracy of the first mode compared to the second one. The rule is mentioned as following:

$$\delta\lambda_i \approx \lambda_i(2/N)^{1/2}$$

where

$$|\lambda_i - \lambda_{i+1}| > \delta\lambda_i$$

with $\lambda_1 = 8.37\%$ et $\lambda_2 = 5.60\%$

and $N = 107$ due to 108 independent time steps where the mean was removed, we obtain,

$$\delta\lambda_1 = 1.14\%$$

and where

$$|\lambda_1 - \lambda_{i+1}| = 2.77\%$$

we confirm that

$$|\lambda_i - \lambda_{i+1}| > \delta \lambda_i$$

which confirms that the first mode can be analyzed alone.

For mode 2 and 3, the rule shows that they cannot be analyzed separately while the fourth and the fifth can. This last conclusion does not change anything to our analysis as it is only based on the first mode.

When comparing to EOF decomposition of sea surface temperature (as example), it is unexpected that first mode only explains 8% of the variance (e.g. Alvera-Azcarate et al. 2024). We can explain this phenomenon as the spatial scales in SST are typically larger than the spatial scales in sea surface currents. Indeed, the influence of atmospheric processes is stronger on SST than on sea surface currents (the Rossby radius of deformation much larger in the atmosphere than in the ocean (Gill, A. E. (1982). *Atmosphere-Ocean Dynamics*. Academic Press, Orlando, 662 pp.). It is therefore expected to observe lower amplitude in the computed modes related to surface currents.

3) In relation to specific comment 1, the authors introduce "*Importance*" (Fig. 11b and ln. 428, pg. 22) as a quantity related to the principal variability mode of the surface velocity field. Nevertheless, this quantity is not clearly defined, while its very short description is vague. The authors should clarify the exact meaning of this metric and designate it properly. In the subsequent description of the principal variability modes, the meaning of the statement (pg.22, lines 425-426):

"the first (I suppose, that the authors mean the principal) mode which explains 8.37% of the variability of the Ionian Sea (I suppose, that the authors mean of the variability of the Ionian Sea surface current velocity field) for the whole period and explains 30% of the variability at the end of 2020" is insufficient and confusing. How is the contribution of a variability mode specified for a single time step? Do the authors treat the eigenvalue spectrum as a function of time? If so, how do they address EOFs' degeneracy issues at each time step? Does this description comply with standard SVD analysis?

Indeed, our wording was confusing. The word variability should not be used in this context. Instead of using the term "variability", we should have used the term "amplitude", which varies according to time. The explained variance (the singular values) associated to the modes do not move over time as we use a classical SVD decomposition. Therefore, we could reformulate:

Revised text:

"The principal (first) mode explains 8.37% of the variability of the selected sub-domain (corresponding to the central Ionian Sea) over the whole period. This latter grows in winter and has an amplitude peak at the end of 2020 equal to 0.3."

Thus, the eigenvalue spectrum is not a function of time; it is a single value associated to a mode which does not vary according to time. Therefore, the degeneracy issue is not addressed for each time step, and we stick to a standard SVD analysis.

We verified the manuscript to ensure that this error is not repeated.

4) In the "Validation" sub-section 4.1 (pg.11, lns. 263-264), it is stated that:

"To this end, the velocities were separated into their zonal and meridional components and plotted in a scatter plot, from which were derived regression lines".

Delayed as much as after DIVAnd analysis, this sentence states that for validation purposes only, the current velocity $\vec{v} \rightarrow$ is decomposed for the first time ("to this end") into its components u and v . Is that so?

We agree that this sentence lacks context; further explanations are needed.

For the analysis, the velocities u and v are analysed jointly (obtained by minimizing a cost function depending on u and v) as we always need both velocity components to build current maps. The two components of the velocity (u and v) are considered separately when computing validation metrics such as the RMSE and the correlations with the independent observations. Additionally, both components are also decoupled for the SVD decomposition.

We could summarize by saying that: The results are always analyzed with both velocity components (as we need vector maps), while computations are done on separated velocity components.

We will avoid using "decomposed" in the manuscript as it can lead to misunderstanding in the context of SVD analysis and will replace it with "per component" which is less confusing.

Additionally, it appears that the authors compute *regression lines* to quantify the correlation between the interpolated velocity components (for clarity say, u_{int} and v_{int}) and the observed velocity ones (say, u_{obs} and v_{obs}). However, the utilized *linear* regression method is not specified (is it ordinary linear least squares method, Theil-Sen, other ... ?)

This comment is agreed as this linear regression is one of the center points of the validation. The method should be clearly defined for reproducibility and understanding.

The method is the ordinary least squared method. For both the (u_{obs}, v_{obs}) and (u_{int}, v_{int}) we search for the function that relates linearly the observations and the interpolated field. The following equations' slopes and intercept a_u, a_v and b_u, b_v are sought.

$$u_{int} = a_u u_{obs} + b_u$$

$$v_{int} = a_v v_{obs} + b_v$$

Where (u_{int}, v_{int}) are the interpolated field, (u_{obs}, v_{obs}) are the observations used for interpolation, (a_u, a_v) are the slope of the linear functions, and (b_u, b_v) are the intercepts.

This will give us a quantitative basis to compare both. To this end, we use an ordinary least squared method implemented in the package GLM.jl

, while no information is provided about the *statistical significance* or, alternatively, a *confidence interval* for the computed slope values (say, a_u and a_v). Given the aim of this quantification, such information would be very helpful and possibly critical (see below). Even more important, the regression lines are introduced to show how well the interpolated velocity components (u_{int} , v_{int}) fit the observed ones (u_{obs} , v_{obs}). Note that in physical terms, the opposite check (i.e. the observed velocities fit quality into the interpolated ones) would be meaningless, as always, *estimations* have to be compared with *observations*, not the opposite. Therefore, u_{obs} and v_{obs} are expected to be depicted on the horizontal axis of the corresponding diagrams (in Fig.4), and u_{int} and v_{int} on the vertical axis. Nevertheless, the opposite occurs in both panels of Fig.4, showing that the roles of facts (observed values) and estimates (computed values) have been actually reversed. This fact further drives in slope values a_u and a_v greater than one (as explicitly stated in lns.266-267 of pg.11, that $a_u = 1.34$ and $a_v = 1.28$), which are subsequently interpreted in physical terms as (pg.13, lns. 271-272):

"Moreover, we find both of the regression slopes up to one, which means that on average, the independent drifters have higher velocities compared to the interpolation results."

where I suppose that the expression "up to one" actually means that both slopes are *greater than one* (otherwise a contradiction results with Fig.4 and the statement that $a_u = 1.34$ and $a_v = 1.28$). In other words, it seems that instead of linear relationships of the form:

$$u_{int} = a_u \cdot u_{obs} + b_u \quad \text{and} \quad v_{int} = a_v \cdot v_{obs} + b_v$$

for the meridional velocity component u and the zonal component v , which express the *estimated* values as a function of the *observed* ones, the authors tacitly adopt the inverse relationships, that is, equations of the form:

$$u_{obs} = a_u^* \cdot u_{int} + b_u^* \quad \text{and} \quad v_{obs} = a_v^* \cdot v_{int} + b_v^* \quad [2]$$

which express the observed values as a function of the estimated (which is meaningless) and which are subsequently depicted in Fig.4. However, in this way the slope values a_u^* and a_v^* in the above Eq.[2] appear to be greater than one ($a_u^* > 1$ and $a_v^* > 1$), while, according to the expected relationships given in the aforementioned Eq.[1], they are expected to take values lower than one ($a_u < 1$ and $a_v < 1$). Therefore, the correct relationships (Eq.1) have to be adopted, and the physical interpretation of the slope values has to be adjusted accordingly. Towards such a physical interpretation, confidence intervals for the slopes a_u and a_v , might be of great value.

We agree on that point. In our idea, we wanted to compute the relationship to compare both without any further thoughts. The positive slope to compute the observations from the interpolated field was explained, and we were happy with it. However, we agree that it is much more physically meaningful to derive the interpolated velocities from the observations and computing the coefficient to get the interpolated velocities from the observations. Therefore, we revised the scatterplots and the subsequent computations with the

optimal least squared method. The new graphics and metrics are shown here and will be updated in the manuscript.

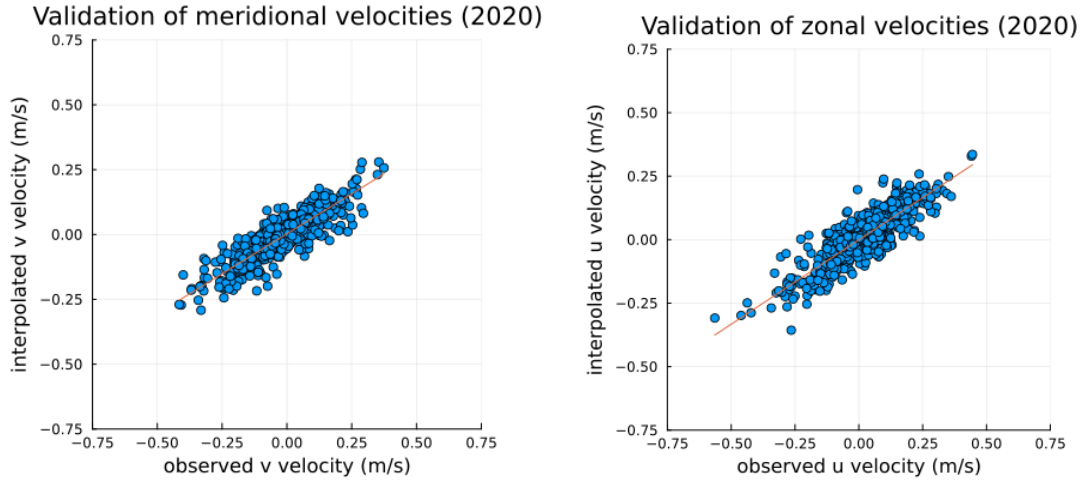


Figure 2: Comparison of the interpolated and observed velocities and linear regression computed with the least squared method.

Giving new metrics for the linear regression: (expressed in m/s)

$$u_{int} = 0.66 u_{obs} + 0.005$$

$$v_{int} = 0.62 u_{obs} + 0.003$$

Both p-values related to u & v velocities are below $1e-99$, indicating that the probability of observing such a relationship under the null hypothesis is extremely low, which means the relationship between the two variables is highly statistically significant.

The significance of the slopes (i.e., the confidence intervals) are computed for both of the new observation/interpolation relationships. The confidence intervals are equal to ± 0.015 and ± 0.013 for meridional and zonal velocities, respectively, which are both an order of magnitude below the speed computed here.

The conclusion remains the same as the one raised in the manuscript, as DIVAnd smooths the field for the interpolation to be coherent. However, the physical explanation now makes sense as we explain the interpolation results from the observations, which is coherent with our framework.

5) Introduction of the correlation length L or l (pg.9, ln.210 and pg.11, ln.234) and the error variance ϵ^2 (pg.9, ln.211) or ϵ as referred in Table2 (and later, in ln.234, pg.10, possibly associated with the signal-to-noise ratio) is insufficient, while the description of their computation is exhausted to a reference to the "Julia Black Box Optim". Instead, their implication to the RMSE given by Eq.(5) should be clearly stated, and a short description of their computation method should be given, along with appropriate citations.

Both correlation length (L) and the estimate of the relative error variance (ϵ^2) are free parameters of the method.

The estimate of the relative error variance ϵ^2 is introduced in the cost function as $\mu = 1/\epsilon^2$, which will weight the observations. To distinguish every data source (satellite, drifter & HFR), we decided to associate a different ϵ^2 to the different sources of observations. With the optimization of the parametrization, it provides precious information as it will quantify the optimal contribution for each dataset and will estimate their reliability compared to the independent drifters.

The correlation length is proportional to (α) in the norm and will set the range to which information the observations will be extended. This free parameter is unique for all the observations (the observations are all constrained by only one value).

The optimization of the parametrization is carried by testing different values of ϵ^2 (distinct values for drifter, satellite and HFR) and L . The tested values are in a given range that we set based on spontaneous results obtained with manual parametrization and our knowledge of the technique. To find the most accurate parameter values, we sought the set of parameters that lead to the lowest RMSE. To do that, we use an algorithm that uses an adaptive differential evolution optimizer that runs for a given number of iterations to find the best parameter values as implemented by the Julia package BlackBoxOptim. At each iteration, the optimizer generates new candidate parameter sets by combining and perturbing existing ones, evaluates their performance based on the resulting RMSE, and retains the best-performing combinations to guide the search in subsequent iterations.

Furthermore, note that the correlation length L (ln.246, pg.11) appears to be written as l in Table 2. Similarly, the second parameter, designated as ϵ^2 in ln.246, pg.11, that was earlier introduced without any description or designation -possibly erroneously- as signal-to-noise ratio (ln.234, pg.10), is referred as ϵ and without units in Table 2 (although, units of m/s are subsequently assigned to RMSE, in lns.259-259, pg.11). Authors have to use a consistent notation and clearly define all the implicated parameters.

We agree on the fact that we lacked consistency throughout our notations, and we will read the manuscript again, paying attention to write everything in the same way.

6) As shown in the following list of "technical corrections", many syntax or phrasing errors are spread throughout the entire manuscript, resulting in insufficient sentences that downgrade the text quality, readability, and comprehension. Therefore, the English language needs careful elaboration, and many statements (particularly those related to mathematical methodology, notation, and results) need to be rephrased or corrected by an English-language expert.

This comment agrees with one of the first reviewer comments. We will pay attention to the syntax and

phrasing errors within the manuscript to improve the quality and comprehension of the manuscript.

TECHNICAL COMMENTS

1. I believe that the preposition "on the central Ionian" in the manuscript title has to be replaced by "into the central Ionian".

Thank you for this point. Indeed, “into” is better and more accurate.

2. In pg.1, ln.18, provide appropriate citations and/or web links for "Copernicus" and "EMODnet" platforms.

The appropriate citation for the database has been added to the revised manuscript.

3. Apply the standard hierarchy with parentheses and brackets, that is { [()] }, as in many places in the manuscript, mostly related to citations, parentheses are enclosed within another parentheses. For instance, in lns. 28-29, pg.2, replace the sentence:

"(e.g. Cotroneo et al. (2016); Poulain et al. (2013); Soto-Navarro et al. (2010); Martínez et al. (2024))"

by

"[e.g. Cotroneo et al. (2016); Poulain et al. (2013); Soto-Navarro et al. (2010); Martínez et al. (2024)]"

The same is valid for the similar cases spotted in ln.47, pg.2, in lns.33, pg.16, and in ln.356, pg.18 at least.

This remark has been revised in the entire manuscript; your remark makes citations clearer.

4. Provide an estimation of the referred short water residence time of the Mediterranean Sea (ln.53, pg.2), along with appropriate references.

Thank you, we overlooked giving an exact number for the residence time of water.

We added an appropriate citation and the exact number to support the statement.

5. Change the single-column Table 1 to two-columns Table.

The table was changed accordingly. Thank you.

6. Provide full names and appropriate citations for DIVAnd and CMEMS upon their first

appearance in ln.145 and 146 of pg.7, correspondingly. Do the same for any other acronym.

The proper citation is given right after DIVAnd with the full name of the method. For CMEMS, the citations are given for each dataset used in the paper. However, we do not have any citation for CMEMS in general.

7. Correct the sentence (ln.165, pg.7) "...the Ibiza Island, *on the at the* Ebro river delta,..." as well as in ln.169, pg 7 "...is conserved, since *the of the* regridding ".

We appreciate those remarks. We corrected it and had a check of those typos within the entire manuscript.

8. The information carried by the field maps in Fig.2 is important. Therefore, it merits a much easier view. For this reason, all panels must be magnified to the maximum width.

We agree with your point, as this information is crucial. This was changed as we separated the drifter panel in two different maps (see the response to the first reviewer), and the HFR and Satellite maps are now larger as they are in a separated figure.

9. Eqs. (1) and (2) are not "*physical laws*" as stated in ln.194, pg.9. The same is valid for the similar statement in the parentheses in lns.459-460 of pg.23.

Thank you for this remark. Indeed, it is wrong to name it physical laws, as it is a mathematical technique. We changed the whole paragraph according to this comment and with the first reviewer's.

10. I suppose that the " $\sim$ " symbol in relations (3) and (4) has been used with the meaning of "*almost equal*". If so, then the correct symbol ($\cong$) must be used.

It is true, we overlooked the meaning of $\sim$ which has a clear mathematical significance. We changed the symbols of those equations with the $\approx$ (\approx in LaTeX) mathematical symbol.

11. The vector symbol for "n" is missing in Eq.(3), as well as in the subsequent sentence (ln.197, pg.9). As a vector it has to be notated either as $\vec{n}$ (recommended) or as **n** (bolded). Otherwise, confusion is risked, as for instance occurs in the notation of observations number (n) used by the authors in Eq.(5), also referred as N_d in Eq.(1). In the same reasoning, please note that the symbol u (in ln.197, pg.9) is referred as "*the velocity adjacent to the coastline*" although, throughout the manuscript has been declared as the meridional component of the current velocity $\vec{v}$. As it was stressed earlier (see the last sentence in the Specific Comment 1), much attention has to be given to the different notation between *scalar* and *vector* quantities.

It is true that our equation writing is confusing. We revised it as we bold the vectors within the mentioned equations (following the [Copernicus style guide](#)). Accordingly, the paragraph was revised to make the equations clearer for the reader.

12. Correct the title of sub-section 3.3 as " parameterization".

We did the change, thank you.

13. In connection to the mean dimensions of grid elements, a latitude average equal to 49° is mentioned in ln.216, pg.9, that seems to be outside the adopted domain limits (which are bounded northwards by 47°N).

This is a mistake, thank you for spotting it. We corrected this with the right boundaries and the subsequent calculus for the grid size.

14. In ln.229, pg.10, u and v should be mentioned as "velocity *components*" not as velocity.

Indeed, these are components of the velocity. This is clearer now.

15. In ln.231, pg.10, clearly specify what the physical quantity or parameter is designated by $\hat{y}_i$ (velocity, velocity magnitude, velocity direction, velocity components, other ?).

The definition of those quantities were refined accordingly.

Respectively, specify the parameters for which Eq.(5) is applied (as in ln.243, pg.10).

??

16. On pg.10, give appropriate references for "Julia Black Box Optim" and "DUACS".

SciML Open Source Scientific Machine Learning. *SciML/BlackBoxOptim.jl*. Julia. 28

octobre 2013, Released 12 août 2026.

<https://github.com/SciML/BlackBoxOptim.jl>.

Thank you, we added a proper citation for the package and better explained its use in the text.

17. Consider "Parameter values" as a title of sub-section 4.1.1 instead of "Parameter value", since many parameters are treated there.

Thank you for pointing out this typo. We checked for those errors within the rest of the manuscript.

18. Resolve notation consistency issues in Table 2. What is the meaning of the symbol "/" or

"l" in the upper first cell? Add the notation "L" of the correlation length in column 1, line 2, as in the other lines. Furthermore, in ln.246, pg.11, ϵ^2 is declared as the second parameter to be listed in Table 2, yet ϵ is actually listed in this Table. In the caption of Table 2, the "epsilon" parameter, which was previously defined as "error variance" (ln.207, pg.9), is now described as "an error ratio", while in ln.234, pg.10, it further appears to have been associated with "signal-to-noise ratio". Please clarify accordingly, providing an exact Mathematical definition of this parameter and consistently follow a unique notation throughout the text. Additionally, the "error parameter" ϵ appears to be dimensionless in Table 2, but later (ln.259, pg.11) it is stated that "The error for December is equal to 0.072 m/s for the meridional velocities (u) and 0.029 m/s for the zonal velocities (v), which are..."

We agree that the document lacked consistency in the notations. We revised everything to ensure that this is not the case anymore. Thank you!

19. The sentence "Indeed, whenever...the error variance" in lns.251-253, pg.11, is incomprehensible. Please rephrase appropriately.

We reformulated it as "indeed, whenever two observations are similar in space and time, the associated error variance..." which makes it clearer for the reader. Thank you!

20. The title of sub-section 4.3 "Seasonal variation with climatic averages per month of 2021" is misleading and hardly comprehensible. Further note that climatic averages can not be inferred from one or a few years. In general, climatic averages are implied by 30 years of observations or more. Prefer the term "averages" instead of "climatic averages" for mean values implied by a limited number of data, providing always the reference period.

We agree that "climatic" can be confusing. We decided to remove this word from the paper and often reformulated it as "decadal averages" to be more precise.

21. What is the meaning of "consistent enough" in the statement (lns.305-306, pg.15): "The Center Alboran Gyre and the Eastern Alboran Gyres are not consistent enough neither in winter nor in summer to be captured on these averages,..."? Please clarify or rephrase appropriately.

Consistent enough means that we observe it in the decadal averages. We clarified in the manuscript to make it clearer for the reader. We wrote "not consistent enough neither in winter nor in summer to be captured on these averages..."

22. What is the "*climatic scale*" referred to ln.312, pg.16, and what analysis result represents the "*climatology*" referred in the next line? Do nine years of observations constitute a climatology for any parameter?

We agree, we can give the same answer as the point 9 of this section.

23. Consider "*enhancement*" instead of "*emphasis*" in the sentence of ln.324, pg.16.

Thank you for pointing this out, we agree with this change.

24. Remove repeated semicolons (;) in the sentence located in lns.328-329, pg.16, and rephrase appropriately. Moreover, consider the expressions "...*the GLG is unstable*..." or "...*the GLG is not persistent*..." instead of "...*the GLG is not clear*..." in the sentence in ln.330, pg.16.

We changed the word "spontaneous" with the word "monthly" and removed the word "clear" which is not consistent with "unstable" as suggested.

25. Prefer months' acronyms (such as DEC, JUN,...) instead of their serial number in the legends of the six panels given in Fig.8, 9, and 10. Furthermore, the word "*spontaneous*" in the caption of Fig.8 is not compatible with the depicted mean monthly circulation. Please clarify accordingly.

The shortened months names are now used in the text and in figure description. For the word "spontaneous" we changed it in the same way as said in point 13 of the section.

26. Do the authors mean "*temperature distribution*" instead of "*temperature profile*" in ln.346, pg.18?

It is true that profiles often refer to vertical distribution in the ocean. The word distribution is therefore preferred. Thank you!

27. Please verify the meaning of the sentence "*However, the resultswest to east*)" in lns.357-359, pg.18. In its current form, it seems to state that the TGs are independent turbulent flow patterns interpolated in the MIJ path, and not a side-effect of MIJ (for instance, turbulent flow patterns associated with shear velocity in MIJ). This estimation is also stated a few lines later (ln.363, pg.18), where it is noted that the MIJ crosses the twin gyres. In any case, the sentences stated in lns.355-359 have to be rephrased in order to clearly state their physical meaning, as well as the possible cause of the TGs after 2020.

The section concerning the TGs, their appearance and associated explanations was completely revised. This new explanation should meet your expectations as it is made

simpler, better documented with literature, and clearer regarding the explanation.

28. The meaning of the sentence "*The LEC is the strongest and the fastest current of the averaged seasons (figure 7)*" in ln.370, pg.19, is vague, especially in terms of the "*averaged seasons*". Please rephrase appropriately.

We changed it saying "fastest winter current ... based on seasonals means over the considered period" which is now clearer. Your comment made the text clearer!

29. The statement that "*the climatic average does not retain its complete signature*" in ln.393, pg.20, is deficient and misleading. Please clarify accordingly.

It was changed with: "we do not spot it on decadal averages".

30. The sentence "*This gyre is the only gyre spotted on climatic averages from the Cyprus Eddies system depicted in the introduction*" in lns.393-394, pg.20, should be rephrased appropriately, avoiding repetitions and providing an exact reference to the introduction.

It is true that it is not clear, we agree with you. We reformulated as: "This gyre is the only gyre of the Cyprus Eddies system retained by the decadal averages depicted in the introduction section 2.2".

31. The fact stated in the sentence "*Further, the Rhode Gyre is not seen on the averages.*" (ln.396, pg.20) has to be clarified and further commented.

This is the same conclusion with point 30. We changed it for clarity. The reformulation is "Further, the Rhode Gyre is not captured on the summer/winter averages due to its variability in space and time"

31. Although Fig.9 depicts two different snapshots of the mean monthly velocity field (one on 6/2013 and the other on 10/2021), the alleged "*clear transition*" (ln.404, pg. 20) between two circulation modes of the central Ionian surface velocity field, before and after 2016, is not clearly seen or documented and has to be clarified. The authors may consider applying change-point detection techniques to appropriately selected flow parameters to document the suspected transition beyond any doubt (the realization of this task is entirely within the authors' research planning).

We agree that this section was not well written nor well explained. The paragraph was revised in depth and the explanation regarding the appearance of the TG was revised and explained along the velocity of the AIS and the rotation of the NIG, and the subsequent changes over the MIJ velocity.

32. The mean monthly surface velocity field maps depicted in the panels of Fig.10, along with the corresponding SST field, are hardly seen, as well as the unit velocity vectors. All panels need to be enlarged appropriately, and the velocity vectors visibility need to be enhanced.

It is true that the arrows were super thin and hard to be seen. We changed the colorbar for colorblind people and made the arrows larger. Thank you for noticing!

33. The description in caption of Fig.11 is insufficient and misleading. The three panels (a, b, c) are not explained separately, even though they convey quite different and critical information. In particular, Fig.11 does not show the "*Ionian Sea SVD decomposition*" as stated in the caption, but the ""*SVD decomposition of the Ionian Sea mean monthly surface velocity field*". All panels must be described appropriately and separately, while the term "*importance*" shown in panel (b), has to be firmly defined in Mathematical terms along with its physical meaning.

The description of the figure is now much more precise. We agree that those elements were out of the context of the SVD decomposition. We revised the text and the figure description accordingly and should be clear now.

34. How is a "*high correlation*" between TGs and AIS (ln.433, pg.22) implied simply from eye inspection of Fig.11a or by the fact that AIS and MIJ are successive flow patterns? What are the correlated quantities or features? The TGs and AIS flow patterns, their velocity magnitudes, their velocity directions, their velocity circulation, other...? Actually, the entire paragraph should be carefully rephrased and clarified, and the same applies to the three paragraphs extended along the lns.441-457 of pg.23.

Thank you for remarking this. We can bring the same conclusion as on point 27 here.