Review report for Ocean Science EGUsphere Manuscript Number:

**egusphere-2026-1627**

entitled:

"**Mediterranean Sea surface currents obtained with a variational inverse method, insight on the central Ionian Sea**"

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In this article, the authors perform a 9-year reconstruction (2013-2021) of the Mediterranean Sea surface velocity field by applying the multivariable interpolation method DIVAnd, based on satellite altimetry, drifter, and HF radar data. They consequently examine the monthly, annual, and overall (9-year) mean fields to study and discuss the reconstructed surface flow patterns in terms of persistent currents and gyres, as well as their variability, emphasizing seasonal changes. In this framework, they further focus on AIS-related flow patterns in the central Ionian.

Overall, this work represents a valuable contribution to the detailed examination and research of the Mediterranean Sea surface circulation characteristics, based on multiple data sources (synoptic and mesoscale) and on an efficient interpolation method (DIVAnd). Additionally, the focus on the Ionian Sea is very interesting and well posed, as this Mediterranean sub-region is among the least-studied marine areas in the basin. Nevertheless, weaknesses in the Mathematical formulation and descriptions, oversights in the associated notation, and insufficient statements downgrade the text's quality and readability, leaving the reader with open questions. These issues are described more analytically below in the "Comments to the Authors" section, where I provide six major (specific) comments, as well as several technical comments, that I hope will help the authors improve the manuscript.

## COMMENTS TO THE AUTHORS

### 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 (lns 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....?). 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. 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).

**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 $\vec{v} = u(\lambda,\varphi)\cdot\vec{\imath} + v(\lambda,\varphi)\cdot\vec{\jmath}$ ]. 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 $\vec{v}(\lambda,\varphi)$ 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.

**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?

**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 $\vec{v}$ is decomposed for the first time ("*to this end*") into its components u and v. Is that so? 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 ... ?), 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:

$$u_{int} = a_u \cdot u_{obs} + b_u \qquad \text{and} \qquad v_{int} = a_u \cdot v_{obs} + b_u \qquad [1]$$

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:

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

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.

**5.** Introduction of the correlation length L or *l* (pg.9, ln.210 and pg.11, ln.234) and the error variance $\in^2$ (pg.9, ln.211) or $\in$ 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.

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 $\in^2$ 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 $\in$ 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.

**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.

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

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.
4. Provide an estimation of the referred short water residence time of the Mediterranean Sea (ln.53, pg.2), along with appropriate references.
5. Change the single-column Table 1 to two-columns Table.
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.
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....".
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.
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.
10. I suppose that the "~" symbol in relations (3) and (4) has been used with the meaning of "*almost equal*". If so, then the correct symbol (≅) must be used.
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.
12. Correct the title of sub-section 3.3 as " parameterization".
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).
14. In ln.229, pg.10, u and v should be mentioned as "velocity *components*" not as velocity.
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...?). 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".

17. Consider "Parameter values" as a title of sub-section 4.1.1 instead of "Parameter value", since many parameters are treated there.
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, $\in^2$ is declared as the second parameter to be listed in Table 2, yet $\in$ 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*" $\in$ 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*...".
19. The sentence "*Indeed, whenever....the error variance*" in lns.251-253, pg.11, is incomprehensible. Please rephrase appropriately.
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.
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.
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?
23. Consider "*enhancement*" instead of "*emphasis*" in the sentence of ln.324, pg.16.
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.
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.
26. Do the authors mean "temperature *distribution*" instead of "temperature *profile*" in ln.346, pg.18?
27. Please verify the meaning of the sentence "*However, the results .....west 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.

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.
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.
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.
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.
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).
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.
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.
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.