Response to reviewer 2

We thank the reviewer for the useful and inspiring comments. In the following, we first respond to the General observations and then we address point by point the Detailed comments. For the sake of clarity, we enumerated them. Line numbers in the response to comments refer to the revised version of the manuscript unless otherwise stated.

General observations

The manuscript evaluates the impacts of different observation types on the Corsica Channel transport in a simulation of the NW Mediterranean Sea using the ROMS model with 4DVar data assimilation scheme. It follows a well-established methodology that have been applied many times to different regions around the globe. It represents an interesting contribution, that can be helpful in setting up a forecast system and to define future investments in the regional ocean observing system.

I feel the text would benefit from a major revision to strengthen its conclusions and improve the readability and figures. In particular, the discussion is fragmented and the figures are inconsistent in use of axes and colorbar ranges, making them hard to compare.

The conclusions summarize well the main results, but lack a closing statement that clearly defines the contribution of this paper and the expected importance in the larger picture.

We thank the reviewer for her/his overall positive feedback and suggestions. We have revised the Introduction to improve its flow (see points 1, 3, 4, 5, 6, 7). We rearranged the structure of the Results and Discussion section splitting into two different sections: 3. Results and 4. Discussion. Section 3 is, in turn, split into 3.1 Performance of the DA system, and 3.2 Observation impact, whereas Discussion is composed of 4.1 Changes in circulation and 4.2 Insights into DA mechanisms (specifically see point 33 below). This makes the draft better structured and organized. We modified the colorbar of Figure 7 and Figure 10 (specifically see point 64 below) to make it more readable. We also added a closing statement in the Conclusion (specifically see point 45 below).

Detailed comments

Introduction

1) Highlight the differences of your simulation to the Mediterranean Sea Physics Reanalysis, apart from resolution.

Our setup differs from the Mediterranean Sea Physics Reanalysis (Escudier et al, 2021) in the following several aspects: a) horizontal resolution (see also point below); b) the use of 4D-Var method instead of 3D-Var; c) the assimilation of surface velocity from HF radar data; and d) the assimilation of in situ velocity data from the mooring system located in the Corsica Channel.

We modified part of the introduction as follows:

“In this paper, we present an improved version of the 4D-Var DA system previously implemented

by Bendoni et al. (2023) for the north-western Mediterranean, using the ROMS model (Shchepetkin and McWilliams, 2003, 2005). In addition to HFR-derived surface velocities and satellite SST, the system also assimilates SLA, in situ temperature, salinity and current profiles for the year 2022. It also represents an improvement over the Mediterranean Sea Physics and Reanalysis (Escudier et al., 2021), provided by the Copernicus Marine Environment Monitoring Service (CMEMS), by increasing the resolution from 1/24° to 1/36°, the use of a 4D-Var instead of a 3D-Var algorithm, and the assimilation of velocity observations".
Lines 83-89 updated manuscript.

2) What difference does your higher resolution makes? Why is this necessary?

The internal Rossby radius in the northwestern Mediterranean is of the same order of magnitude of the resolutions generally used in the literature (Beuvier et al., 2012). An increase in resolution is generally desirable to enrich the simulation and resolve smaller scales (i.e meso and submesoscale). We had to compromise between the resolution, the computational effort requested by the data assimilation procedure and the available computational power. Furthermore, with a view to employing the DA system as a donor model for downscaled coastal application, a step in resolution starting from 1/36° (our ROMS application) is preferable than 1/24° (NEMO).

- Beuvier, J., K. Béranger, C. Lebeaupin Brossier, S. Somot, F. Sevault, Y. Drillet, R. Bourdallé-Badie, N. Ferry, and F. Lyard (2012), Spreading of the Western Mediterranean Deep Water after winter 2005: Time scales and deep cyclone transport, J. Geophys. Res., 117, C07022, doi:10.1029/2011JC007679.

3) Introduction lacks flow. In special the transition between the examples of 4DVar systems and the oceanography of the north-western Mediterranean Sea at lines 28-29.

We reformulated the passage as follows:

"...and in the Mediterranean Sea with specific application in the coastal area of the Tyrrhenian Sea (Iermano et al., 2016), in the Adriatic Sea (Janeković et al., 2020), and in north-western part of it (Bendoni et al., 2023).

The north-western Mediterranean Sea, also known as the Liguro-provençal Basin, is a crucial maritime…"

Lines 26-29 updated manuscript.

4) Lines 29-35 – Will become more readable if broken up into smaller ones.

We enlarged and rephrased this part of the introduction, now it reads:

"The north-western Mediterranean Sea, also known as Liguro-provençal Basin, is a crucial maritime region, hosting the Pelagos Sanctuary (Notarbartolo di Sciara et al., 2008) and several Marine Protected Areas (Francour et al., 2001). It is characterized by a cyclonic circulation (Northern Gyre) that involves Atlantic Water (AW) and modified Atlantic Water (mAW) at the surface, and Eastern Intermediate Water (EIW) and Tyrrhenian Intermediate Water (TIW) at intermediate depths. Winter Intermediate Water (WIW) is mostly located at intermediate depth in the western part of the basin, whereas Western Mediterranean Deep Water (WMDW) occupy the deeper layers (Astraldi et al., 1994; Napolitano et al., 2019; Schroeder et al., 2024; Barral et al., 2024).

Around Corsica, the Eastern Corsica Current (ECC) and the Western Corsica Current (WCC) flow along opposite coasts and converge to form the Northern Current (NC, or Liguro-Provençal-Catalan Current), which flows cyclonically along the Italian and France coasts, up to the Catalan coast (Astraldi et al., 1990; Millot, 1999). The ECC passes through the Corsica Channel (CC), a narrow strait between northern Corsica and the Capraia Island, about 30 km wide at the surface and 450 m deep. This strait represents the main connection between the warmer, saltier Tyrrhenian waters and the colder, fresher waters of the Liguro-Provençal basin (Bethoux, 1980; Astraldi et al., 1990).
The northward transport across the CC shows a marked seasonal cycle, with higher values during winter and spring and a net reduction in summer and autumn (Astraldi and Gasparini, 1992), occasionally reversing direction (Sciascia et al., 2019). This modulation aligns to the seasonal dynamics of the Tyrrhenian Sea. In winter, it is characterized by a large-scale cyclonic circulation, when both surface and intermediate waters flow along the Italian coast and bifurcate, one part reaching the CC, and the other veering southward to join a semi-permanent cyclonic structure close to the Bonifacio Strait called Bonifacio Gyre. In summer, most of the water mass is recirculated within the Tyrrhenian basin with little outflow toward the Ligurian Sea (Astraldi and Gasparini, 1994; Artale et al., 1994). In addition, the modulation of transport through the CC can be affected by the presence of a recurrent anticyclonic structure, peculiar of the summer season and located in the channel area, known as Ligurian Anticyclone (LA) (Ciuffardi et al., 2016; Iacono and Napolitano, 2020)."
Lines 29-50 updated manuscript.

5) Lines 58-60 - This sentence is somewhat misleading, since modelling studies without DA can't by definition assess the impacts of DA. I recommend reviewing and potentially removing the reference to the non-DA simulations here.

We reformulated the sentence as follows:
"Several modeling studies without DA (Béranger et al., 2005; Sciascia et al., 2019; Poulain et al., 2020) analyzed the dynamic of the CC. Those, instead, using DA (Vandenbulcke et al., 2017; Escudier et al., 2021) and including the CC in their computational domains lack a specific assessment of how assimilation improves the current representation in the channel."
Lines 73-75 updated manuscript.

6) Lines 64-65 - I don't understand how the observation impacts can be used to evaluate the dynamical scales. Could you, please, explain.

In this statement we mean that by identifying the spatial distribution of observations contributing to the variation of a certain quantity, it is possible to have an estimate of the spatial extent and scale involved. However, we agree with the reviewer that the statement is too generic and can be misleading. We modified it as follows:
"Evaluating the contribution of different observations to transport increments through the CC helps clarify how DA constrains the model through the relevant physical mechanisms. Furthermore, the spatial distribution of the assimilated observations to which the transport increment is most sensitive reveals the regions that are potentially most influential for transport variability."
Lines 79-82 updated manuscript.

7) Lines 66-69 – It gives the impression that there are new observations assimilated that represent the "improvement" in relation to Bendoni et al. (2023). However, the first paragraph of session 2.1 gives a different idea. Therefore, this paragraph could use some rephrasing for more clarity.

We modified the text at the beginning of Section 2.1

"The ROMS 4D-Var Data Assimilation system (Moore et al., 2011b) implemented for this study (version 4.3), builds upon the configuration developed by Bendoni et al. (2023), with modifications applied to the data assimilation framework, expanding the amount and type of assimilated observations (see Section 2.2) and by extending the analysis to a whole year."

Lines 97-99 updated manuscript.

Session 2.1

8) Line 76 – Please specify the version of ROMS you used.

Done, version 4.3, see also point 7 above.

9) Line 111 – Can you justify your choice for the number of outer/inner loops and assimilation window length?

The choice for the combination of inner/outer loops and assimilation window length is based on a compromise between literature values, and computational resources. Furthermore, since we focus on the observation impact methodology, and Levin et al. (2020) showed that the most part of the ∆I is ascribable to the first outer loop, we opted for this choice.

We modified line 111 as follows:

"In this study, we use a single outer loop and 9 inner loops trying to find a compromise between the available computational power, the time required to run the experiments and the reduction of the cost function".

Lines 139-140 updated manuscript.

10) Were there any sensitivity experiments?

We performed some sensitivity experiments on the amount of inner loops, but not covering the whole year. Sensitivity experiments were principally performed on the type of assimilated observations and the characterization of the standard deviation for the calculation of the background error covariance matrix.

11) Lines 112-114 – This interchangeable use of the terms "forecast" and "background" is confusing. Background is the accepted term used in data assimilation literature, while "forecast" is reserved to the "free run" initialized from an analysis. Please, review this and correct it through the text.

We can understand the reviewer's request but in data assimilation the two terms can refer to the very same simulation and, based on the context, they have a different sense. In our framework, we use the same length for the forecast and for the assimilation window. We run the (n)th forecast for 3 days starting from the last time step of the previous (n-1)th analysis. Such a

forecast is indeed the background for the n(th) analysis cycle. When we refer, for example, to the run about which the nonlinear model is linearized, we use the term background. When, on the contrary, we refer to the skill and performance of the run starting from the analysis in an analysis-forecast sequence, we use the term forecast.
This is a case where background and forecast overlap, but they can differ (e.g. in case the assimilation window is 3 days and the subsequent forecast is 7 days). We believe it is important to keep them separated based on the context of reference.

12) Moreover, I don't understand how this FR was run.
The freerun FR was run starting from 2019 up to the whole 2022 and no assimilation was performed.

13) Did you do a forecast after each analysis?
Yes, we run the forecast after each analysis.

14) Why not use your previous 4DVar analysis as initial conditions for the next assimilation window?
It is what we did. The last time step of each analysis was used as the initial condition for the subsequent analysis window.

15) Did you consider overlapping analyses?
No, we did not consider overlapping analysis.

16) Please, clarify how this was run and the rationale behind it.
We reformulated the sentence as follows:
"The analysis/forecast sequence is characterized by a 3-day long assimilation window followed by a 3-day forecast, resulting in 122 windows over the year 2022. No overlapping between analysis (AN) is performed and each analysis uses the forecast (FC) from the previous window as background; hence, in the following, the terms "forecast" and "background" are used interchangeably. The choice of 2022 is motivated by the broader availability of HFR observations during this year."
Lines 140-144 updated manuscript.

Session 2.2:

17) You give no explanation for how your observation errors were defined. The values look like what I would expect for instrument error, where representativeness errors would be expected.
Observation errors are based on literature values and, in general, apart from the correct order of magnitude, they can be seen as a sort of tuning parameters. However, the calculation of superobservations (when several data are within the same computational cell), is performed as max(err_obs, std_data) where obs_err is the error assigned on the basis of literature values and std_data is the standard deviation of the data within the cell.

18) In addition, there is no spatial structure to the errors. Could you, please, justify your choices?

Spatial error is not included since the observation error covariance matrix R is assumed diagonal and does not contain spatial correlations among observations. This simplifies the assimilation procedure a lot. Attempts to relax this hypothesis are an active field of research:

- Goux, O., Weaver, A. T., Gürol, S., Guillet, O., & Piacentini, A. (2025). On the impact of observation-error correlations in data assimilation, with application to along-track altimeter data. Quarterly Journal of the Royal Meteorological Society, e5026.

19) Lines 127-135 – The 3-hour low pass filter of the HFR velocities would still contain a tidal signal. However, you did not mention if and/or how you included tides in your simulation. Although the tidal signal is small in this area, this inconsistency can impact a "strong-constraint" DA scheme where all main physical processes are in principle included. Could you, please, elaborate how you deal with this?

We did not take into account the tidal signal since tides in the area are in the order of centimeters and, furthermore, the boundary conditions are daily averaged values and do not contain the tidal signal. We did not perform a specific filtering procedure on the HFR velocities for simplicity and because we assumed the tidal signal in the velocity field would be negligible.

We added the following:

"Since the tidal signal in the area is of the order of centimeters, and considering that we use daily averaged values as boundary conditions, we did not take into account tides in the modelling system."

Lines 103-105 updated manuscript.

We also added the following in section 2.2 Assimilated observations:

"We did not perform a specific procedure to remove the tidal signal since we assumed the tidally induced velocities to be negligible."

Lines 161-162 update manuscript.

20) Lines 145-168 – Please, break into separate paragraphs by observation type. This will help make it clear the different procedures applied to each observation.

Done, it was an error to have a single paragraph.

21) Line 146 – Please, add the reference for CORA.

Done, we added the references:

- Szekely, T., Gourrion, J., Pouliquen, S., Reverdin, G., and Merceur, F.: CORA, coriolis ocean dataset for reanalysis. SEANOE [data set], https://doi.org/https://doi.org/10.17882/46219, 2025.
- Cabanes, C., Grouazel, A., von Schuckmann, K., Hamon, M., Turpin, V., Coatanoan, C., Paris, F., Guinehut, S., Boone, C., Ferry, N., de Boyer Montégut, C., Carval, T., Reverdin, G., Pouliquen, S., and Le Traon, P.-Y.: The CORA dataset: validation and diagnostics of in-situ ocean temperature and salinity measurements, Ocean Science, 9, 1–18, https://doi.org/https://doi.org/10.5194/os-9-1-2013, 2013.

22) Lines 154-155 – I am curious why you chose to assimilate a SST product with a coarser resolution in relation to our model grid. Could you, please, justify your choice and the impact of errors that are carried from the SST product?

There was a typo in the text and the correct product is the following product SST_MED_PHY_L3S_MY_010_042, at 1/20° resolution (that is still lower than our model resolution). The reference is corrected too:

- Pisano, A., Nardelli, B. B., Tronconi, C., and Santoleri, R. (2016). The new Mediterranean optimally interpolated pathfinder AVHRR SST Dataset (1982–2012). Remote Sensing of Environment, 176, 107-116. doi: https://doi.org/10.1016/j.rse.2016.01.019;
- Embury, O., Merchant, C.J., Good, S.A., Rayner, N.A., Høyer, J.L., Atkinson, C., Block, T., Alerskans, E., Pearson, K.J., Worsfold, M., McCarroll, N., Donlon, C., (2024). Satellite-based time-series of sea-surface temperature since 1980 for climate applications. Sci Data 11, 326. doi: https://doi.org/10.1038/s41597-024-03147-w.

We chose this product because, mostly focussing on the CC dynamics, we assumed that the 1/20° product was sufficiently resolved to constrain the transport across the channel. Furthermore, the use of a 1/100° product (the other available at a resolution higher than the model grid) would have required the super-observation procedure to reduce the resolution to 1/36° anyway. We did not fully understand what the reviewer meant with "the impact of errors that are carried from the SST product". Observation errors are taken into account with the **R** matrix.

23) Lines 160-161 – Is the bias correction for the ADT comparable with the Reanalysis used for boundary conditions?

The order of magnitude of the bias correction is comparable to the order of magnitude of the variability of the boundary condition water level from the Reanalysis.

24) Lines 164-169 – Is the background QC applied to all the observations, or only the ADT? If only the ADT, why?

Background QC was applied to all observation types. We used the same value for the beta parameter. Lines 196-200 updated manuscript.

Section 2.3:

25) Lines 189-191 – It could be helpful to show the transect position in a map. Figure 1 is a good place to add this.

We added the transect in Figure 1.

26) Also, could you provide the rationale for choosing this particular section?

The section corresponds to the Corsica Channel section employed in a previous study (Sciascia et al., 2019) and also to the location of the mooring system CoCM and dedicated cruise aimed at monitoring the dynamics of the corsica channel repeated CTD casts.

Section 3.1

27) Lines 199-204 – Since you use the ratio between the eigenvalues as a guiding metric to identify overfitting, it would be better to show it in Figure 2.

We modified Figure 2b as suggested.

28) Line 205 – This is the first occurrence of "FR". Please, add the definition.

We modified the sentence at line 91 as follows: "The nonlinear model without assimilation (freerun, FR) was run from 2019 to 2022, after…"
Line 116 updated manuscript.

29) Line 217 – These should be positive values (a negative reduction would mean the simulations are going worse).

We modified the text removing the minus sign. Line 251 updated manuscript.

30) Line 225 – Is the difference in the fit of u and v related to the variability of the velocity components? It would be helpful to show the observation standard deviations next to the RMSEs.

We believe that the difference in fit of u and v is principally due to the smaller magnitude of the u component (eastward) with respect to the v component (northward), that is also related to their variability. We think adding another source of information in the subfigures would produce too crowded plots. However, we mention the possible explanation for the difference in the fit between u and v in the discussion subsection 4.1.

"If we focus on the physical processes affecting the performance of the DA system we can try to explain, for example, why the error reduction is more pronounced for the v than the u velocity component observed by the CoCM (Figure 4a, b, e and Table 1). Since the latter has a lower observed average magnitude (0.008 m/s) and standard deviation (0.022 m/s) with respect to the former (0.05 m/s and 0.09 m/s, respectively), the DA has more chances to correct v than u, also because we made the assumption that the background error covariance is proportional to the standard deviation of the state variables. being the flow orographically constrained, the eastward velocity (u) cannot reach large values and possible small variations are more hardly caught by the model."
Lines 398-404 updated manuscript.

31) Lines 237-241 – Please, present the standard deviations for all observations to support your affirmation. This could be added to Table 1.

Done, updated Table 1.

32) Lines 251-256 – All that the time series show is data was in fact assimilated.

We agree with the reviewer and we further speculated in point 36 below.

Session 3.2

33) There is a lot of jumping back and forth with the figures, which harms the readability of the paper. Please, consider rearranging your discussion.

The reviewer is right and we rearranged the Results and Discussion section by splitting it into two separate sections: 3 Results and 4 Discussion. They are further divided into 3.1 Performance of the DA system, and 3.2 Observation impact. The Discussion is composed of 4.1 Changes in circulation and 4.2 Insights into DA mechanisms.

34) Lines 260-262 – I disagree that that graphs show that there is no BIAS. A better way to show this is by plotting the bias in Figure 3. Moreover, the graph for in situ S does not look "evenly distributed" for innovations.

We understand the point raised by the reviewer. However, we stated there is not a "significant bias", not that the bias is absent. As mentioned above, we think that adding the bias or other quantities to the subplots of Figures 3 and 4 would make them too crowded. We reformulated the text as follows, also considering that for salinity the reviewer is right.

"Overall, the points in Figure 7 are approximately evenly distributed between positive and negative innovations for all observation types, except for in situ salinity (Figure 7f). This can be viewed as an indication that a clear and evident bias is not present in these observed components of the state vector, but for the in situ salinity for which a non symmetric distribution for positive and negative innovation values is present (44.4% negative **d**; 55.7% positive **d**)."
Lines 309-312 updated manuscript.

35) Lines 273-276 – Please explain how can the impacts on transport shown in Figure 7 be many times higher than the values and variability shown in Figure 8.

The top left of each subplot of Figure 7 reports the scale value by which the transport value has to be multiplied to get the actual values.

36) Lines 276-281 – So, the observational studies consider the velocity constant with longitude? And how do you explain the difference to the other model studies? It is not good enough to say additional analysis could be done. Please, explain this better.

Yes, in general observational studies consider the northward velocity constant with longitude because data are available only at the vertical line of the mooring. The point is that the FR is in line with other model-based studies, whereas the discrepancies come out after the assimilation is performed. We believe that such a difference can be ascribed to the assimilation procedure and until we perform DA over other years, it is difficult to understand if 2022 was a particular year of low transport or not.
We modified the text as follows:
"Recalling that the yearly averaged transport values that we obtained at the CC for FR, FC and AN are equal to 0.49, 0.31 and 0.28 Sv, respectively, these values are lower than previous estimates based on observations, ranging from 0.54 Sv to 0.71 Sv (Astraldi and Gasparini, 1992; Astraldi et al., 1994), and numerical models, such as 0.49 Sv in Sciascia et al. (2019) and 0.5 Sv in Béranger et al. (2005), the latter based on a multi-decadal time series. However, reanalysis by de la Vara et al. (2019) and Barral et al. (2024) obtained an average transport around 0.33-0.34 Sv and 0.35 ±0.365 Sv, respectively, considering the whole transect including the CC and the area from Capraia Island to the Italian coast.

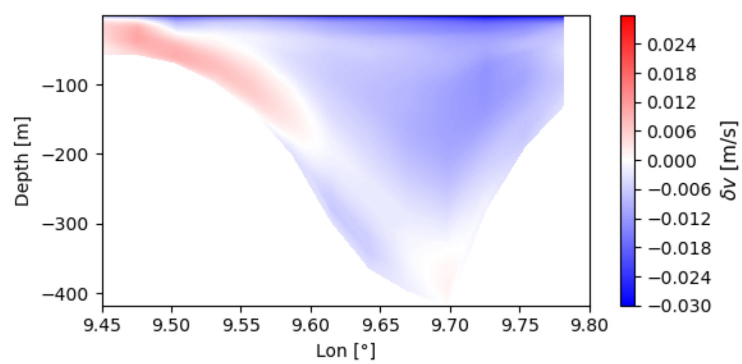
Part of the discrepancy with observational estimates may stem from the assumption of negligible longitudinal variability in the northward velocity across the channel that is not fully supported by our model results (Figure 5e and Figure 8d, e) and was also observed by previous numerical studies (Sciascia et al., 2019). Interestingly, the two reanalyses of de La Vara et al. (2019) and Barral et al. (2024) obtained an annual average transport comparable to our result by considering the whole Tyrrhenian transect. These values lower than those from previous literature can be ascribed to a possible negative annual averaged transport for the area between the Island of Capraia and the Italian Coast, or to a more appropriate representation of the flow in the area.
The transport across the whole transect linking the Tyrrhenian and Ligurian Seas, and the interaction between the two sub-transects, also in light of basin-scale mass budgets, deserve further studies and analysis. Furthermore, additional analysis, particularly over a longer time period, will help determine whether the year 2022 reflects inter-annual variability (Vignudelli et al., 1999), or is the signal of a longer-term trend."
Lines 338-349 updated manuscript.

37) Lines 282-287 – What explains this variability and the differences in the assimilation model?
We did not fully understand the point raised by the reviewer. The variability between winter and summer season is a characteristic of the CC transport. We believe that the difference between DA-informed simulations and freerun, especially during the summer season, is mainly due to the mismatch between observations from the CoCM and the freerun during such a period.

38) Lines 304-308 – Please, explain why the impact per observation is bigger in situ T and S in relation to HFR.
We believe that in situ T and S data have a larger impact per observation since they directly affect the whole water column, whereas HFR data influence the surface layer and can have only an indirect effect on the water column.
We added the following:
"This can be explained by the fact that in situ data directly affect the whole water column, whereas surface data can only influence it indirectly, through the adjoint."
Lines 332-333 updated manuscript.

39) Lines 309-313 – This calls for a look at the vertical structure (transect) of the increments. You could link to the discussion of Figure 9.
We reorganized the paragraphs: first we talk about the overall reduction in transport in Figure 5 (Figure 6 in the original manuscript), then we analyze the different trends of the increment based on the period of the year (Figure 6; Figure 8 in the original manuscript) and analyze their structures (Figure 8; Figure 9 of original manuscript). Finally, we link the comments to Figure 8a to the yearly-averaged velocity distribution of the increment δv.
Now the paragraphs read:
"Comparing the non-assimilative (FR) and the assimilative (FC and AN) runs it is important to stress that the latter are the cumulative result of corrections applied to the background by DA over each assimilation window. Observing Figure 5, it is clear that DA systematically reduces the northward velocity in the channel to better match the observations, leading to a corresponding reduction in total transport (Figure 6a). However, Figure 6b, shows that northward transport reduction is dominant up to mid-August 2022, after which positive transport

increments become more frequent than negative ones. This evolving pattern is further dissected in Figure 6c: from January to early April, CoCM data primarily drive the transport corrections; between April and early August, HFR data become more influential, despite not being directly located at the CC transect, where the transport is computed. Figures 8a and c show the average velocity increment δv during the periods dominated by the JERICO facilities, CoCM (from 01 January to 09 April), and HFR data (from 22 April to 11 August), respectively. CoCM observations tightly constrain the flow near their location but with the side effect of accelerating the northward flow in the surface western part of the transect. In fact, the δv values, averaged over the whole year, are characterized by an overall reduction for the most part of the CC transect and a slight increase at the upper western flank (not shown). This may be the result of insufficient corrections to the boundary conditions, redirecting the volume transport to unconstrained portions of the domain (Figure 8a, Figure 5e). Interestingly, it can help to explain why the total transport (Figure 6a) is less affected than the velocity alone (Figure 5a, b, c) when the FR and the AN runs are compared. Figures 5d and e show that while the northward flow weakens across most of the transect in AN with respect to FR, particularly on the eastern side (that observed by the mooring), a partial compensation occurs through a weaker (in absolute value) southward flow on the western flank and the already mentioned slightly northward acceleration at the upper western flank."
Lines 354-371 updated manuscript.
Here, we report the yearly averaged value of δv.



40) Lines 339-347 – You kind of hint at the potential issue here: The placement of your southern boundary. Given the importance, please expand your discussion.
We added the following:
"Additional analyses might expand the computational domain to include an additional portion of the domain which potentially influences the transport through the CC. However, it is not straightforward to identify the extension which maximizes the impact of initial condition and atmospheric forcing at the expense of boundary condition. Furthermore, extending the domain to contain the whole Western Mediterranean basin would excessively increase computational costs."
Lines 393-397 updated manuscript.
See also the discussion at point 67 from Reviewer 1.

41) Line 348 – Here is a jump in your discussion. You should add some explanation and refer to session 2.3.

After the rearrangement of the Results and Discussion section, the statement to which the reviewer is referring is now moved at the beginning of subsection 4.2 Insight into DA mechanisms and it reads:

“Observation impact can allow us to investigate in which ways DA modifies the model state with respect to a particular metric (transport across a section in our case) by optimally balancing between observations and background solution.”

Lines 431-433 updated manuscript.

We also added the following:

“Developing more the analysis, we can estimate the contribution of each observations to the transport increment through $\Delta Tr_{TL} = \mathbf{d}^{T} \cdot \mathbf{g}$. The presence of a seasonality in ΔTr when we consider observation typology (Figure 6c), that is roughly cold months controlled by CoCM while warm months until late autumn by HFR, could be explained by the corresponding seasonality in the CC transport. Small northward velocity values in warm months tend to produce small innovations d and, consequently, possible small $\mathbf{d}^{T} \cdot \mathbf{g}$. To better control the dynamic at the channel, DA relies on another source of observations that is HFR. In cold months, the opposite occurs and the role of CoCM observations is predominant.

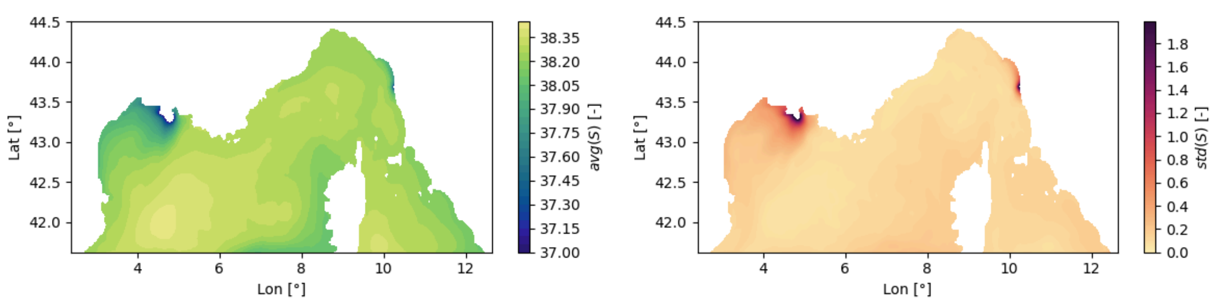
Moreover, the variability of the elements of **g** reveals the location of observations which have the greatest influence on the model. Figure 10 shows the spatial distribution of the root mean squared values of the elements of g (RMSg) calculated over each computational cell for each observation type.”

Lines 439-447 updated manuscript.

42) Lines 352-355 – This again shows the weight given to your southern boundary, on top of potential issues on the way the river flux is added to your simulation. How do the sea surface salinity maps look?

Salinity surface maps show plumes at the mouth of Rhone and Arno rivers.

Here the yearly averaged surface salinity and standard deviation maps for the Analysis run are reported.



Please, consider also the response to point 72 from Reviewer 1.

43) Are the river plumes well represented?

From the qualitative point of view, by graphically inspecting the modelled sea surface salinity, the plumes are well represented. However, we did not specifically calculate the model skills with respect to observations close to the mouths, as this was not the primary objective of the work.

Instead, we focused on the whole behavior of the salinity field, whose representation is improved by the assimilation procedure compared to the FR.

44) Lines 380-395 – This is a very interesting analysis! As the heat flux reflects only what is happening at the surface, it would be good to see what is happening below. I believe the water column heat content or vertically integrated density would be helpful here.

We thank the reviewer for this request since it helped us to notice that our interpretation of the above-mentioned process had some flaws. ROMS is a Boussinesq model, and for the Boussinesq approximation the density variations are neglected in the momentum equations, except in the buoyancy term along the vertical coordinate, and the continuity equation reduces to conservation of volume. As a consequence, steric expansion cannot be directly modelled.

For these reasons, we reformulated the whole paragraph trying to interpret the results in the light of the above-mentioned aspects.

We modified the lines 380-403 as follows (we also modified Figures 11 and 12):

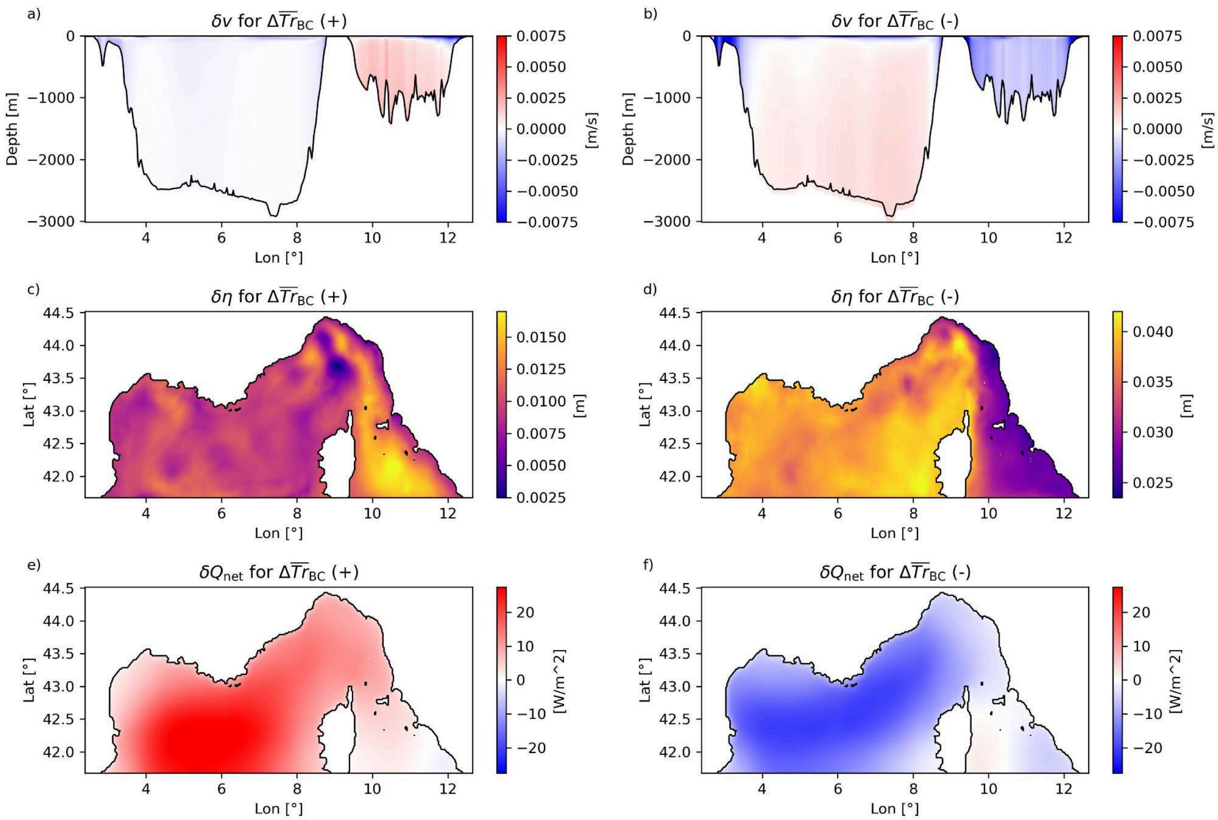
"For most assimilation windows the primary way DA constrains the CC transport is the modification of the boundary conditions (Figure 6d). By selecting those with the largest positive and negative transport increments largely attributable to BC modifications (90-th and 10-th percentiles of the ΔTr_{BC} distribution, respectively), we calculated the northward velocity increment at the southern boundary δv (Figure 11a and b), the free surface height increment $\delta\eta$ (Figure 11c and d), and net heat flux increment δQ_{net} (Figure 11e and f), between analysis and background, averaged over the two sets of assimilation windows. A positive ΔTr_{BC} is indeed associated with a northward-inducing $\delta\eta$ (Figure 11c) that is consistent with a clear northward barotropic acceleration on the Tyrrhenian side and a less intense southward barotropic acceleration, distributed over a much wider cross sectional area, at the Ligurian-Provençal basin side (Figure 11a). For the negative ΔTr_{BC} such a mechanism is more marked for both δv and $\delta\eta$. The barotropic nature of these increments is confirmed by the almost uniform increment velocity distribution along depth (Figure 11a and b).

Interestingly, the δQ_{net} is characterized by a pronounced surface heating/cooling in the Ligurian-Provençal basin when the CC transport increases/decreases (Figure 11e and f). Since the ROMS model adopts the Boussinesq approximation, such a mechanism cannot be related to steric adjustments, but rather to a modification to the baroclinic pressure field to maintain the consistency between the barotropic correction at the boundaries and observations in the interior. Additional analysis is required to make explanatory hypotheses at the base of the observed mechanism.

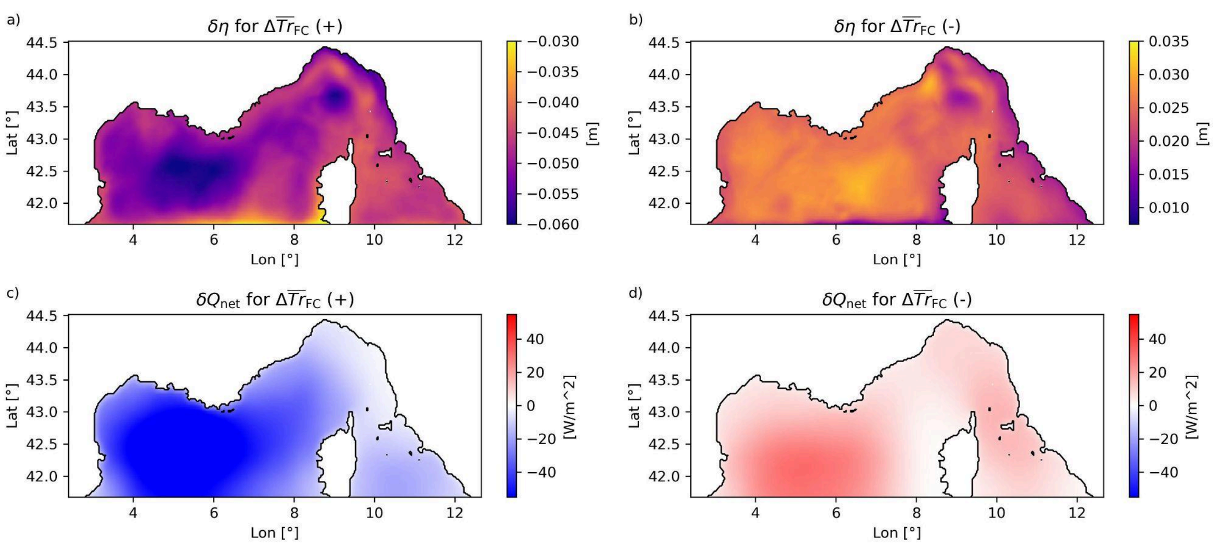
Astraldi and Gasparini (1992) proposed that CC dynamics are governed by atmospheric conditions over the western part of the Ligurian-Provençal basin, where winter heat loss enhances the steric gradient between the Tyrrhenian and Ligurian-Provençal basins, driving Tyrrhenian waters into the Ligurian Sea and strengthening the ECC. Our results cannot directly mimic the processes described by Astraldi and Gasparini (1992), however, focusing now on the largest positive and negative transport increments attributable solely to the correction to atmospheric forcing, ΔTr_{FC} (95-th and 5-th percentiles of the ΔTr_{AF} distribution, respectively), we observe that $\delta\eta$ (Figure 12a and b) and δQ_{net} (Figure 12c and d) align with the mechanism proposed by Astraldi and Gasparini (1992): an increase (decrease) in transport is triggered by a differential surface cooling (warming) with associated eastward (westward) positive gradient in the free surface increment. Even in this case, additional analyses are required to delve into the mechanism responsible for the agreement with literature results.”

Lines 484-507 updated manuscript.

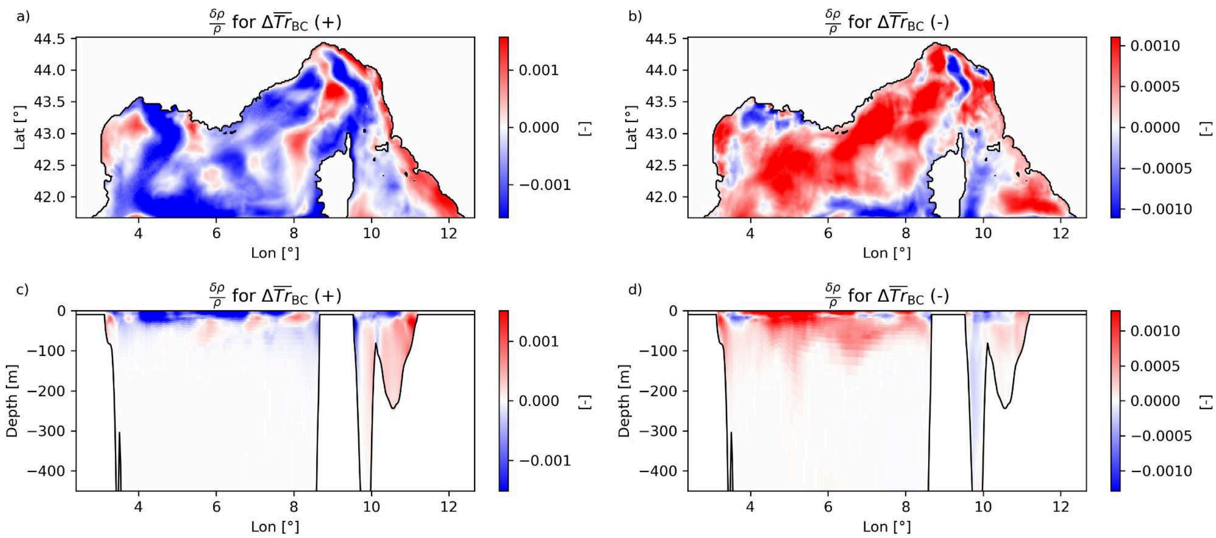
Here, we also report the updated Figure 11



And updated Figure 12



As requested by the Reviewer, we report below the relative increment in density δρ/ρ associated to the analysis reported in Figure 12, averaged over the upper 50 meters and along a longitudinal transect at 42.5° Latitude. It is clear that the cooling/heating produces increase/decrease in density mainly at the surface. However, this is not associated with a shrinking/expansion of the related sigma layers. For this reason we reported in the above-mentioned modified paragraph: "such a mechanism cannot be related to steric adjustments, but rather to a modification to the baroclinic pressure field to maintain the consistency between the barotropic correction at the boundaries and observations in the interior."



Session 4

45) I feel there is a missed opportunity here to discuss how effective the different observations are in constraining the simulation and the potential implications for forecasting and the "greater" western Mediterranean Sea – the usual goal of an assimilative system. You have lots of information that can be helpful in driving the evolution of the observing system.
A closing paragraph would do the job.

In the conclusion we added the following paragraph.

"This work represents an additional step toward an operational 4D-Var data assimilation forecasting system for the Northwestern Mediterranean sea, given the significant effect the employed observations have in constraining the circulation of the area for both the analysis and the forecast stages."

Lines 538-540 updated manuscript.

Tables

46) Table 1 – I am confused by your delta signals. While for RMSE a negative value represents improvement, the opposite seems to be truth for the correlation. I would rather see the actual values for the RMSE, correlation, and BIAS (instead of deltas). This would better make the point if your simulation is actually fittng well to the obs. And, please, be consistent with the +- signals.

We understand the point of the reviewer but we try to better explain the reasons for our setting.

In general in the Data Assimilation community, a reduction in error is an improvement (so the negative values mean improvement), whereas an increment in correlation is an improvement as well (so the positive values mean improvement). We are saying that if you reduce the error or increase the correlation, you have a good result. For bias, we want it to be reduced in absolute value, so the negative values mean the model is improved.

The absolute values for the RMSE are already reported in the Figures and we would like to be more informative by explicitly showing how much the assimilation procedure can improve the simulations.

47) Table 2 – Please, improve your caption to clearly indicate what you mean by “Global” and “Datum”.

We modified the caption of Table 2 as follows:

“Root mean square impact for the different observation typologies, both globally (Global refers to the effect on transport of all observations belonging to a certain typology), and considering the average contribution of a single observation (Datum refers to the average effect on transport of a single observation belonging to a certain typology). The last column reports the total number of observations per type.”

Figures

All the modified figures requested by the Reviewer are reported at the end of the present document indicating both the number they have in the draft and the number they have in the updated version of the manuscript (round brackets).

48) Figure 1 - a) The “HFR” area is not clear. This can be presented by a polygon showing the coverage area. Also, what you present seem to be presenting is the mean stream function - this should be clear in your caption.

We added a contour line to the HFR area and we removed the streamlines since information about the yearly averaged flow field is reported in Figure 9. In this way the figure is more readable.

49) Here would be a great place to show the section chosen for the transport calculation used in your observation impact calculation.

We added the section in the subplot a).

50) b) I feel the total observation density to not be very informative, and would be better plotted as a "pcolor" or "contourf". Instead, the distribution of each observation type can give a better idea of the coverage and expected impacts.

Subplot b) (c in the update version of the manuscript) has been produced as a pcolor coloring each cell of the model domain based on the amount of observations within it. The information about the distribution of each observation typology is partially contained in Figure 10.

51) c) Why do you have an empty depth bin close to 2000m?

Because accidentally no data are available for that depth interval.

52) Figure 2 – Please, show the ratio between eigenvalues in sublot (b).

We modified Figure 2 as requested.

53) Figure 3 – Could you, please, use the same axis ranges for the number of observations in all your subplots? This will make it easier to compare.

We understand the request of the reviewer, but we think it would lead to an excessively flattened representation for in situ data with respect to HFR and SST data.

54) Figure 4 – As for figure 3, please use the same axis limits for the number of observations.

Please, see the previous response for point 53 above.

55) It would also be interesting to have the std of the (u,v) observations to be able to evaluate how much the variation of the fit by depth is relative to the variability of the currents.

As already mentioned in a previous comment, we prefer to avoid putting the std of the data in the plot and we added such information in Table 1. We discussed the effect of current variability in model skill at the point 30 above.

56) Figure 5 – The reference to the subplots in your label is wrong. Always add the unit after the depth.

We corrected the references in the caption ('c' instead of 'd', and 'd' instead of 'e'). We added the depth unit both in the caption and in the text in the subplots. (This is Figure 9 in the updated manuscript).

57) Figure 6- Please, add the units for depth.

Done, both in the caption and in the text within the subplots. (This is Figure 5 in the updated manuscript).

58) Figure 7 – Please, have use the same axis and colorbar for all your subplots.

Using the same colorbar range and axis would lead to a non-readable figure. The x-axis can have values in the range -0.25/0.25 or -2.5/2.5; y-axis are multiplied by 10^{-4} or 10^{-3} and the colorbars have maximum values which differ by almost 2 degrees of magnitude. We instead changed the colormap to increase the readability.

59) This figure is underexplored in the text, which makes me question if it is necessary.

The use of this type of figure is a common practice in data assimilation papers dealing with observation impact (report references) and it gives you information about the correctness of the procedure. We refer to this in the text at lines 304-322 (updated manuscript).

60) Figure 8 – b) This is unnecessary. You don't seem to explore this figure enough in your text.
Subplot b, by showing the equivalence between the transport increment calculated from the nonlinear model and that obtained by the observation impact procedure, ensures the tangent linear assumption holds for the adopted framework (duration of assimilation window, typology if metric employed). We are reluctant to the proposed change as we think it is important to provide this information.

61) Subplots c) and d) arguably contain the most important results in your manuscript, and should be deeper explored in your text. Try to keep in mind what is the main point you want to make with your paper.
We agree with the reviewer about the importance of subplots c and d.
After the rearrangement of the structure of the paper, the comments associated with these two subplots gained a larger exposure and are not hidden by the surrounding text.

62) Figure 9 – Please, add your section position to the maps.
The section has been added to the plots in green. (This is Figure 8 in the updated manuscript).

63) Figure 10 – Please, use the same limits for your map colorbars.
The use of the same limits for all the colorbars would produce a loss in information content since the different colorbars span two orders of magnitude.

64) The colormap you used is not helpful either. More contrast between values would make it easier to spot differences.
We agree with the reviewer and the colormap has been changed.

Updated Figures list

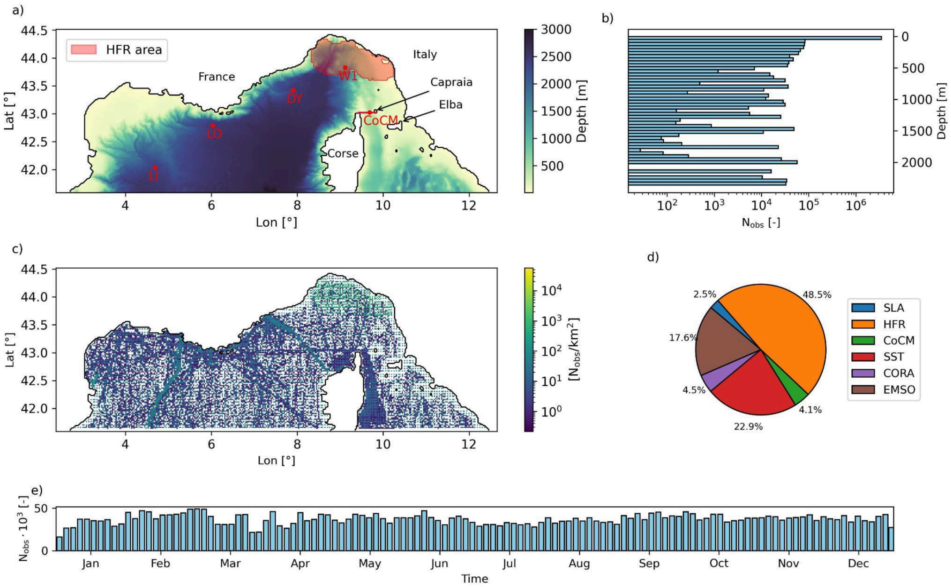


Figure 1 (Figure 1)

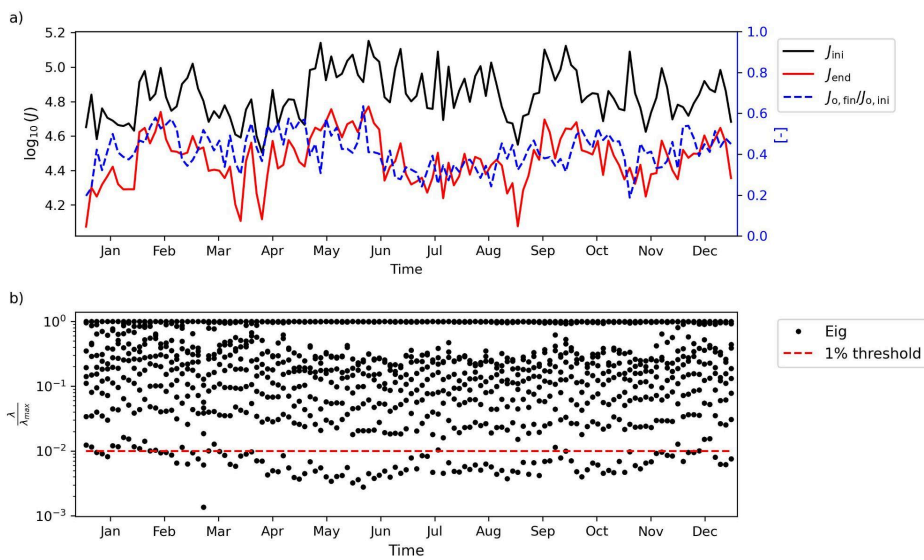


Figure 2 (Figure 2)

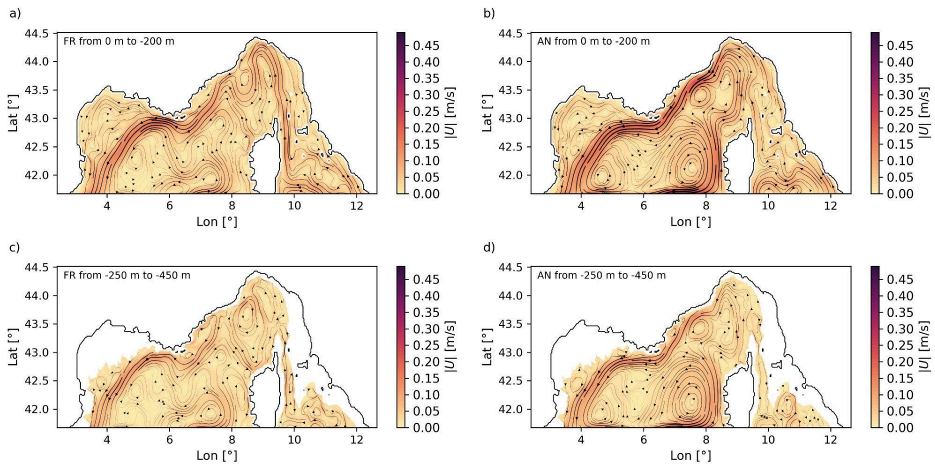


Figure 5 (Figure 9)

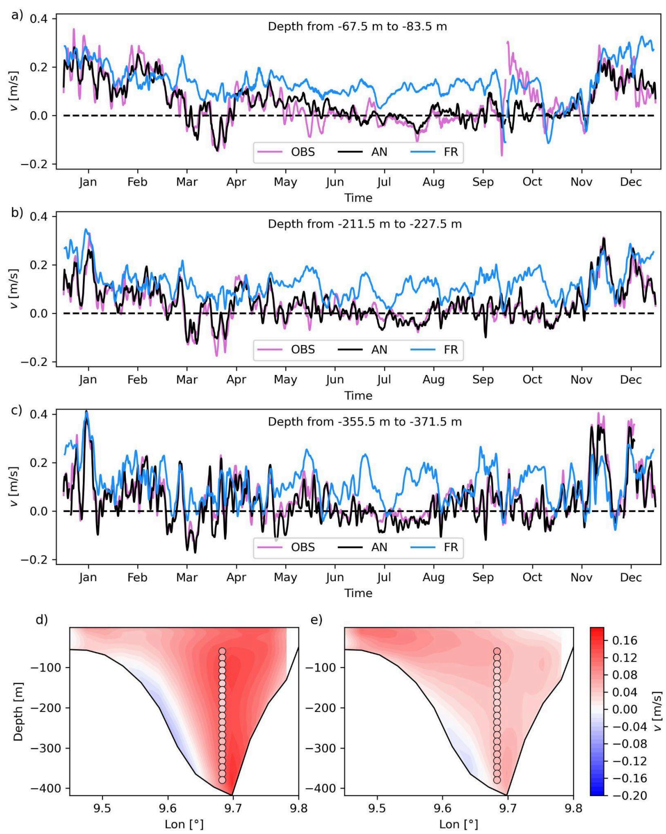


Figure 6 (Figure 5)

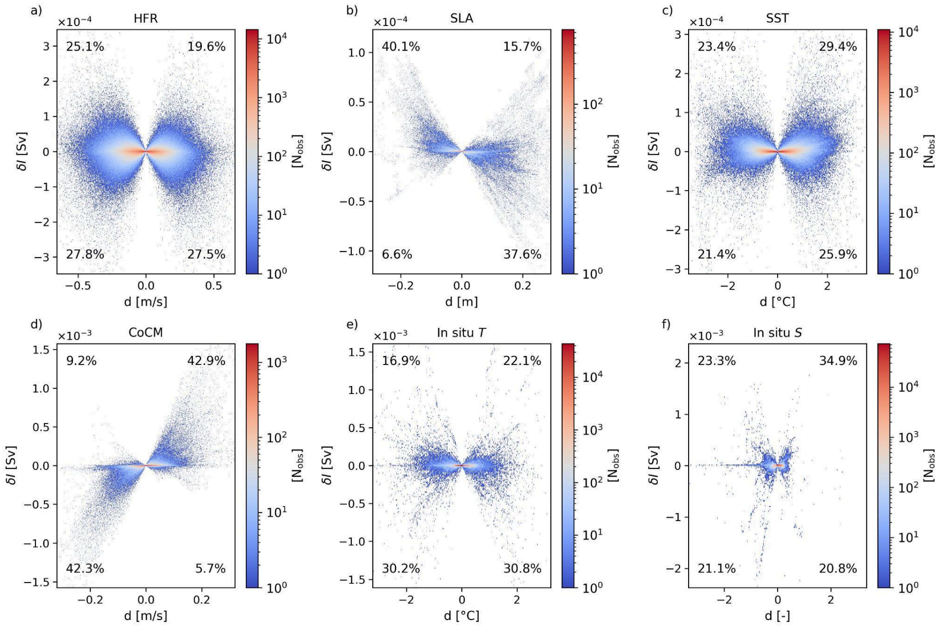


Figure 7 (Figure 7)

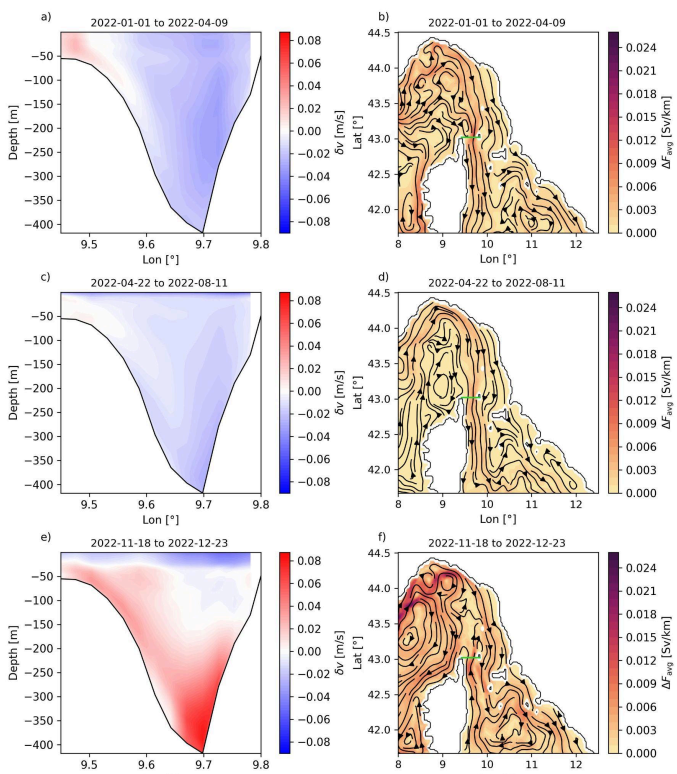


Figure 9 (Figure 8)

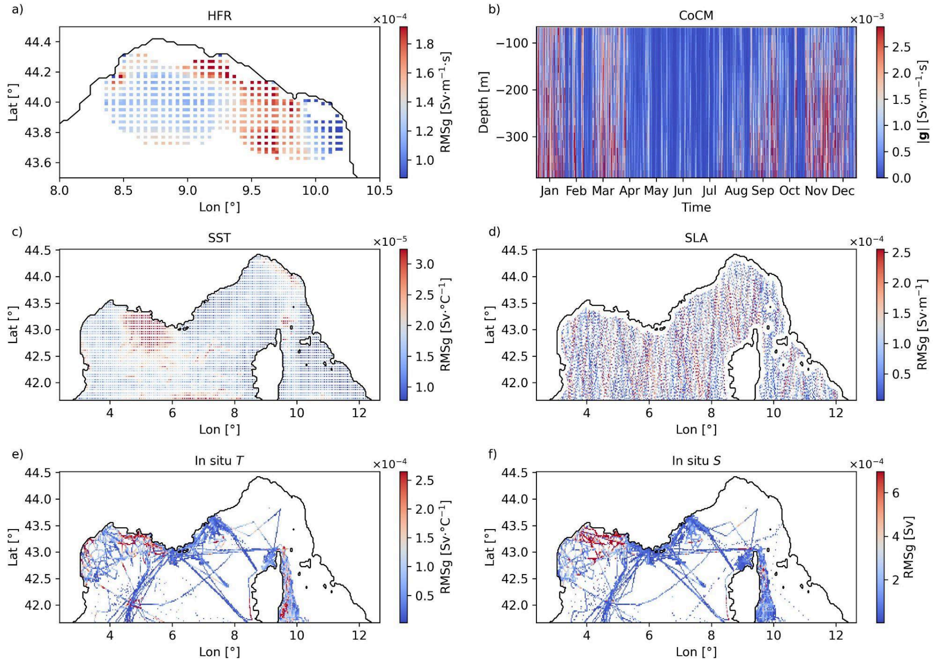


Figure 10 (Figure 10)

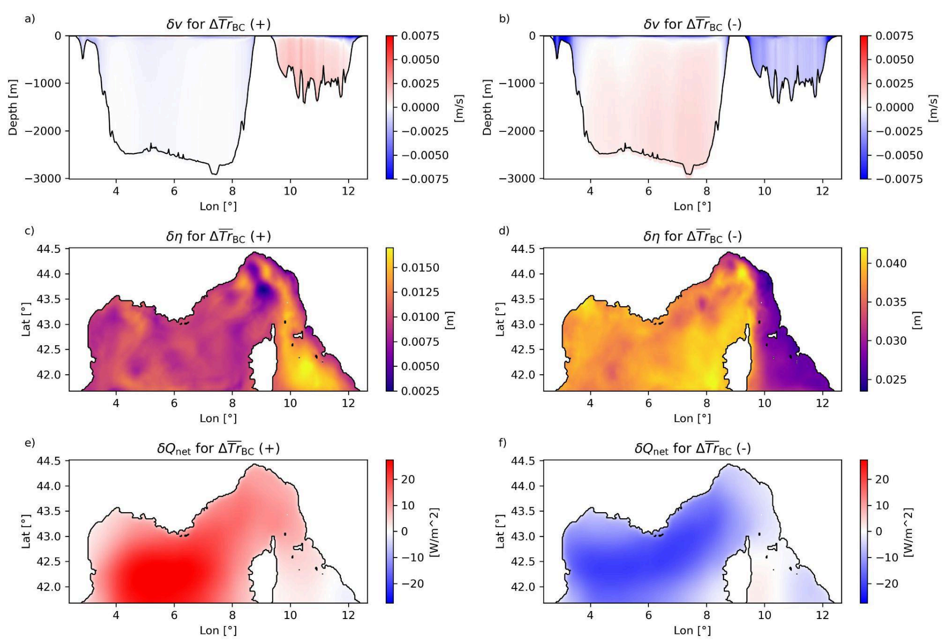


Figure 11 (Figure 11)

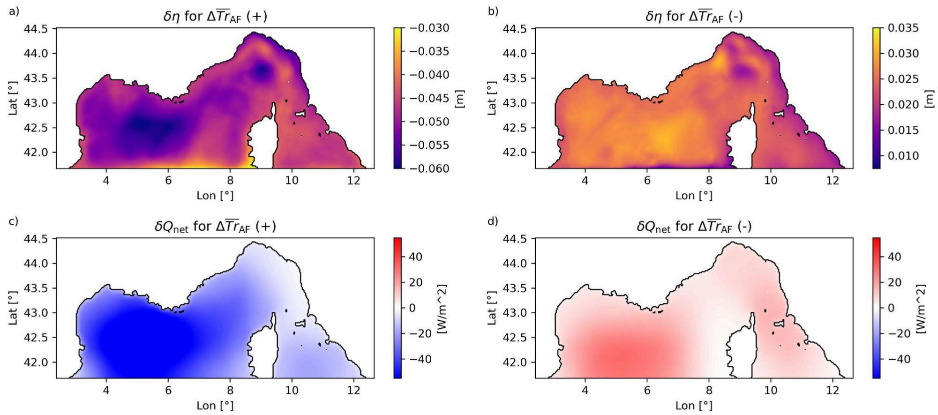


Figure 12 (Figure 12)