ANSWER TO REVIEWS

Interactions between the Somali Current eddies during the summer monsoon: insights from a numerical study

by

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The reviewers comments are reproduced in blue and our answers are written in black and the changes added to the manuscript are given in "*italic*".

I Answer to Reviewer # 1

This study examines the interactions between the three major anticyclonic structures of the western Arabian Sea – Southern Gyre (SG), Great Whirl (GW) and Socotra Eddy (SE) – and their accompanying turbulence during the Southwest Monsoon. The use of 3 primitive equation numerical simulations run for decadal periods allows to identify different scenarii of interactions. The topic of investigation is of interest since there is no regional study focusing specifically on these interactions and the Arabian Sea regional dynamics is far to be fully understood. Specifically, although it has been evidenced that some mesoscale structures are permanent at seasonal scales (noticeably, SG, GW and SE), their robustness and variability at interannual scales is still debated and the use of long simulations is of particular interest in this perspective. However, even if the objectives and outline of the paper follow an interesting and logical path, the depiction of different scenarii is too descriptive and deserves further dynamical diagnostics. For this reason, I recommend major revisions before publication. In section 1, I explain my point of view on possible weaknesses of the study and point out some suggestions for improving the science. In section 2, I list some issues that may deserve some rectifications. In section 3, I correct some typos and discuss some improvements for the figures.

Preamble #1 clarifying the modelling strategy used in this study

Numerical model studies of the Arabian Sea found in the literature are all based on regional models of limited area. The solutions they proposed are not exempt of the influence of the open boundaries. At low latitudes, such influence can have several consequences, such as the contamination of the locally forced circulation by artificially boundary generated waves, or such as missing or poorly represented remote forcing (because generated outside the model domain).

For this reason, we based our investigation on the solution of global models. Although we haven't yet demonstrated it, we suspect the dynamics of the Southern Gyre to be very much influenced by the Indonesian through-flow, the wave dynamics of the easternmost part of the Indian Ocean and the whole south Indian Ocean subtropical gyre, such that it would be very difficult to realistically simulate the SG characteristics with a model of limited area. *An original element of our study is that it is a regional study performed with a global model*.

However it is not simple to study the regional dynamics using eddy resolving or eddy-permitting global models as these global models are very demanding in terms of computational resources which drastically limits the number of sensitivity experiments that can be done.

For this reason, we used model simulations made available to us by the Drakkar consortium (http://www.drakkar-ocean.eu/) because these simulations have been shown to be quite realistic in their representation of the ocean circulation in recent publications (as listed in section 2). *But the simulations we used were not designed to be sensitivity studies of the Arabian Sea* (as were the regional simulations of Vic et al., 2014 for example). The numerical and forcing choices made for these simulations were driven by other and more global objectives. In addition, *because we know that the solutions proposed by present state of the art eddy-resolving OGCMs still have some dependency on parameter choices and also because these models produce turbulent flows that have a chaotic behavior, we decided not to use a single simulation but several of them, and to focus on the part of the solution that is robust through all simulations.* This strategy was used in the Dynamo model inter-comparison experiment (Willebrand et al., 2001, Böning et al., 2001, Barnier et al., 2001). Of course it would have been much better to have available a large ensemble of simulations that would permit a probabilistic approach, but such simulations have not yet been done.

Preamble #2 clarifying the too "descriptive aspect" of the proposed scenarii

We acknowledge that the paper is rather descriptive and we claim the right to publish a descriptive paper. What is wrong with it as long as the material which is described is correct and original? Note however that we consider that some of the diagnostics used here (spiciness on isopycnal surfaces and vertical sections grouping current speed with relative vorticity and isopycnal depth) are already quite elaborated diagnostics.

The way the paper is written corresponds to the primary objective of this paper which is to *describe* the phenomenology of these "fast*" eddy interactions because this has not been done yet, and because such description is a necessary step before one can begin to investigate the dynamics of these interactions.

*By "fast" we mean phenomena that are too "fast" to be properly described by conventional in-situ observations (including Argo array) or by satellite observations (the orbital revolution of altimetry satellite is not fast enough to continuously map these eddy motions and interactions).

We agree that additional diagnostics aiming at providing a better dynamical understanding of the processes at work are of interest. The reviewer suggestion to use relevant statistical diagnostics that would better “synthesize” what is seen in the series of snapshot is certainly a good suggestion.

We developed our analysis in this direction and have added a section that presents an analysis of the seasonal evolution of the PDF of spiciness in the area of the Great Whirl (section 4.4 with 3 new figures in the revised paper).

Our response to the review and the changes made to the revised version of the paper are guided by the above preambles.

1 General Comments

1. The use of the 3 simulations is a bit wobbly since you don’t clearly evidence their relative importance and how they are supposed to impact the dynamics.
We clarified the reasons for this 3 simulations strategy in the "preamble #1" of this response. To answer more specifically the comment of the reviewer, the strategy to use 3 simulations that were not specifically designed to be sensitivity studies can certainly be qualified as "a bit wobbly", but it is certainly better than using a single simulation. Because the focus is on highly non-linear turbulent processes, many occurrences of these processes are necessary to assess their significance. This is why we have chosen 3 simulations, each providing 10 years of data, such that in total 30 realizations of the SG/GW annual interaction cycle are available, from which scenarii can be drawn. Nevertheless, the reviewer remark indicates that additional information regarding the specificity of the different simulations must be given in the light of the objectives of the study. To make our multi-simulation strategy to appear less “wobbly”, we explain it in more details and have modified the last paragraph of Section 2 as follows:

Change #1:
Is removed: *"The differences between the simulations are not used to investigate the model sensitivity but to provide some assessment on the robustness of our results to some changes in model configuration, the features described in the paper being robust or not through all the three simulations. The analysis of model results presented here is performed on the last 10years of every simulation using model outputs every 5 days."*

Replacement text is: *"These global simulations were not initially designed to be sensitivity studies of the Arabian Sea to various processes or parameters (as were the regional simulations of Vic et al., 2014 for example), and are not suited to be used for that purpose. The use of several simulations rather than a single one is motivated by the fact that the solutions provided by present state of the art eddy-resolving OGCMs still show some dependency on parameter choices and are subject to a chaotic behavior specific to turbulent flows (Sérazin et al., 2015). Identifying the parts of the solution that are robust through all simulations allows to building confidence in the results. A similar paradigm was used in the past for model inter-comparison studies (Willebrand et al., 2001, Böning et al., 2001, Barnier et al., 2001). This is why we have chosen 3 simulations, each providing 10 years of data, such that in total 30 realizations of the SG/GW annual interaction cycle are available, from which scenarii are drawn. The analysis of model results presented here is performed on the last 10 years of every simulation using model outputs every 5 days and focuses on features that are robust through all simulations."*

I suggest (i) not to use the 1/4° simulation, (ii) to investigate how the different physics of the models infer on the dynamics, and (iii) to compare the same 10 years (or more) simulated by the models, as interannual forcing variability in the region may be strong.

About (i), the 1/4° simulation has a resolution of $\delta x \sim 25$ km which does not allow to resolve mesoscales: deformation radius at the latitude of the Socotra Eddy is 70 km (Chelton et al. (1998)) thus you are in an eddy-permitting regime (at least 10 δx below the deformation radius are necessary to be eddy-resolving) that impedes the study of mesoscale dynamics.
We never pretended that the 1/4° model was "eddy-resolving". The 1/4° model configuration ORCA025 has been always described and used as "eddy-permitting" in the paper (e.g. Section 2). The use of ORCA025 in our analysis is limited (Fig. 1 and a panel of Fig. 3) the most relevant part of the discussion being given to ORCA12. We do not think that the solution of the 1/4° model is wrongly used in the paper and therefore keeping it or not is a question of usefulness and not a question of correctness.

Because ORCA025 model is a widely used model configuration, and because it is the ocean component of many high resolution Earth System Models in Europe (at ECMWF, UKMO, CERFACS, IPSL, CMCC), we consider that it is useful to report any assessment of its solution. Therefore, we find interesting to report that the "eddy-permitting" 1/4° solution behaves in a way that is qualitatively comparable to the "eddy-resolving" 1/12° solution in term of large scale circulation in this region (Fig. 1) and in its representation of the "great features" of the Somali Current Eddies (with a resolution of 25 km the model has more than 10 grid points in the GW and the SG, both these eddies having a diameter larger than 350 km).

Our decision is to keep the ORCA025 model in the paper. We verify however that no confusion could be made regarding its "eddy-permitting" nature. For this reason we added the following paragraph (Section 2):

Change #2:
Text added (in section 2): *"Although ORCA025 is only eddy-permitting, even at these low latitudes, it is widely used to perform ocean reanalyses (e.g. Balmaseda et al., 2015) and is the ocean component of several Earth System Models in Europe (at the UKMO for example, Megann et al. - 2014, Williams et al., 2015). Therefore, we find useful to report any assessment of its solution. As it is shown in this study, the*

"eddy-permitting" 1/4° solution behaves in a way that is qualitatively comparable to the "eddy-resolving" 1/12° solution in term of large scale circulation of the Indian Ocean and in its representation of the main features of the Somali Current Eddies."

About (ii), you justify the use of different simulations to convince the reader on the robustness of the scenarii. I would say that it will make the reader more curious on how the dynamics is supposed to be changed by the model physics and forcing than making him/her comfortable with the results. Particularly, you mention the "tearing off from the boundary current of intense patches of positive vorticity". This process may be impacted by the boundary layer structure, thus by the friction and the slip condition. Another potential source of differences between simulations is the wind stress dataset used to force them. For instance, Beal and Donohue (2013) suggest that the northern flank of the GW is aligned with the zero wind stress curl. I suggest that the authors build on those differences.

The more simulations we have and the more possibilities of studies we have. It is therefore important to focus: one cannot satisfy the curiosity of every reader on how the dynamics is supposed to be changed by the choice of parameters. Some readers will inevitably be frustrated.

We do not deny that the differences in model parameters between the simulations have some influence on the eddy-eddy interactions described here, but it is not the topic of the paper to investigate them.

As we already argued in Preamble #1, the present global simulations (which were not designed for sensitivity studies to various parameters) are used to increase the number of realizations of the "turbulent" processes we are describing (see Change #1 above). Since we have clarified our strategy, it would introduce more confusion to speculate on the effects of the parameters, especially since the available simulations are not designed for that.

Nevertheless, we expanded the discussion on lateral friction to clarify again our strategy.

Change #3 (section2):

The differences in lateral friction (free-slip versus partial slip boundary conditions) may have an impact on the mean profile of the boundary current, and consequently impact its stability. However, there are too many differences between the free slip and the partial slip experiments (differences in spin-up time, forcing, and period of integration, see Table 1) to assess in a significant manner the impact of the friction parameter (or the wind forcing) on the eddy-eddy interactions described here.

About (iii), Beal et al. (2013) show that planetary waves generated in the Arabian Sea drive variability and feedback on the monsoon at interannual timescales. As such, it is worth comparing a same decade (or more than 10 years if available, that would better serve statistics) simulated by the models.

This is still the same issue regarding the strategy of using 3 different simulations. We do not have simulations covering the same decade. We would have done a different paper in we had such simulations. As said before, we adapted our strategy analysis to the available modelling data sets, which have only 4 years in common, a much too short period to draw any conclusion. This 'limitation' guided our strategy to use the 3 different simulations to identify robust behaviors.

Regarding the specific comment of the reviewer, it is quite possible that interannual variations within a given scenario can be influenced by Rossby Waves. It is also possible (even likely) that the selection of one scenario with regard to another in a given year may also be decided or influenced by Rossby Waves or interannual variations in the monsoon winds. These are issues raised by our study, but cannot be addressed with the simulations made available to us at the time of the study. This will have to be investigated with longer hindcast simulations when available (in another paper). To account for the reviewer comment, we have included a short discussion in the conclusion.

Change #4 (section Conclusion):

It is worth mentioning that within a single simulation, the scenarios described above show a significant year to year variability. It is quite possible that interannual variations within a given scenario be influenced by the Rossby waves generated in the Arabian Sea. It is also possible that the occurrence of one scenario with regard *to another in a given year be triggered by Rossby Waves. These are new issues raised by our study that could not be addressed with the simulations made available to us.*

2. All scenarii are only based on sequences of snapshot maps and one section. This is pretty weak and lacks quantitative and more integrated diagnostics. Namely, quantifying eddy drift (you mention a SG drift of 1 m/s) requires an accurate eddy tracking (e.g., Morrow et al. (2004); Chelton et al. (2011)). For instance, monitoring eddy statistics (vorticity, size, position, . . . , as done for the GW in Vic et al. (2014) as a function of time would bring quantitative aspects to go further than qualitative descriptions. Probability density functions (PDFs) of the eddy characteristics would allow to classify more precisely scenarii (e.g., PDF of spice in an eddy through years would indicate if merging occurs or not). I strongly encourage the authors to investigate statistically the eddy life cycles through eddy-tracking and statistics.

On the eddy tracking methods:

Eddy tracking methods have been developed to follow a large number of mesoscale eddies in the open ocean from the satellite altimetry observations. We could apply such methodologies to track the GW of the SW in our model simulations but this is not necessary: We only have to track TWO VERY WELL DEFINED LARGE EDDIES and we have available 5 day snapshots at a spatial resolution of 1/12° (better than 10 km, i.e. much better than any satellite observation). The tracking by "eye" (i.e. looking at individual velocity snapshot), is just very accurate (one or two grid points or ~20 km or better). In the paper, we now mention that the tacking was made by looking at individual 5 day snapshots.

<u>Change #5 (end of section 2):</u>

Because the model outputs provide a dense space and time sampling of the ocean variables, the tracking of the Somali current eddies described here (mainly the GW and the SG) was simply made by looking carefully at individual snapshots.

On the usefulness to provide statistics, such as Vic et al. (2014):

These authors performed a series of sensitivity experiments, so it was useful for their objective to provide eddy statistics to be able to quantify what was different between simulations. Note however that they used an ensemble approach (performing 8 realizations) to assess if the changes in statistics are due to the change in parameter or to the intrinsic variability of the eddy processes that are studied. The statistics they present would have no significance if they had only one realization because the dynamical system that is studied does have significant intrinsic variability.

We are in a very different framework here and their methodology cannot be applied to our data set (such approach requires dedicated ensemble simulations which are not so easy to perform and not presently available at 1/12° resolution, but that we are presently performing at ¼° resolution).

Nevertheless, we understand that although our analysis is drawn from long time-series (10 y) of 5 day snapshots (i.e. movies), the paper only shows a few snapshots which sometime do not obviously reproduce with enough accuracy what was seen in the movies. This may have created a shift between the text and the figures and contributed to the “too descriptive” character of the paper. We attempted to correct this impression by providing some relevant statistics.

<u>Change #6 (new section 4.4):</u>

We included in the revised paper a new sub-section 4.4 with 3 additional figures, entitled:

"Evolution of Spiciness in the Great Whirl region"

In this sub-section we investigate the time evolution (over 10 years) of histograms of spiciness that allows to distinguishing between the various scenarios.

2 Specific comments

1. abstract: these cyclones are identified as major actors in mixing water masses. There is no clear

evidence for mixing so you shouldn't say major.

We agree.

<u>Change in text:</u> We replaced "major actors" by "potential actors".

2. p737,l1 : Precise which time of the year and introduce the wind stress features at this time; specifically, that the wind stress is upwelling favorable.

The time of the year is summer as mentioned a few lines above at the beginning of the paragraph, and the whole paper being focused on the period of the summer monsoon, it is indeed redundant to mention the "time of year" here.

<u>Change in text:</u> "at this time of the year" has been removed from the sentence.

The reference to the upwelling favorable winds is relevant when we mention the existence of the cold wedges as they directly result from the upwelling process.

<u>Changes in text:</u> when the cold wedge is mentioned for the first time in the introduction:

" … cold upwelled water attached to its northern flank (the *southern cold wedge*, driven by the upwelling favorable winds)."

3. p737,l17 : You mention observations by Beal and Donohue (2013), idealized experiments by (Jensen (1991); Wirth et al. (2002)). You should add some recent results by Vic et al. (2014) on the GW evolution (link with Rossby waves, intrinsic interannual variability,. ...)

We agree.

<u>Change in text:</u> Vic et al. (2014) are now cited in the introduction and in several other occasions when we found it relevant.

4. p738,l18 : Why do you mention fast dynamics? compared to what? you should give scales for the variability. (also in p751,l20 and p753,l19)

"fast" is not related to a specific time scale, but is used in a qualitative way to emphasize the lack of dense observations in space and time to describe those eddy-eddy interactions (e.g. the nadir along track altimetry of Topex/Poseidon with a 10 day repeat orbit significantly aliases these "fast" motions. Whether or not the processing applied to produce the gridded AVISO fields properly corrected this aliasing is still an issue, especially regarding the boundary current). The text being explicit regarding the sampling issue, "fast" is not that useful and can be removed.

<u>Change in text:</u> "*fast*" has been removed here and in other use (also in the title of section 4).

5. p739§2 : Why does the coarsest simulation have more vertical levels than the higher resolution simulations?

Computational cost. The highest resolution 1/12° model was still too costly to handle 75 vertical levels. There are also "historical" reasons specific to Drakkar model configurations. These configurations originally used the 46 levels. The change to 75 levels has been done only on the ¼° configuration, but is now implemented to the ORCA12.

<u>No change in text.</u>

6. p739,l21 : This paragraph is useless and should be removed, it confuses the reader.

We disagree. This paragraph is there to support the validation of the ORCA12 model, since it refers to a number of publications that use model outputs for very diverse studies. It provides a broad assessment of the ability of the global model to simulate regional circulation features.

<u>We keep the paragraph.</u>

7. p740,l17 : Why is the validation performed only on the coarsest simulation? I must say that the validation does not make the reader feel at ease with the simulation as no observational dataset is used! Maybe you should present some validation against Aviso EKE or surface currents.

Two remarks regarding the objective of Section 3 that are relevant to the review's specific comments #7 to #9.

- First remark: the use of "upper-layer circulation" in the title of the section may sound to refer very much to the "first model level". This is misleading since the analyses are performed for the "upper-ocean" in the depth range 0m to 200 m, most analyses shown being around 50 m depth.
<u>Change in text:</u>
In the title of the section "*upper-layer*" is replaced by "*upper-ocean*".

- Second remark: the objective of this section is to show that the representation by the model of the essential large circulation features of the upper ocean circulation (the EACC and its continuation into the SC, the SG and its feeding of the SECC, the GW) is consistent with their description proposed in the literature. This is needed to go forward and begin to look at the interaction between the SG and the GW. For example, it would not make sense to continue our study if the model was not able to simulate the crossing of the equator by the EACC for example.

Now let's go back to the reviewer's specific comment #7.
The validation has been performed on every simulation (See Fig. A1 of this review) but we decided to show only the coarse resolution. We do that because we consider that it is of interest to our community to know that this ¼° eddy-permitting model provides a reasonably good solution in this region (see Change #2).

We modified the text to be more accurate and explicitly mention that the ORCA12 simulations were validated in the same way as ORCA025.

<u>Change in text</u> (*new text in italic*):
"We present here a short validation of the upper ocean circulation in the Arabian Sea during the Southwest Monsoon for the ¼° resolution ORCA025 simulation. *The two ORCA12 simulations have been validated in the same way.* The results described in this section *although illustrated with ORCA025* hold for the two ORCA12 simulations (small differences being the appearance of structures of smaller scale and some spatial and temporal lags expected from the turbulent nature of the flow), and are very consistent with the circulation schemes proposed in the literature (e.g. Schott et al., 2009; Beal et al., 2013)."

Regarding the reviewer remark about the lack of validation by comparison to observational data sets, there have been some validation (in Akuetevi PhD thesis, 2014), but not shown in this paper. As said in the last paragraph of this section:
"At the scale of the whole Indian Ocean, the year-round large circulation patterns and the planetary wave dynamics (not shown, Akuetevi, 2014) simulated by the 1/4° and 1/12° models are in good qualitative agreement with the synthesis of surface drifter and satellite altimetry performed by Beal et al. 2013."

To make the reviewer more at ease, we provide here (Fig. A1 below) the figure equivalent to the Figure 1 of Beal et al (2013) which shows monthly mean patterns of the circulation from a synthesis of surface drifters and AVISO altimetry. This is extracted from the PhD thesis of Akuetevi (2014) referred to in the paper (written in English and free access via the Web) and therefore available.

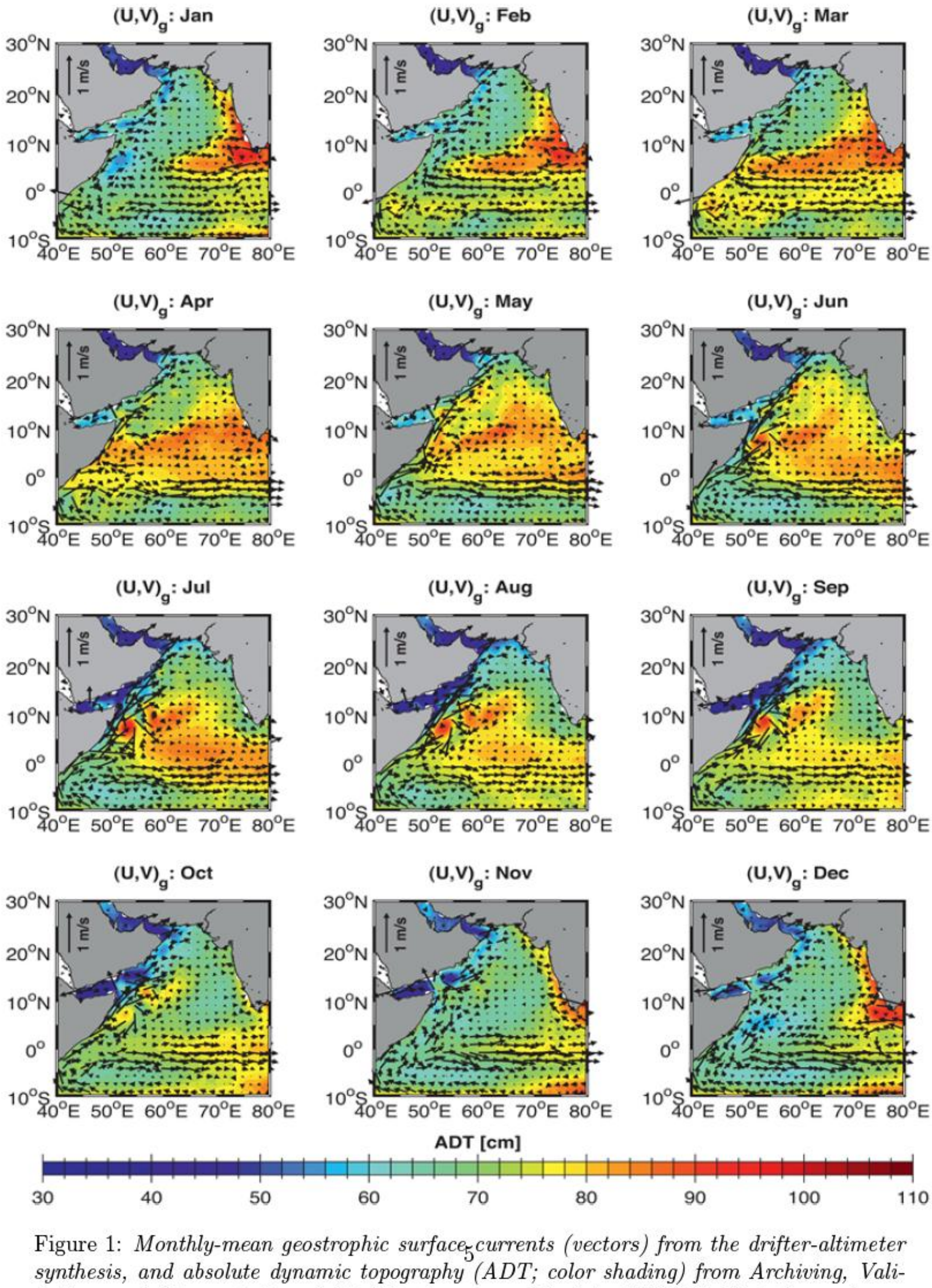


Figure 1: *Monthly-mean geostrophic surface currents (vectors) from the drifter-altimeter synthesis, and absolute dynamic topography (ADT; color shading) from Archiving, Validation, and Interpretation of Satellite Oceanographic data (AVISO). Note the early appearance of the GW, or its precursor, in March, and the year-round SECC. Landmasses are shaded gray [After Beal et al. (2013)].*

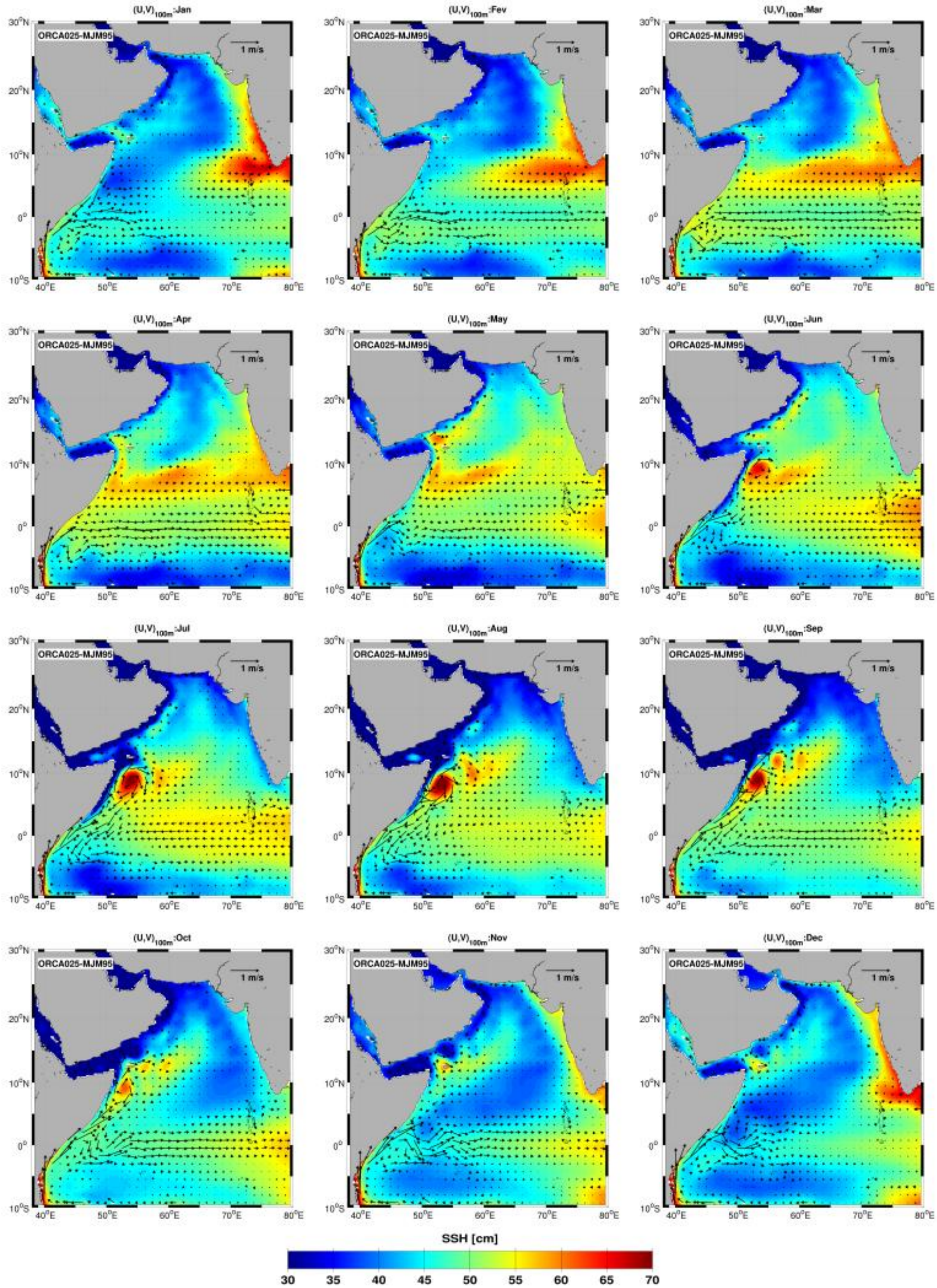


Figure 2: *Monthly-mean currents over 10 years (vectors; $m{\cdot}s^{-1}$) at z = 100m and sea surface height (SSH; color shading; cm) from MJM95 simulations. Landmasses are shaded gray.*

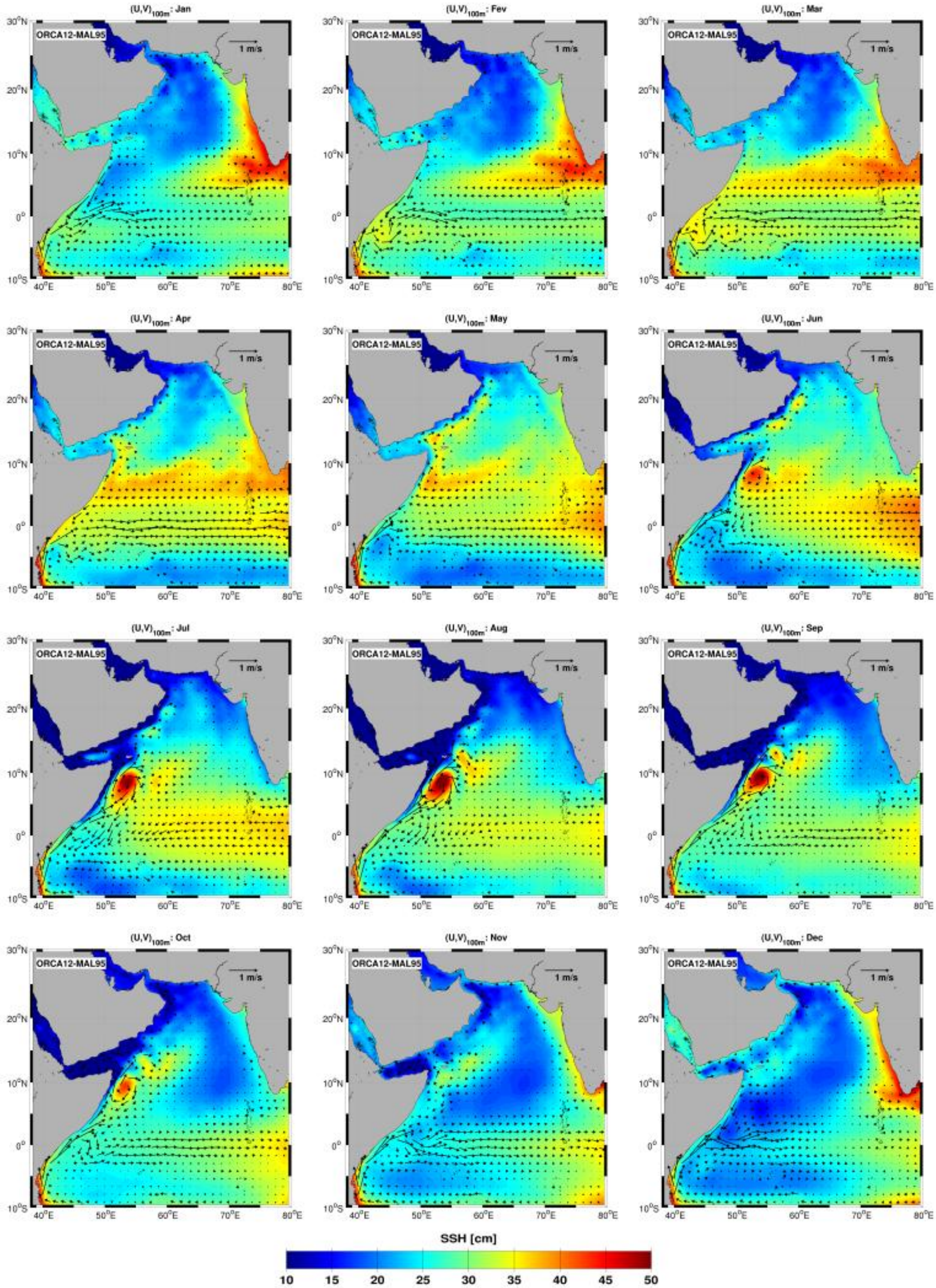


Figure 3: *Monthly-mean currents over 10 years (vectors; $m{\cdot}s^{-1}$) at z = 100m and sea surface height (SSH; color shading; cm) from ORCA12.L46-MAL95. Landmasses are shaded gray.*

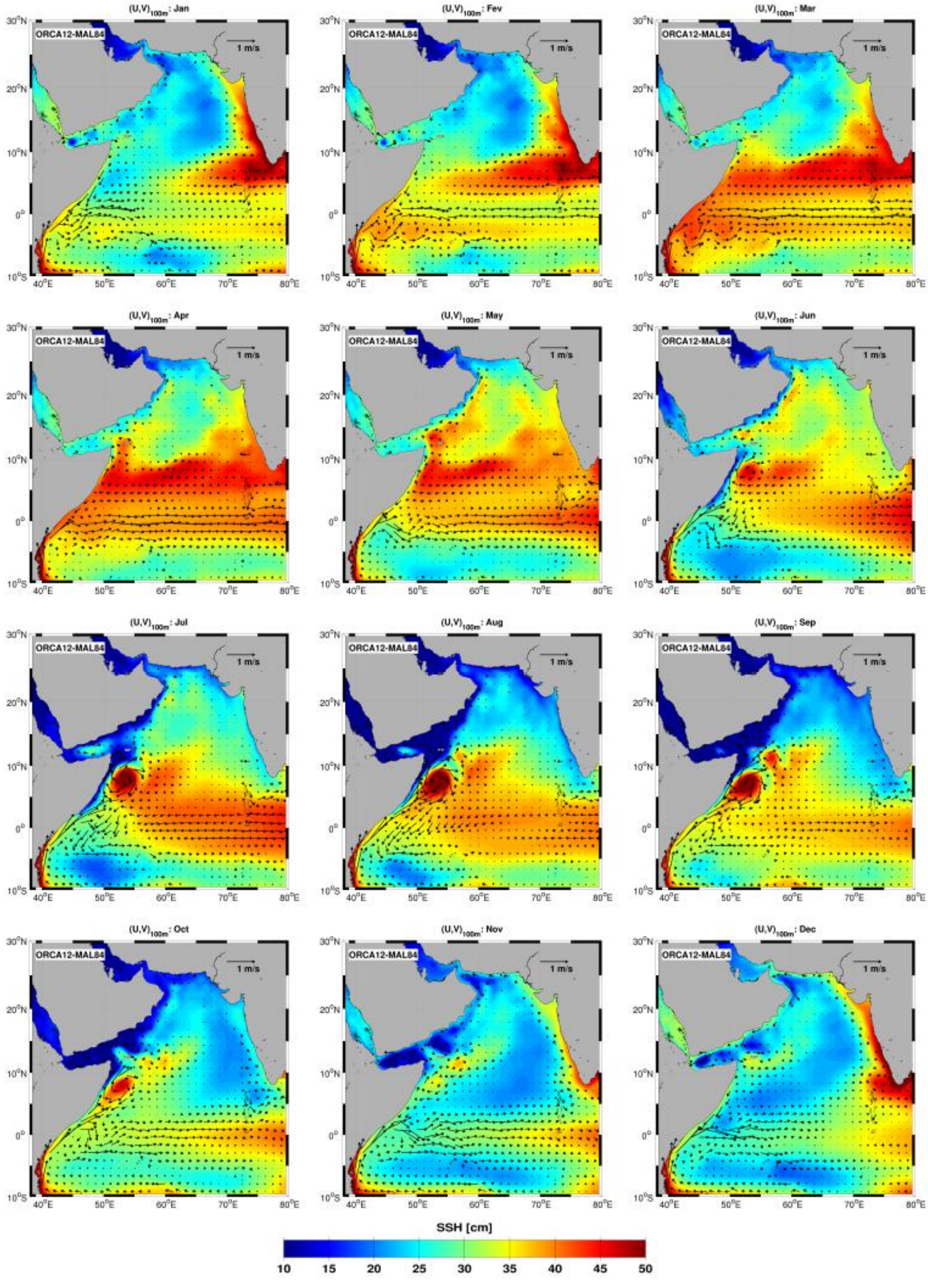


Figure 4: *Monthly-mean currents (vectors;* $8m{\cdot}s^{-1}$*) at* $z = 100m$ *and sea surface height (SSH; color shading; cm) from ORCA12.L46-MAL84. Landmasses are shaded gray.*

8. p740, l26 : Why currents are shown at 100 m depth and not at the surface as you focus on surface features in the article?

Currents are shown at 100 m depth to alleviate the Ekman drift effect that may blur the geostrophic currents which are those used in Beal et al. (2013) (their Fig.1).
Change in text (new text in italic):
" ... the currents at 100 m depth *(mainly to have currents below the Ekman layer which are in better geostrophic balance than the currents of the first model level)* ..."

9. p741,l22 : very good agreement : if so, why not comparing surface currents and show it?

As we said above in the preamble to comment #7, the objective of the comparison is to verify the consistency of the model solution with the broad circulation features as revealed by the existing literature, not to quantitatively compare model currents with observed currents (a challenge by itself, e.g. Scott et al., 2010, Ocean Modelling). Therefore, our comparison should be qualified as qualitative and using "very good agreement" is likely not appropriate (although we are very satisfied by the results shown in Fig. A1).
Change in text:
"... *very good agreement*" has been changed in "... *qualitatively good agreement*".

10. p742,l6 : minimum sea surface temperature : on which area? Is there a threshold value to qualify wedges as cold?

This was poorly written since it is the SST and not the minimum SST that is plotted in Fig. 2. We just want to say that we use SST snapshots to identify the cold wedges, the definition of which is given in the introduction as well marked wedges of cold upwelled water attached to the northern flank of the large anticyclonic eddies (SG and GW) and does not need to be repeated here.
Change in text:
"... *and use the sea surface temperature (SST, Fig. 2c) to detect the cold wedges*."

11. p742,l7 : Give the formula for spiciness

It is not a simple formula that can just be recalled in one line. We did not say it in the paper, but the formula for spiciness used in the paper is exactly the one described in Flament (2002), which includes Tables. We clarify this.
Change in text (new text in italic):
"We compute the spiciness (Π, Fig. 2c) *using the formula described in Flament (2002)* on isopycnal σ_0=23.8 (the depth of which varies between 50 and 100m)."

12. p742,l16 : Again, why currents are at 50 m ?

Again: Ekman and because it is a depth often reached by the isopycnal (23.8) on which spiciness is plotted (see above).
No change in text.

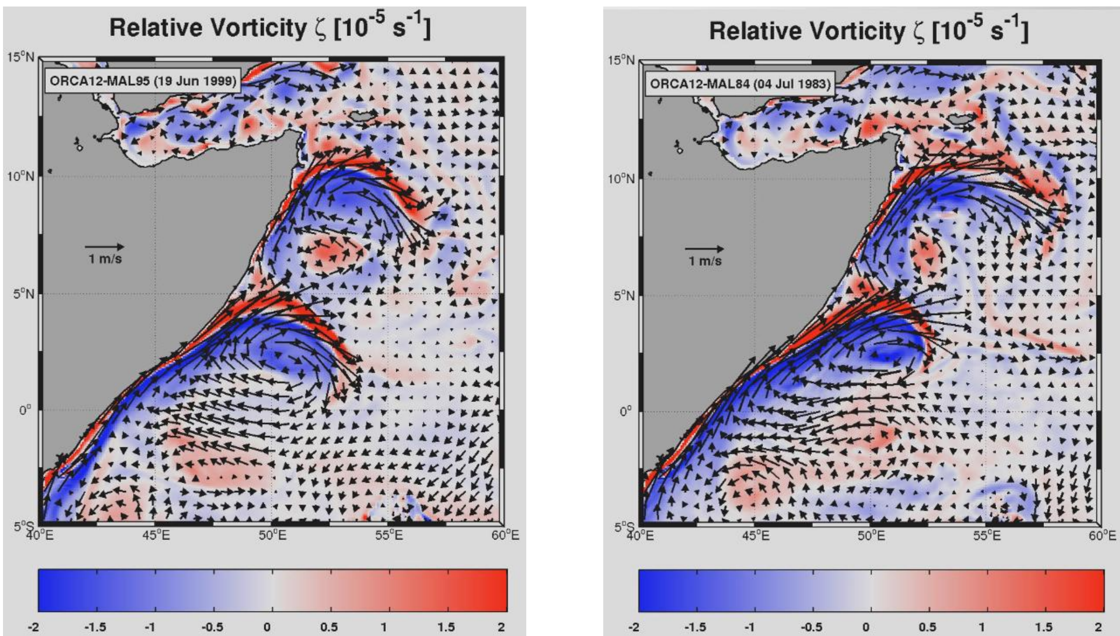
13. p743,l6 : detachments of positive vorticity : It may have to do with the generation of a frictional boundary layer. A same phenomenon occurs on the shoreside of the Gulf Stream (Gula et al. (2015)). Shedding of cyclones also occur. Do you have those formation of eddies with the no slip and free slip simulations? How are eddy characteristics changed (vorticity range, size, . . .)?

This item includes 3 different questions, thus requires 3 answers.

- The detachment of positive vorticity is different from what is discussed by Gula et al (2015) for the GulfStream (which is not a low latitude boundary current). In Gupa et al. trains of cyclones are generated on the cyclonic (inner) side of the Gulfstream, and their generation is due to horizontal shear instability triggered by the bottom drag on the irregular slope. They are "submesoscale" processes. In any case, the 1/12° resolution is not enough to resolve this type of instability (1/60° is necessary). In our case, the process of detachment of positive vorticity followed by the formation of a cyclone is the one described in

Robinson (1991) or more recently by Akuetevi and Wirth (2015) as an intermittent detachment of the thin layer of large positive values (cyclonic) of relative vorticity that exists along the coast. We refer to these papers in the text.

- We do have formation of bursts and cyclones in both free slip and partial slip configurations, as it was said in the paper at the beginning of the section 4.2.1 on bursts "… *are observed in all three experiments*" and refering to Fig. 3-5. But because Fig. 3 and Fig. 5 only show the free slip case, we add a reference also to Fig. 7 (a partial slip case where cyclones are quite visible). The similarity between the slip and partial-slip cases is illustrated in the figure below (Fig. A2) that we provide only for the review and which shows situations that are hardly distinguishable between the two simulations.



(a) Partial Slip MAL95 (b) Free Slip MAL84

Figure A2: Snapshots of current (vector) and relative vorticity (color) at 50 m depth in (left) the partial slip MAL95 experiment, and (right) the free slip MAL84 experiment. Differences between the two maps are barely discernible. Both figures show 2 very similar situations with an intense filament of positive vorticity along the coast (also described in Akuetevi and Wirth, 2015) which shoots off shore in the open ocean on the northern limb of the large anticyclones. These shoots later generate cyclones. In the present case, we can see a cyclone merging into the GW.

- We did not look if the characteristics of the cyclones are dependent of the lateral friction parameter but we could probably find some differences. But we also expect a large variability from one cyclone to another. Therefore a very large number of cyclones should be looked at to reach significant results. This is another study.

Nevertheless, the important result of the paper will still hold:
The generation of bursts and cyclones follows the same scenario in all simulation, in a way similar to the idealized study of Akuetevi and Wirth 2015, indicating that this process is robust to this parameter, which gives an explanation for the origin of the flanking cyclones observed by Beal et al.

Change in text (section 4.2.1):
"Fig.3-5" is replace by "Fig. 3-5-7"
We also provide more information regarding Akuetevi and Wirth (2015) (*new text in italic*):
"These events were previously identified and called bursts in analogy with the bursts or ejections of

vorticity patterns in the classical boundary-layer dynamics (see Robinson, 1991; Akuetevi and Wirth, 2015). *Akuetevi and Wirth (2015) explain that the thin layer of large positive values (cyclonic) of relative vorticity that exists along the coast in a low latitude western boundary current is the siege of intermittent detachments of cyclonic vorticity bursts."*

14. p744,l6 : scenario i : I don't see the merging between a cyclone and the GW (anticyclone). How can you infer that from the maps?

Yes indeed. The top-right and middle right panels of Fig. 3 (in which time is bottom-up) show a "merging". On 19 June (middle-right panel), the cyclone is located in the center of the GW. On 9 July (top-right), the cyclone has greatly diminished its intensity and is being absorbed into the GW. It is difficult to isolate the role of the cyclone only because the interactions are multiple: for example, the interaction between a cyclone and the GW often occurs simultaneously with the collision of the GW with the SG. Our comments regarding these interactions and the associated mixing are based on cases when this interaction could be isolated, as in August 1986 in MAL84 shown in Fig. A3 below.

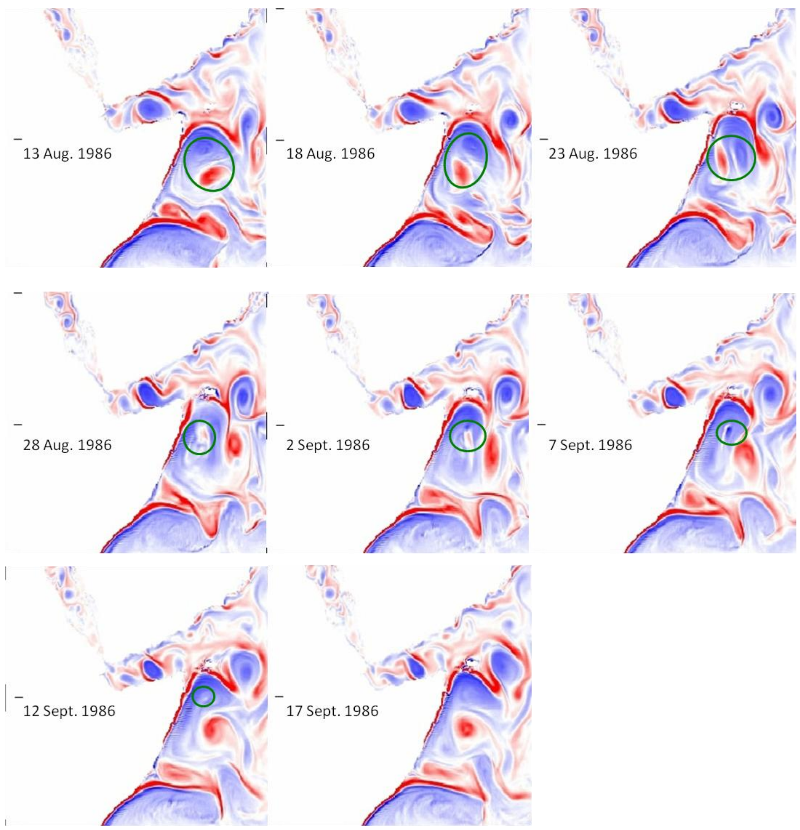


Fig. A3: Simulation MAL84 at 1/12°. Sequence of 5 day snapshots showing the « merging » of a cyclone into the Great Whirl (green circle).

<u>Change in text (section 4.2.2):</u>
"as illustrated in Fig. 3 for the 1/12° simulation)."
Is replaced by:
"This is illustrated in Fig. 3 for the 1/12° simulation: On 19 June (middle-right panel), the cyclone is located in the center of the GW (the red spot outlined by the green arrow). On 9 July (top-right), the cyclone has greatly diminished its intensity and is being absorbed into the GW."

15. p744,l12 : This process is responsible for the mixing : Fig. 5 shows a different scenario : collision between GW and SG. You should show exactly the same snapshots than Fig. 3 for spiciness.

16. p744,l19 : clearly influences : not so clear... You should show climatological mean and standard deviation of spice to see the water masses properties and variability to infer on mixing efficiency.

We agree that the mixing efficiency is not quantified in the paper and therefore we are now much more careful when we mention mixing. What we meant to say when we discussed mixing was very simple: the processes related to the generation of the cyclones entrain the upwelled waters of the cold wedges into the large scale circulation and this certainly influences their mixing with the surrounding waters. The whole section concerned by the two remarks above has been modified.

Change in text:

i. The cyclonic vortex remains attached to the large anticyclonic eddy, circles around it, returns towards the western boundary before being sucked up into the large anticyclonic eddy and both merge.

This is illustrated in Fig.3 for the 1/12° simulation: on 19 June (middle-right panel), the cyclone is located in the center of the GW. On 9 July (top-right), the cyclone has greatly diminished its intensity and is being absorbed into the GW. This process somewhat weakens the large anticyclonic eddy and contributes to its decay. The cyclonic vortices created by the GW most often follow this trajectory. This trajectory is the one proposed by the analysis of Beal et al. (2013).

ii. The cyclonic vortex does not pair with large anticyclonic eddy and drifts in the open ocean.

The behavior of the bursts after ejection followed by the dipoles formation is a very well-marked phenomenon which entrains the upwelled-water masses detached by the bursts from the cold wedge and could contribute to their offshore mixing. As can be seen in the properties of the water masses transported by the SG and the GW in Figs. 5 and 7, from the early to the late phase of the southwest Monsoon, the bursts, their transformation into cyclonic eddies and their chaotic behavior are greatly triggering the entrainment of the upwelled waters within the eddies and offshore region of the Somali Coast. The dynamics of the bursts also injects positive vorticity within the large anticyclonic seasonal eddies (the SG and the GW), prompting the short timescale variability of these eddies, as observed by Beal et al. (2013), but also contributing to their decay.

17. p745,l1 : 1/4°simulation is useless in this discussion since it is not designed to resolve mesoscales.

We disagree: if the ¼° has a chance to be relevant, it is on the large anticyclones which at the resolution of ¼° (~25 km) are resolved with at least 10 grid points since their size is greater than 350 km. This is the case here since the plot of Fig. 3 show that this model is able to generate the large anticyclones, the bursts and the cyclone in a way that qualitatively compares with the 1/12°. Of course, as we said in the paper, these features, and especially the smaller ones (cyclones) are not as well resolved as at 1/12°.

No change in text.

18. p745,l12 : The intensification of the southwest monsoon during June amplifies the intensity of the GW : you should be more precise, Vic et al. (2014) show that the action of the wind stress curl intensifies the GW.

This is probably true in our simulation but we haven't done the calculation to prove it. We just notice that the GW intensifies when the monsoon intensifies and we consider that this is enough given that the objective of the paper is not the study of the generation of the GW by the wind (as it is in Vic et al.) but is to describe the interaction between the SG and the GW.

No change in text.

19. p745,l16 : migration at a speed of 1 m/s : This estimate is based on snapshots shifted for more than 1 month. You should use the 5-day outputs to give a more accurate estimate. Again, tracking the eddy center would allow to be more precise.

No. This estimate is based on five day snapshots, and is an "approximated value" because the speed is not a constant and varies from snapshots. The instantaneous speed varies due to many eddy-eddy interactions such that its "average value" depends upon the period considered. But again what would be the relevance to the objectives of the paper to provide an accurate time series of that speed? An approximated value seems good enough.

<u>Change in paper:</u>
A speed of *approximately* 1 m/s.

20. p745,l19 : You should provide the frequency of events in this part.

We do not understand this comment. We are looking at processes that are seasonal processes, so the reference "frequency" is the season. If the reviewer refers to the "occurrence" of the various scenarios during the 10 years period of the simulations, then we consider that we must describe the scenarios first. Note also that the new figure 10 provides a continuous view of the occurrence of the processes.

<u>No change in text.</u>

21. p745,l24 : Scenario ii has not been described in literature and is the most frequent in your simulations. Can you extend on that?

Scenario ii is the "collision with no merging" scenario. This issue was briefly discussed in the conclusion when we said that *"the conclusions of the present study should also be challenged by future studies that may use sufficiently dense (in space and time) satellite observations (e.g. SWOT) or eddy resolving ocean reanalyses, thus giving opportunities to consolidate our findings or suggesting alternative explanations."*

It is possible that the southern gyre is not easily detectable is SSH data because of its vicinity with the equator (it is likely largely ageostrophic). Indeed, our model dat show a small signal in SSH and a large signal in vorticity (Fig. A4 below).

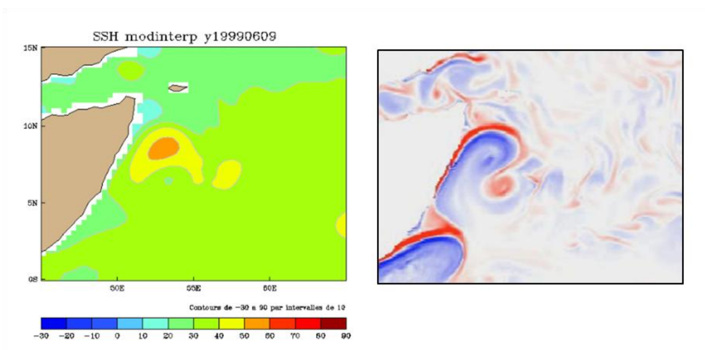


Figure A4: Snapshot of SSH and relative vorticity on the same day (6 June 1999) in simulation MAL95 showing that the Southern Gyre which is clearly visible in the vorticity map has a very weak signature in the SSH map. This figure is not shown in the paper, but is presented to the reviewers to confirm what we say in the paper.

We did look carefully at AVISO data (period 1993-2004). We combined these SLA data with the mean SSH of Maximenko and Niiler (1995) in the exact same way as it was done in Beal and Donohue (2013) to produce an absolute SSH that can be compared to the model SSH. To verify the correctness of our procedure, we reproduced their results. Then we also collocated (in space and time) the SSH from the 1/12° MAL95 simulation onto the Aviso data base to make comparisons (same period 1993-2004).
Many results can be drawn from this comparison, but regarding the present study, we found that the Southern Gyre does not appear clearly in the AVISO or in the model sampled like AVISO. We also found a number of sequences in AVISO data that appear, by analogy with MAL95, consistent to the collision process (Fig. A9a) and less that are clearly consistent with a southern gyre that does not migrate northward (Fig. A9b). But we found that the time and space resolution is not sufficient to reach a clear conclusionwith additional information as we have for the model.

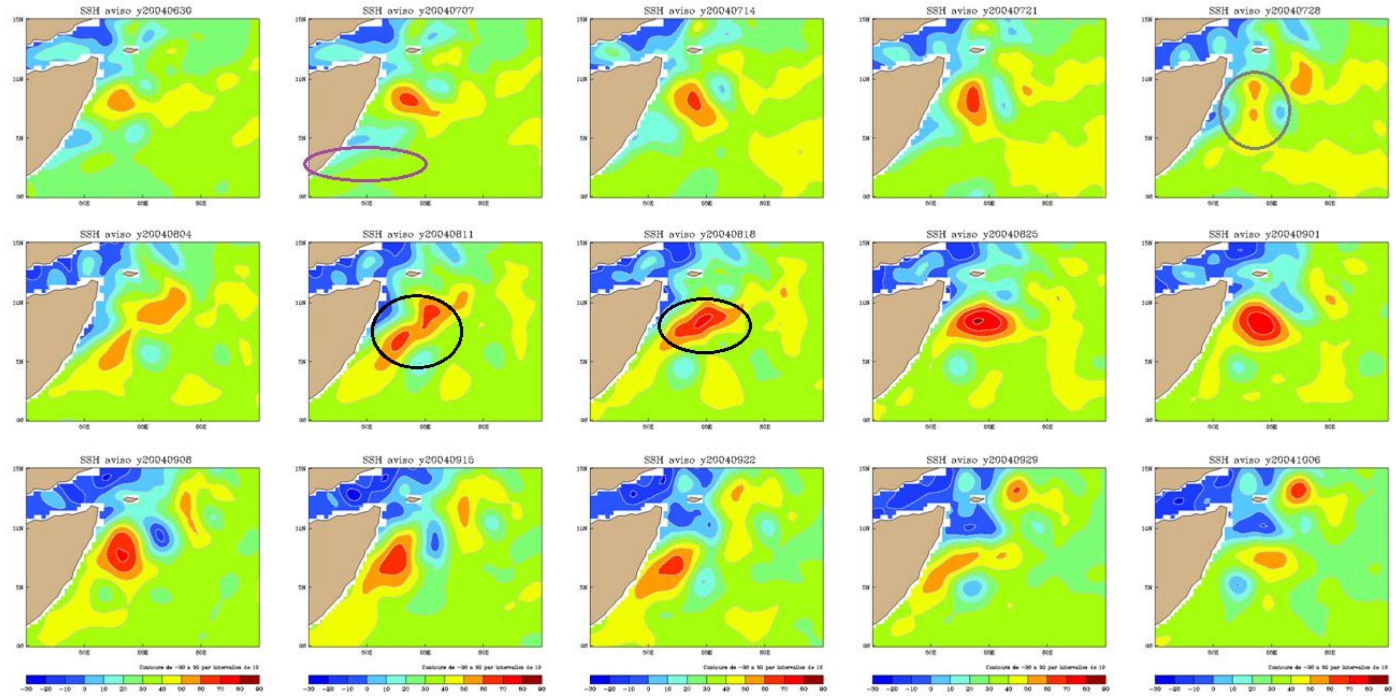


Fig A9a. Sequence from 30 June to 6 October 2004 of 7-day snapshots of SSH from the AVISO SLA data to which the mean SSH from Maximenko and Niiler (1995) has been added. The SSH high outlined by the purple circle on 7 September suggests that a SG in growing. The sequence suggests that the SG migrates northward and collides with the GW (grey circle on 28 July). Then, the sequence shows that only one eddy remains (black circle on 11 and 18 August). The sequence could very well correspond to a collision without merging of the two eddies because it is very similar to that shown in Fig. 9Ab for the 1/12° MAL95 simulation which corresponds to a year when the SG and the GW collided but did not merge.

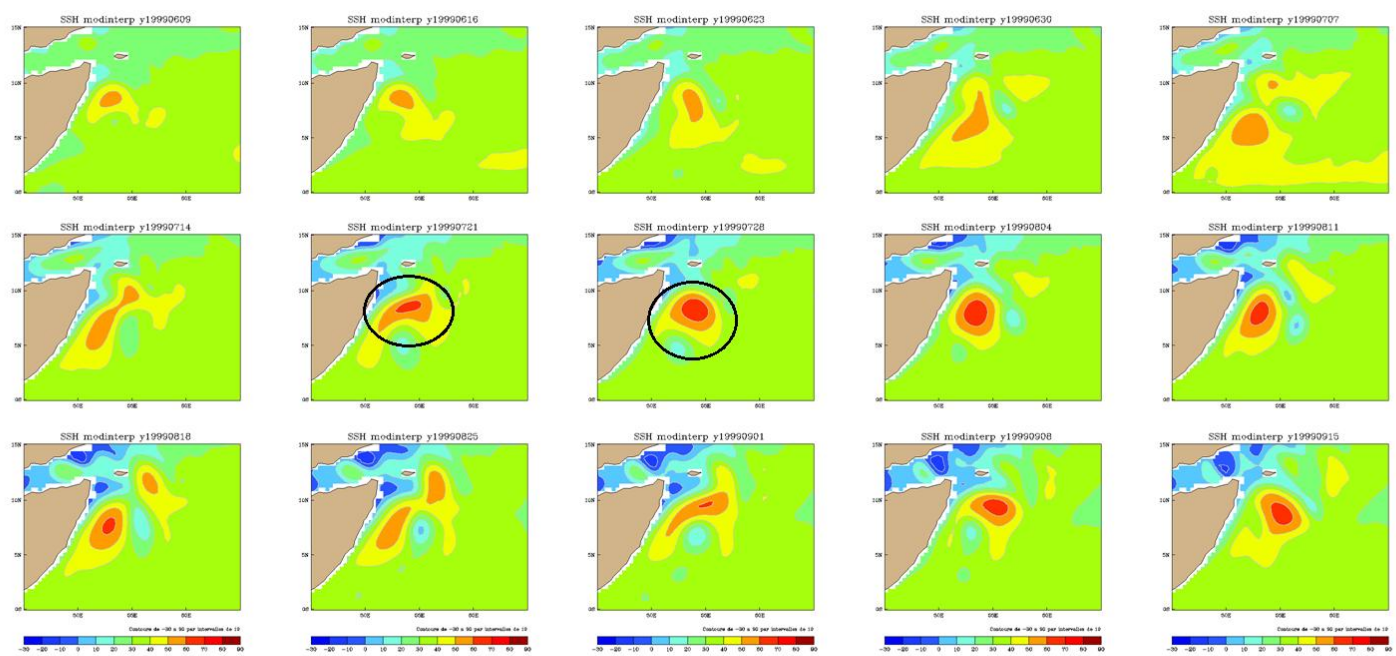


Fig A9b. Sequence (from 9 June to 15 september 1999) of 7-day snapshots of SSH from the 1/12° simulation MAL95 outputs collocated in space and time onto the AVSISO data. The southern Gyre did migrate northward that year and collided with the Great Whirl. The black circles outline the occurrence of the collision. The GW moved to the northeast and in the place where the Socotra Eddy is usually found. This situation is comparable to that shown with the AVISO data in Fig. 9Aa above.

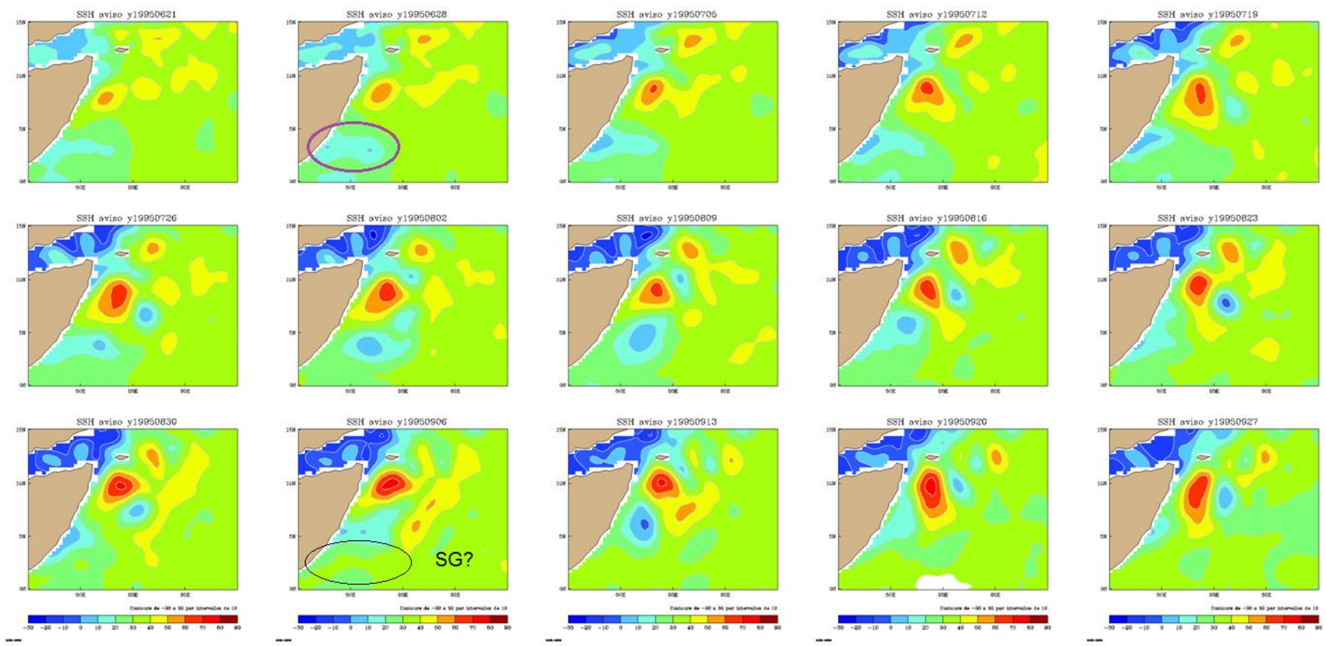


Fig A10a. Sequence from 21 June to 27 September 1995 of 7-day snapshots of SSH from the AVISO SLA data to which the mean SSH from Maximenko and Niiler (1995) has been added. The SSH low (purple circle) that is found to persists along the Somali coast below 5°N suggests that there is no SG (which should be a high) before 6 September when a SSH high appears (the black circle). This suggests that collision occurred between the SG and the GW that year.

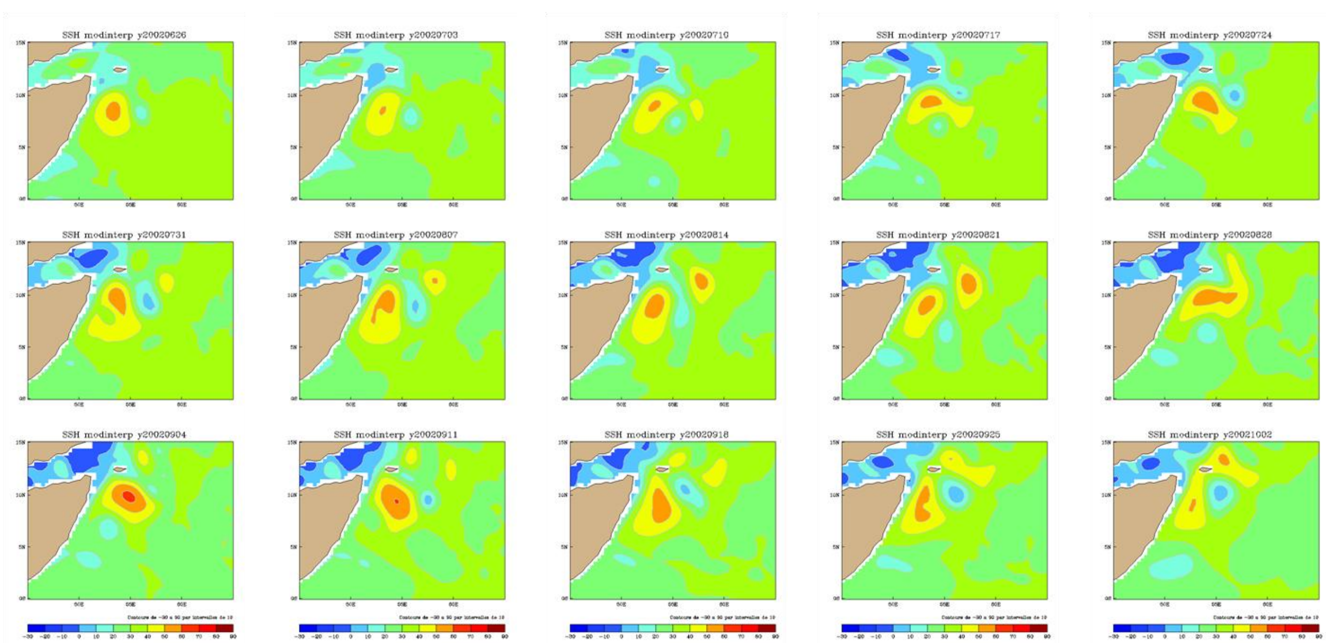


Fig A10b. Sequence (from 26 June to 2 October 2002) of 7-day snapshots of SSH from the 1/12° simulation MAL95 outputs collocated in space and time onto the AVSISO data. The southern Gyre did not migrate northward that year and no collision occurred. This situation is comparable to that shown with the AVISO data in Fig. 10Aa above.

Following the reviewers comment, we extended this discussion in the revised paper. A new paragraph has been added, which challenges the finding of this study. If required, the above figures could be proposed as additional material to be put on line but not in the paper itself because it will require four additional large figures.

Change in text (new paragraph is the conclusion):

Model results produced by a single numerical code must be interpreted with caution as they are, to a degree that is often not possible to assess, influenced by specificities of the numerical code used, and the scenarios described here are no exceptions. Therefore it cannot be ruled out that the numerical model used here (i.e. NEMO) although describing the various possible scenarios in rather robust way may unrealistically favors one specific scenario (i.e. the collision without merging of the GW and the SG) rather than the others. Indeed, it is somewhat puzzling that our most frequent scenario is not being frequently mentioned in the literature. A reason could be that this event is not as frequent in the real ocean as the model shows, and that the northward motion of the Southern Gyre is more often limited below 5°N. Our models would therefore be biased toward one specific scenario for reasons that still have to be determined but might well be related to the fundamentals of the numerical code (e.g. vertical coordinate system, order of the numerical schemes, etc.) rather than configuration settings (all three configurations used here are favouring the same scenario). But studies of the Southern Gyre are rare (attention is usually given to the Great Whirl) and to our knowledge satellite altimetry has not been applied to the dynamics of this circulation feature. Looking at the model 5 day snapshots of Sea Surface Height (SSH) we found that the Southern Gyre begins to be detectable in this variable only after it reaches latitudes of 4 to 5°N (not shown).
To further investigate this issue, we compared our model outputs with satellite altimetry data. We combined the AVISO sea level anomalies (SLA) data with the mean SSH of Maximenko and Niiler (1995) in the exact same way as it was done in Beal and Donohue (2013) to produce an absolute SSH that can be compared to the model SSH. Then we collocated (in space and time) the SSH from the 1/12° MAL95 simulation onto the AVISO data base. We processed both data sets on the same period (from 1993 to 2004).
We found that the Southern Gyre does not appear clearly in the AVISO or in the model sampled like AVISO (not shown). A reason for that could be that the Southern Gyre is largely ageostrophic in the vicinity of the equator and therefore has a weak signature in SSH. We also found a number of sequences in AVISO data that could correspond to the collision scenario because of a (subjective) analogy with MAL95 data (not shown), and fewer that are clearly consistent with a southern gyre that does not migrate northward (again by analogy with the model, not shown). But analogy with a model sequence is certainly not accurate enough to to reach a clear conclusion on the process described by the satellite data. It is therefore possible that the present nadir altimetry which provides heavily filtered/extrapolated maps of SSH every 7 days, do not have the adequate sampling to follow this circulation feature with the required level of details, and that additional observations or a different processing that take into account the fast evolution of the signal are necessary.

22. p746,l19 : How does the physics of the simulations change the frequency of events?

The frequency change of the events could be influence by the interannual variations of the monsoon winds, by the intrinsic variability of the processes themselves, and also by model parameters such that lateral friction. We do not have the proper set of global model simulations to address this issue. We consider that our answer to comment 21 also covers this comment.

23. p748,l22 : it challenges the collapse interpretation based on the collapse of the two cold wedges. Do you have a reference for that? What does collapse of the two cold wedges mean?

We used "collapse" when the two cold wedges merge to make one because it has been used in the literature, but "merging" is certainly appropriate.
But (see review #2 comment 13), as we mentioned in the introduction, the merging (or collapse) of the

two cold wedges has been in few previous studies interpreted as the merging of the SG and the GW. Our model results suggest that this merging does not necessarily means that the two anticyclones are merging. This is just what this sentence aimed to say. We agree it is not clear and that it also appears as a quite unnecessary statement at this place of the paper. The sentence is removed and the text is simplified.
<u>New text</u>:
The collision did not produce the coalescence between the SG and the GW, but their respective cold wedges have merged.

24. p749,l26 : Can you compare the shield of the vortex to the situation in Valcke and Verron (1997)?

Comparing idealized QG simulations (layer model) with a full PE model in a realistic configuration is not that simple. We did not do it.
<u>No change in text.</u>

25. p750,l23 : they result from different ways of interaction of the GW with the topography of the Socotra Island. : This is a strong statement and you didn't mention it before. Could you give arguments supporting that the GW interacts with the topography of Socotra Island?

This is an obvious statement. The Socotra Island is connected to the African continent through a topographic ridge, and the northern limb of the Great Whirl is (just by chance?) tangent to this topographic feature. And looking at the snapshot of Fig. 4 (bottom panel) or the sequence of Fig. 8, it would be naïve not to suggest that interactions of the flow with the topography have a role to play.
<u>No change in text.</u>

26. p751,l5 : fine-scale. It's not precise enough, you're talking about mesoscale.

We mean "*the sharp currents and vorticity fronts, smaller flanking cyclones*".
This has been added in the revised paper.

27. p751,l23 : topographically constrained : it's currently not supported by your analysis so you should not mention it, or if it's a well known feature, give a reference.

Again this is obvious and related to the coastline. The natural" way of eddy motion is the the west. NBC eddies can move (north) westward toward the Caribbean Sea and the North Atlantic because the orientation of the coastline. It is not the case of the Somali Current eddies: because of the orientation of the coastline, these eddies cannot follow their natural way of motion. If this is not a topographic constraint, what is it?
<u>No change in text.</u>

28. p752,l16 : main drivers of the mixing : At this time in the manuscript, we're not sure they are the main drivers. Be more cautious.

OK. We have not quantified mixing. Bit Fig. A3 of this review strongly suggest that when a cyclone merges into the GW, it does so in mixing its water masses with those of the GW.
We use "likely important drivers"

3 Technical corrections

In the following, (xxx) means "remove xxx".

Every correction accounted for except when specified.

1. abstract : allows (us)
2. abstract : encounterS
3. p736,l24 : (our) - Fig. 1
4. p738,l6 : sentence is too long and elusive : The lack of understanding. . .
5. p738,l17 : (in space and time) -¿ spatio-temporal

We did not follow the suggestion

6. p738,l24 : A section does not perform: In section 4 we perform
7. Table 1 : change the name of simulations to more convenient ones. We don’t know what MJM95,MAL84 and MAL95 stand for.

We did not follow the suggestion

8. p740,l25 : (a)
9. p741,l12 : (month of)
10. p741,l19 : (no figure shown) -¿ not shown
11. p742,l19 : parallel to THE coast
12. p742,l19 : (It is noteworthy to mention ...) -¿ Note that ...
13. p743,l7 : precise ’Western Boundary Current’ as it is not written before.
14. All figures : fontsize is too small

We shall correct that when the paper is accepted.

15. Map figures : time should increase from left to right and top to bottom, as in usual text reading.

We shall correct that when the paper is accepted.

16. vorticity maps : should be non-dimensionalized by the Coriolis frequency to give an approximate Rossby number, useful to quantify the non-linearity of the dynamics.

Not possible near the equator where f is zero. We did not follow the suggestion.

17. p745,l6 : (unravel) -¿ untangle

Same meaning. We did not follow the suggestion.

18. p746,l14 : (At rare occasions) -¿ Occasionally

We did not follow the suggestion. Rare is important here.

19. p747,l13 : (ζ plot) -¿ Fig.5 a-d

We did not follow the suggestion since there is no ambiguity on the vorticity plots, and figures are not referred to as (a), (b), etc.

20. p747,l15 : (shooting) -¿ protruding

Shooting has a "dynamical" connotation thus more appropriate than "protruding" which appears more "static". We did not follow the suggestion.

21. p750,l9 : (more or less)
22. p751,l8 : (permitted) -¿ allowed
23. p751,l9 : (give more light) -¿ shed light
24. p752,l15 : (detached) -¿ detach