

Anonymous Referee #1

Received and published: 6 January 2013

The paper aims at assessing the role of eddies in the Ligurian-Provencal and Tyrrhenian basins, focusing on the differences between eddy populations. The assessment mainly relies on numerical outputs from two different models, the operational MERCATOR product and a ROMS-based configuration. Allegedly, this is the first comparative study between eddy populations in the two basins. The paper shows that in the surface layers of both basins, the majority of eddies can be found near the boundaries, while in the deep layers, they are concentrated in the central parts. The Ligurian-Provencal basin is richer in intermediate and deep eddies than the Tyrrhenian basin, however. Furthermore, surface and intermediate eddies are mainly anticyclonic while deep eddies are mainly cyclonic. The paper is organized and easy to follow until paragraph 3.2 on eddy distributions: the last part lacks of a logical sequence. The English language is fine even though different typos can be found throughout the manuscript. Subpanels of figures are often referenced wrongly. Thus, I recommend proof-reading of the whole manuscript. One typo is even in the title (Asymmetric). The introduction is focused, right to the point and clearly states the aim of the work (role of mesoscale eddy activity in the two basins). The English language in the introduction is also somewhat better than in the rest of the paper. The methodology lacks of important information about the ROMS-based configuration (see points 6, 7 and 9 below) and has important limitations for studying the submesoscale (see points 4, 5 and 15 below). As acknowledged by the authors themselves (see point 35 below), model results should also be better validated, especially targeting eddy populations (see points 11, 17, 18 and 19 below). Some doubts on the reliability of the ROMS configuration can be raised. Finally, the discussion part on theory is confusing and not sharp. For all these reasons, I suggest a process of MAJOR REVISION. Specific concerns are reported below, page by page.

The authors would like to acknowledge the thorough review carried out by referee-1. In order to distinguish our answers from the reviewer's comments the author's responses are labeled as 'AR'.

Professionals will carry a prof-read of the revised version of the ms. before re-submission. Figures will be carefully revised in order to improve clarity. Additional information will be added to the methodology section. We will address the doubts regarding the ROMS configuration and we shall improve the discussion.

A MAJOR revision is in progress before re-submission.

CONCERNS

Pag3522

1) L15: To what exactly the asymmetry in the title is referred to? The eddy sense of rotation (anticyclonic vs cyclonic)? Why is this not reported in the abstract?

AR: To avoid further confusion the revised manuscript will be entitled: **'Eddy distribution in adjacent sub-basins. A numerical study of the Tyrrhenian and Ligurian Seas'**

2) L19-20: the transport of what? This is related to point 24 below. Pag3524

AR: This refers to the barotropic stream function calculated between 0-50m (Fig. 12a) and between 500-2000m (Fig. 12.b). Formulae and more details will be added to revised version.

3) L14-15: It looks to me that the ECC runs southwards, contrarily to what it is stated. Pag3525

AR: the Eastern Corsica Current is a bifurcation of the TC and runs northwards, , along the Eastern side of Corsica, as suggested by its name (Millot, 1999; Astraldi and Gasparini, 1992; Vignudelli et al., 2000; André et al., 2005); on the Western side of Corsica there is the WCC –Western Corsica Current (Taupier-Letage and Millot, 1986; Millot, 1999; Astraldi and Gasparini, 1992; André et al., 2005).

4) L14-15: Authors state here that the focus is on mesoscale, not submesoscale. This is correct in my opinion (see points 5 and 15 below). I have the impression that the submesoscale word was just added throughout the paper at the end. Pag3527

5) L11-14: Rossby radius is order of 10km (as stated by the authors at L19-20 of pag3523). At 3km resolution (1/32 deg), you have 3 grid points to resolve it. Thus, it is clear that the current resolution is fine for mesoscale eddies but not for submesoscale ones

AR: Before completely removing the discussion of submesoscale eddy activity the authors will attempt to calculate 1 km solutions.

6) L18-19: What was the specific reason to prefer the MERCATOR product to the MFS one?

AR: No specific reason! They are both Mediterranean products and we used MERCATOR to avoid forcing ROMS with climatology, thus capturing some inter-annual variability.

7) L21-22: I do not understand and this needs better explanations. COADS is a very coarse product (1 deg x 1 deg). Why the authors are not using the heat and salt fluxes coming from COSMO-I7, i.e. the same high-resolution atmospheric model they are using for the momentum fluxes? This may lead to inconsistencies between momentum and heat forcing at the surface, important in areas of convection.

AR: COADS is (0.5 deg x 0.5 deg) not a (1 deg x 1 deg) resolution. Unfortunately, we did not have access to COSMO-I7 heat fluxes (for the 2010-2011 study

period). I am not sure which 'salt fluxes' the referee is referring to? Nevertheless, convection seems to be well represented for the Ligurian and Tyrrhenian Seas, during summer Mistral events (see e.g. Small et al., 2012 and classical references therein). As oppose to convection in Polar Regions, convection in the Mediterranean Sea is essentially mediated by the wind.

In any case, this is a common practice. There are plenty of ROMS's studies in the scientific literature using QuikSCAT wind data and these are obliged to use COADS (and/or) heat flux, since QSCAT does NOT provide atmospheric flux data (see e.g. Kersalé, M., Doglioli, A.M., Petrenko, A.A., 2011. Sensitivity study of the generation of mesoscale eddies in a numerical model of Hawaii islands. *Ocean Sci.* 7, 277–291; or Couvelard, X., **R.M.A. Caldeira**, I.B. Araújo, R. Tomé, (2012). Wind mediated vorticity-generation and eddy-confinement, leeward of the Madeira Island: 2008 numerical case study, *Dynamics of Atmospheres and Oceans*, Volume 58, Pages 128-149, ISSN 0377-0265, 10.1016/j.dynatmoce.2012.09.005).

8) L25, typo, local. Pag3528 AR => Corrected in revised version!

9) L6: the ROMS setup lacks of important information present in all numerical studies, namely: a) which open boundary conditions are used? Is it a simple nudging to MERCATOR products? If yes, what are the nudging scales and how large are the nudging areas? b) which numerical schemes are the authors using? c) what are the values for viscosities and diffusivities? d) which vertical turbulent schemes are used? K-eps?

AR: We will provide more detailed description of the numerical model setup in the revised version of the ms. a) Open boundaries are ORLANSKY (for 3D variables) and Flather (for 2D variables). Nudging area (sponge layer) has about 10 points reaching 10-60km, depending on the region; b) We used ROMS's SPLIT-UPSTREAM-SCHEME (Marchesiello et al., 2009) with numeric diffusivity rotated along geopotential; c) explicit viscosity and diffusivity are null everywhere except in the sponge layer where it is set to $150\text{m}^2/\text{s}$; d) KPP (Large et al., 1994).

10) L20: Are really "v" and "u" used with the opposite usual meaning? Pag3529 AR => We will check and correct (if necessary) in the revised version!

11) L6-16: the KE visual comparison is ok but a stronger validation is needed. Specifically given the focus on eddy populations it would be worthy to have comparison of frequency distributions of eddy SSH coming from AVISO vs models.

AR: Following Chelton (2010), the eddy radius detection by AVISO products is approximately 40km. Our largest eddies are less than 70km in diameter. Thus we will not capture most mesoscale eddy activity in AVISO products for this region.

Recent studies have used Synthetic Aperture Radar (SAR) data to capture eddy activities in the Ligurian region, however SAR is not freely available (see e.g.

<http://www.ocean-sci-discuss.net/10/55/2013/osd-10-55-2013.html>). We are currently making efforts to include some SAR data analysis in the revised version of the ms.

HF radar and/or any other high-resolution data set can only (partially) capture the in situ dynamics as the ADCP sensor does. In spite of all the limitations the numerical model solutions does provide us with a basin-scale view at mesoscale resolution suggesting that there is a need to start linking the dynamics of these two adjacent basins. Hopefully this will also contribute to spark new observational efforts.

12) L18: Fig3b or 3a? Pag3529 AR => 3a-2009; 3b-2010

Pag3530

13) L12-16: not clear. What are the authors referring to? Please rephrase. AR => We will clarify this in the revised version!

14) L25-27: I completely agree with the statement by the authors. For the same reason the choice of COADS climatology for heat and salt fluxes is not justified (see also point 7 above) Pag3531

AR: Our argument here is that the effect of the coastal winds due to its strong spatial variability (leeward of the mountains), is larger than the effects induced by the heat flux gradients, characteristic of these latitudes. Thus using COADS heat fluxes becomes less critical when compared to not including coastal orographic variability in the wind signal (i.e. resolution dependent).

15) L5: 5km!! Not even 2 grid-points. Submesoscale features are impossible to be resolved with this configuration. I suggest either to remove the suffix "sub" everywhere or to increase the resolution of the ROMS configuration.

AR: Valid concern! We will try to calculate a higher resolution spatial grid, in the attempt to resolve submesoscale features; otherwise we will concentrate the discussion solely on mesoscale dynamics.

16) L24: Fig5b or 5a? AR: Fig. 5a =>2009; 5b => 2010

17) L24-26: I do not agree with what is stated here. It looks to me that there are big differences between observations and model (southern part much warmer, sometime differences in the order of 3 degC). I am wondering how reliable is the current configuration...

AR: We are referring to the representation of the (right) temperature gradients in the right regions, rather than absolute temperature values. Hard to represent absolute SST values without coupled atmospheric-ocean models and/or including data assimilation.

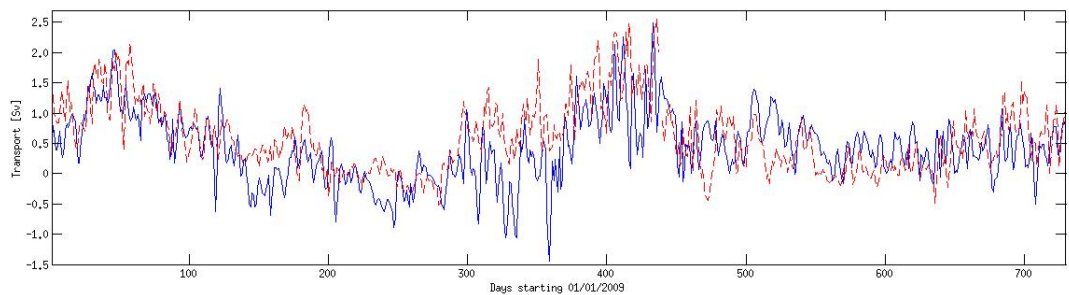
18) L27: The comparison with the current measurements in the Corsica Channel is fine but I do not see a logical link with the primary focus of the paper, i.e. an

analysis of eddy populations. Frequency distributions of eddy SSH look to me more appropriate (see point 11 above).
Pag3532

AR: No numerical study can be fully validated only partially validated. Considering that the freely available satellite data does not adequately resolve for mesoscale eddy activity; considering that there is very little data freely available for the region, it is important (in our view) to show that in critical regions such as the Corsica Channel the model is capturing realistic dynamics including the inter-annual variability (2009-2010).

19) L5: The comparison with the current measurements in the Corsica Channel should be more quantitative. How are the averages and standard deviations from observations compared to model? Can the author show a vertical section of the model temperature and salinity field at the Corsica Channel? Can they also put it together side by side with the observed one?

AR: Attached we include a copy of the edited figure:



Some statistics will be included and discussed in the revised version:

Observation mean: 0.59 Sv
Observation standard deviation: 0.57 Sv

Simulated mean: 0.43 Sv
Simulated standard deviation: 0.56 Sv

We are making efforts to find and include further data-model comparisons in the revised version of the ms.

20) L7: Fig6, in the central period, observations show a period of weak reverse which is not seen by the model. Can the authors explain why?

21) L7: Fig6, please remove 10^6 in the figure as Sverdrups are indicated in the label.

AR: Done

22) L22: Fig7a or 7b?

23) L23-25: ROMS is a sigma-level model. How did the authors obtain the fields at 10m (or at other depths)? Did they interpolate? How?

AR: Converting ROMS's sigma solutions to z-layers, thus enabling the analysis of specific depths.

24) L27-28: To what exactly these 10 Sv refer to? Did the authors calculate the barotropic streamfunctions? Until which depth? Sverdrup transport? How? Did the authors extract the non-divergent part? Please be precise and specific.
Pag3534

AR: Our method for computing the Sverdrup streamfunction, corrected by the island rule is derived from: Wajsowicz, R. C., 1993: The circulation of the depth-integrated flow around an island with application to the Indonesian Throughflow. J. Phys. Oceanogr., 23, 1470–1484.

More details will be added to the revised ms.

25) L7: typo, slightly AR => Taken care in revised version!

26) L22-29: this part is speculative and belongs to discussions.
Pag3535 and 3536 AR => Taken care in revised version!

27) I am confused

...

These theoretical considerations are more pertinent to the Mediterranean circulation. What is the link with the focus of the paper? What is the link with the differences between eddy populations in the two basins?
Pag3537

AR: Pedlosky et al (1997) 'sliced-cylinder model' discussion is pertinent to interpret basin-scale circulation, which has 'islands' and 'boundary-currents'. Such simplified model can be pertinent to interpret the Mediterranean Basin-scale as well as circulation around the Ligurian and Tyrrhenian basins, considering the islands of Corsica and Sardinia. The findings of the case studied by Pedlosky are similar to our numerical results. Pedlosky et al. (1997) study has also considered the effect of asymmetric wind stress, which is analogous to the Mistral wind effects (in the Gulf of Lion), has in the whole basin.

28) L2: basins is repeated twice AR => Taken care in revised version!

29) L3-10 and Fig11, What exactly the authors want to show in these lines? Again, what is the link with the focus of the paper? Confusing.

30) L3-10 and Fig11, How are these spectra obtained? Which function has been used to calculate the spectral density? Are these averages? Again, please be precise and specific

31) L5: I do not see this. It looks to me that the shift from k^{-3} to $k^{-5/3}$ happens at the same scales for both lines!!

32) L3-10 and Fig11, how are the spectra for the MERCATOR products? Can they be used to compare Ligurian and Tyrrhenian basins at coarse resolutions and test what the authors are saying?

AR: The discussion and interpretation of the energy spectra will be reformulated.

33) L11-12 and Fig12, can the authors point out the similarities between these figures and the ones obtained by Pedlosky et al (1997)? For example, they could indicate with arrows every gyre they are citing in the text.
Pag3538

AR => We will be addressed in the revised version!

34) L7: typo, though Pag3539 **AR => Taken care in revised version!**

35) L24: yes, “partially-validated” and needs a better validation as explained in points 11, 17, 18 and 19 above

AR: No numerical study can be fully validated only partially validated. Nevertheless, we feel that our provocative results should be reported in order to push forward the need to collect data at appropriate scales. On the other hand, with the data and technology available to us is virtually impossible to monitor basin scale eddy dynamics in the region, thus numerical studies can also contribute to push forward future observational efforts.

Anonymous Referee #2

Received and published: 7 January 2013

This study aims at studying the role of eddies in the Ligurian-Provencal and Tyrrhenian basins, with a special interest on the comparison between eddy populations in both basins. The study relies on a few satellite data, numerical outputs from two different models (operational MERCATOR product and a ROMS configuration) as well as several inputs from other related articles. Significant differences are found concerning the eddy distribution, dynamics and life-time when both basins are compared. An additional discussion part shows interesting similarities between some previous theoretical work and the present realistic study. A final section aims at discussing the eventual impact of such asymmetric dynamics on the biology.

The authors would like to acknowledge the valid points raised by the referee-2. In order to distinguish our answers from the reviewer's comments the author's responses are labeled as 'AR'.

My general comments are:

- The English does not reach the suited general standards for such publication, this is not the case for all sections but for a too significant number of the important parts. In addition, too many sentences are incorrect or inappropriate or badly phrased, leading to confusion or misunderstanding. References to some figures seem to be sometimes wrong.

AR: Professionals will carry a prof-read of the revised version of the ms. before re-submission.

- Most of the figures are too small, do not feature any units and sometimes are not even recalled in the main text ? Some could be merged in order to avoid confusion, and some could be deleted as they do not bring extra-clarity to the text.

AR: Figures will be carefully revised in order to improve clarity and warrantee adequate citation in the text.

- The part concerning the model validation is incomplete as from the start few information are missing concerning the model configuration itself. This part is also not clearly emphasized when compared to the most important part: the eddy activity in the 2 basins.

AR: We will provide more detailed numerical model setup description in the revised version of the ms. Regarding validation it is hard to validate the numerical results further since freely accessible satellite data under-detects mesoscale features in the Mediterranean. Nevertheless, we feel that the current (provocative) study should not be seen as conclusive but as an attempt to stimulate further observations.

- The comparison between MERCATOR and ROMS simulation is not relevant, especially when considering the major differences of set-up between the NEMO Z-level and ROMS sigma level configurations. This should be either less discussed and used because of the strong differences or more developed in order to bring relevant pieces of information for discussion.

AR: We agree! The discussion and/or figures associated with MERCATOR results do not significantly contribute to the eddy-distribution discussion thus it will reduce this discussion from the upcoming revised version of the ms.

- the theoretical part is very promising. However, as this could be one major interest of your paper, it should be much improved, in terms of accuracy and development. - The final part on the biological impact is interesting but very light! It should then either be developed or proposed as an extra discussion in the conclusion.

AR: We will expand the theoretical part of our discussion. We will also consider expanding the biological impact section or moving it to a general Discussion Conclusions section.

For all these reasons, I suggest a process of MAJOR REVISION. Generally speaking this paper features promising ideas but it should be re-targeted to the most relevant issues the authors want to raise in order to reach publication standards. The general unfinished presentation (English, figures, sections organization) strongly penalizes the overall impression of this paper.

AR: A MAJOR revision is in progress for re-submission a.s.a.p.

Some specific concerns are reported below, page by page.

P3523: L17 : "Hard" -> difficult. And what about HF radar ? shouldn't you mention them ? L18: is is however more and more sampled ! MOOSE for example ?

AR: To the best of our knowledgeable HF-radar data and/or SAR – Synthetic Aperture Data is not freely accessible for the region! Nevertheless, future analysis of these and/or new datasets should take into account our numerical results. Future in situ campaigns, airborne campaigns and/or HF antenna positioning could also try to take into account our numerical findings. This study does not pretend to be a conclusive analysis of basin scale eddy dynamics but a provocative discussion suggesting that new scales and approaches need to be addressed in future research.

Notwithstanding, the authors continue to work hard on trying to access more data to help validate the current model solutions. Any suggestions of freely available data and/or potential collaborations are always welcome!

P3524: L10 to the end: several sentences need to be rephrased. P3525: L1: lodge
....develop

L15 to 26: does this part deserve to be that long...I don't think so.

AR => We will address this in the revised version!

P3526: Misspelling : L1 : temperature AR => Corrected in revised version!

P3527: L2: verb is missing to get a clearer sentence. AR => Corrected in revised version!

L7 : as you talk about Boussinesq, what about hydrostatic approx, non-divergence, etc..

AR: ROMS uses hydrostatic primitive equations for momentum. In the Boussinesq approximation, density variations are neglected in the momentum equations except in their contribution to the buoyancy force in the vertical momentum equation (i.e. hydrostatic approximation). Under the hydrostatic approximation, it is further assumed that the vertical pressure gradient balances the buoyancy force:

$$\frac{\partial \phi}{\partial z} = - \frac{\rho g}{\rho_o}$$

Considering that ROMS is so widely used, primitive equations are detailed in the classical literature (e.g. Song, Y. and D. B. Haidvogel, 1994: A semi-implicit ocean circulation model using a generalized topography-following coordinate system, *J. Comp. Phys.*, **115** (1), 228-244).

ROMS numerical algorithms are also well detailed (e.g. Shchepetkin, A. F., and J. C. McWilliams, 2003: A method for computing horizontal pressure-gradient force in an oceanic model with a nonaligned vertical coordinate, *J. Geophys. Res.*, **108** (C3), 3090, doi:10.1029/2001JC001047.

and

Shchepetkin, A. F., and J. C. McWilliams, 2005: The Regional Ocean Modeling System: A split-explicit, free-surface, topography following coordinates ocean model, *Ocean Modelling*, **9**, 347-404.).

L12: "was 1/32 is (3km)" : this is confused English. AR => Corrected in revised version!

P3528: L6 to 7 : rephrase ! "...are needed to correctly reproduce .." would be more suitable. AR => Corrected in revised version!

P3529: L1 to 2 : English

L6 ...: KE is computed with AVISO, but AVISO only gives geostrophic velocities . This should be explained to enhance the comparison validity between models and satellite data !

AR: Maps of simulated surface KE computed from model's surface elevation derived geostrophic velocities have been interpolated on a $1/8^\circ$ resolution grid in order to be compared with AVISO maps.

L14-16: is this information relevant ?

L22 to 26 : 1) one has to define the MLD criteria (therefore the one used in your ROMS version)

AR: The MLD, in this work, is defined within the ROMS K-profile parameterization (KPP, Large et al., 1994). With KPP the MLD is defined as the depth d at which the bulk Richardson number, which measures the ratio between stratification and shearing is equals to a prescribed critical value:

$$Ri_b(d) = \frac{[B_r - B(d)]d}{|V_r - V(d)|^2 + V_t^2(d)} = Ri_{cr} = 0.3$$

Where B_r and V_r are estimates of the average buoyancy and velocity respectively, and V_t is the turbulent velocity shear.

2) What is the order of magnitude ? your figure does not have a very appropriate color scale ?

AR: Figures will be carefully revised

P3530: L3 : "in front" "along" the Gulf of Lions shelf slope is more accurate.

L4 : "intense " ...Please find another word.

L22- 24 : already said previously . Pick one location to put this information.

P3531: L12: "Also" is not the correct word to start this sentence. Rephrase.

L26 : but as you can see the SST along the Gulf coast is in good agreement which means that mixing and atmospheric coupling is good for shallow waters with your configuration but you cannot say it is as good along the shelf slope. This was the sort of thing I would have expected to see in the text. For this part, you do not mention figure 5a, so is it necessary or is it a mistake ?

AR => It will be addressed in the revised version!

P3532: L1 : they are roughly corroborated ! Some are even opposite in direction !! agree there is an overall good agreement, but your sentence is way too inaccurate .
L22: denser -> a more important, as well as -> and L24 : at -> along L27: on->in L28:
10 Sv : where did you get the figure ?

AR => It will be addressed in the revised version!

P3533: L1 : anyway, how many levels do you usually have between 0 and 10 m with ROMS ?

AR: Between 0-10m we have (in z coordinates): -1.1152m; -6.5675m; -8.6351m; and -10.9116m.

L11- 13 : I do not understand this sentence, however it appears important ?
L14- 17: Is it really surprising ? This tendency is expected when resolution is increased.

AR => It will be addressed in the revised version!

L20- ...: and is there not in addition a vertical resolution problem for the mercator grid ? Plus the fact that the sigma and Z vertical grids are expected to produce differences . This should all be much discussed.

AR => It will be addressed in the revised version!

P3534: L5 ...: The figure 10 is not clear.
L16 : enlarge -> grow L18 : in horizontally -> for a horizontally..

P3535: L6 : These idealized approaches usually study L8 : as well as to help guiding L15 : became a discussion : strange expression ?? to be changed.

P3536: 24 : "en route" ? Finally : for all the figures : too small, too many , not a single one with the units (!!)

Fig 7A : you have even left the disk address of your figure . And this does not help you as for the lower panel, you say in the caption "500m depth" and the disk address is ../1000m !

AR: Figures will be carefully revised