**Ocean Science
Discussions**

Interactive comment on "Assymmetric eddy populations in adjacent basins – a high resolution numerical study of the Tyrrhenian and Ligurian Seas" *by* R. M. A. Caldeira et al.

Anonymous Referee #1

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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

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Interactive Discussion

Discussion Paper

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.

CONCERNS

Pag3522

1) L15: To what exactly the asymmetry in the title is referred to? The eddy sense of rotation (anticiclonic vs cyclonic)? Why is this not reported in the abstract? 2) L19-20: the transport of what? This is related to point 24 below.

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

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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.

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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 6) L18-19: What was the specific reason to prefer the MERCATOR product to the MFS one? 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. 8) L25, typo, lokal

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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 MER-CATOR 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? 10) L20: Are really "v" and "u" used with the opposite usual meaning?

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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. 12) L18: Fig3b or 3a?

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13) L12-16: not clear. What are the authors referring to? Please rephrase. 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)

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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. 16) L24: Fig5b or 5a? 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... 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).

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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? 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 10ˆ6 in the figure as Sverdrups are indicated in the label. 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? 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.

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25) L7: typo, sightly 26) L22-29: this part is speculative and belongs to discussions.

Pag3535 and 3536

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?

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28) L2: basins is repeated twice 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? 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.

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34) L7: typo, though

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

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Interactive comment on Ocean Sci. Discuss., 9, 3521, 2012.