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Interactive comment on "Mesoscale variability in the Arabian Sea from HYCOM model results and observations: impact on the Persian Gulf Water path" *by* P. L'Hégaret et al.

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We thank the referees for their in depth analysis of our paper. It was made clear that the manuscript was lacking several major informations, sometimes drowned in other informations on the region. Furthermore, the EOF metohd was not fitted to an inter-annual analysis. To satisfy the referees requests new analyses and methods are provided and the structure of the paper is modified.
The regional features, the water mass properties and the surface circulation, previously described is shortened and presented in the introduction. The data and method section also add the model description, its boundary conditions, limitations and

validation, with comparisons of the mesoscale dynamics and water mass properties between the model output and the measurements.
Then three parts describe our main objectives.
First, describing and quantifying the mesoscale features of the region (eddies, coastal currents and Rossby waves). Their formations, 3-D structures, evolutions and seasonality are presented here. EOF analysis are deleted from this version of the paper and new tools are used (Fourier decompostion, bi-monthly means).
Second, we study the mutual interactions of these features and their interactions with topography and, atmospheric fluxes in the HYCOM model. To study specific processes we use a simple 2D spectral model. In particular, processes studied here are : Eddy merging along the coast; interaction of a Rossby wave with the topography; interaction of a Rossby wave with a dipole of eddies; impact of the wind and heat flux on eddies.
Third, we focus on the impact of these mesoscale structures on the Persian Gulf outflow, focusing on the Sea of Oman, where the dense outflow equilibrates and mixes with surrounding waters. We remove the EOF analysis, studying instead the seasonal patterns of the outflow. More diagnoses on transports, dilution and stirring/mixing processes are presented, in order to quantify the impact of these mesoscale structures.
We hope this changes make the manuscript clearer.

1 Anonymous Referee #1

Received and published: 15 April 2015

This manuscript describes a single regional mesoscale-resolving simulation of the Sea of Oman and Arabian Sea region, carried out for 11 years, with repeating 2011 forcing. The results for the last 6 years of simulation are compared with observations, and anal- ysis focuses on the mesoscale eddies and the Persian Gulf outflow water. There are many interesting features in the results which warrant publication. However, the analysis, particularly of the Persian gulf outflow water, and its modification by the eddies, could be much more thorough and illuminating. I therefore recommend major revision, as detailed below.

1.1 Major recommendations

1. Connect properties of Persian Gulf water to mesoscale eddy activity
In the conclusions, (p515, lines 6-13), it is stated that the characteristics of the PGW are modified by the eddy behaviour. However, in section 5, little direct evidence of the effect of eddy activity on the PGW properties is actually given. The main evidence shown is for the different pathways of PGW in different seasons, which is a function of different mesoscale circulations. Can you show evidence for fragmentation of PGW by mesoscale eddies, or the different evolution of PGW inside an eddy core as compared to an eddy filament? For example, what is the thermohaline signature of the eddy merger events described in section 4 at the PGW level?
Indeed, diagnoses on the impact of the mesoscale eddies on the PGW outflow characteristics were left unclear in the earlier version of the manuscript. We added here correlation between the deformation field, dominated by the eddies, and the gradient of salinity at PGW depth. The fragmentation of the PGW outflow is mainly observed in the Sea of Oman, we add section of ARGO floats showing the different submesoscale structures observed in this region. We also contrasted the evolution of PGW between the various dynamical structures and the various dynamical evolutions.

2. More details of physical processes acting on PGW outflow

While the authors have shown the depth, thickness and width of the PGW outflow, along with the evolution of the temperature and salinity, there is little effort to determine what this information tells us about the mixing which is taking place, the net entrainment etc. One missing quantity would be the integrated transport across the outflow does this increase when the thickness increases, indicating entrainment? Does the outflow reach a neutral buoyancy level, at which point mesoscale eddy stirring might be expected to dominate over entrainment? Does the PGW detrain from the boundary? Can you identify dilution due to (parameterized) vertical mixing in a downslope flow from mixing associated with the mesoscale eddy fragmentation processes?
Indeed, the calculation of the transport was lacking. We calculated the transport pf PGW and of neighbouring water masses in the Sea of Oman across various sections; we thus try to evaluate eddy stirring and coastal entrainment over topography. It also allows us to explain the change of the geometrical characteristics of the outflow. Now it must also be remembered that the model resolution does not allow the detailed analysis of fine scale processes.

3. More details of the numerical simulation
Several important details of the numerical simulation were missing. What is the domain over which the simulation is carried out - is it as shown in figure 1, or is it much larger? What lateral boundary conditions are used? What is the vertical mixing scheme used in the implementation of Hycom? How might this impact the dilution of the PGW outflow?
Figure 1 is changed to show the domain of the numerical simulation. Boundary conditions are Mercator (1/4 of degree) and the vertical mixing scheme is a KPP parametrization. Informations about the model are added to the manuscript for clarity. Since several simulations with different mixing schemes or mixing parameters have not been and cannot be done before the end of the revision we evaluate this impact.

4. Comparison between observations and model
While the model is run with forcing from 2011 cycled 11 times, the observations span the periods 2002 to 2004 (floats) and 1992-2014 (AVISO). In your discussion of the differences between model and observations, I did not notice any consideration of the impact of examining different time-periods. How can you account for any trends which may confuse this comparison between model/data at different time periods?
Indeed, we make our objectives more clear in the introduction of the paper. Our goal here is to describe the seasonal variations of the mesoscale structures and their interannual recurrences. We discuss the time periods of the data.

1.2 Specific concerns

Abstract: p494, line 10-11: "with a higher resolution" - At first I thought you meant that you did some followup simulations at a higher resolution than your initial simulation. But I think you mean higher resolution than possible from the observations - please clarify.
Indeed we meant higher resolution than the observations.

Figure 1: It would be helpful to the reader to label the Sea of Oman in figure 1.
Wa changed the presentation map to include the modeled region from HYCOM and names of the basins.

Section 1, p496, line 6: "this was observed" - please include citations for statements like this.
We changed this formulation and cited a previous paper.

Section 1, p497, lines 7-8: It is not really correct to label the simulation years 2016 to 2021, when the forcing is always 2011. Instead label them "model year 5-11", so the reader understands you are not referring to actual calendar years 2016-2021.
Indeed, it would be easier to understand.

Section 1, p497, line 13 "better results" - better than what?
We meant better description of the basins (such as the Sea of Oman) than other climatologies (Levitus for example). We modifed thsi sentence.

Section 1, p497, line 25-26: The sentence "Nevertheless, the calculations of derivatives, as the wind stress can still present strong horizontal gradients" does not make grammatical sense.
Indeed, and the information is not important, we deleted it.

Section 1, p498, line 1: "Fonction's" should be "Functions".

Section 3.2, p499, line 2: The acronym "MADT" is used for the AVISO altimetric data what is it an abbreviation for?
MADT means "Mean Absolute Dynamic Topography.

Section 3.2 p499, line 3, and elsewhere: When you use "anomaly" in this context, what is it an anomaly relative to? Is it relative to the time-mean over the entire measurement period? Or relative to the spatial mean? Please clarify.
Indeed, thank you for this remark, here, contrary to the Sea Level Anomaly, the anomaly we study here is relative to the spatial mean.

Section 3.2, p500, line 15-17: The third EOF in the Hycom simulation looks very different from that in the observations - can you speculate why?
The EOF analysis was deleted from this manuscript. The difference between the two EOFS lay mainly in the Gulf Of Khambhat, where the river output strongly varies from one year to another. This variability is not present in the model, looping on the 2011 forcing.

Section 3.3, p500, line 25-27: Can you show the two different regions over which you examine the water mass properties on the map? (e.g. Figure 1)
We implemented this correction.

Section 3.3, p501: When discussing the comparison between model and obs, please make it clear when you are talking about the observations, and when you are talking about the model. e.g. line 2 "Several profiles present fresher water" - this is in the observations; line 26 "the stronger salinities that are found in the northwestern part of the basin" - this is in the model results.
We changed the text to make it clearer.

Section 4, p502, line 15-16: Give the Rayleigh Kuo criterion in terms of the two quantities evaluated, to refresh the reader's memory.
We implemented this correction.

Section 4.1, p504, line 14: "The Rossby waves deform over the sea mount" - the seamount is not marked in figure 9, so it is hard for the reader to determine if this is true. I suggest showing the location of the sea mount on figure 9a.
We implemented this correction.

Section 4.3.1, p506, line 1-2: Rewrite this sentence as "Two eddies of the same polarity can merge if they approach close enough that the distance between their centers is smaller than 4 times their radius."
We corrected this sentence.

Section 4.3.1, p506, line 8: Delete "and".
We implemented this correction.

Section 4.3.2, p508, line 23-27, and elsewhere: Replace "slope" or "slopes" (which imply a spatial gradient) with "tendency" or "tendencies" (which imply a temporal gradient).
We implemented this correction.

Section 4.3.3, p509, line 14: "have" should be "has".
We implemented this correction.

Section 5.1, p511, line 22: "a PGW" should be "of PGW".
We implemented this correction.

Section 5.1, p511, line 26: "equilibirum" should be "equilibrium".
We implemented this correction.

Section 5.1, p511, line 27: Add a comma after "salinity".
We implemented this correction.

Section 5.1, p512, line 8: Add a comma after "56 E".

We implemented this correction.

Section 5.1, p512, line 9: Insert "an" before "anticyclonic lee eddy".
We implemented this correction.

Section 5.2, p513, line 17: Are decreases in thickness of the outflow associated with detrainment (e.g. fluid at its neutral buoyancy level is mixed into the ambient fluid) or due to widening or acceleration?

Section 6, p515, line 6: Delete "on" before "its pathway".
We implemented this correction.

Figure 3: Mention that these fields are for 2011. Include " and left to right" after "top to bottom".

Figure 4: Instead of "MADT anomaly" use the name of the physical variable (e.g. "dynamic topography anomaly" ?). Is the x-axis of the amplitude plots time in months? Label axes.

Figure 7: Is this observations or model?

Figure 8, especially at right: black contour lines are too thick, obscuring the color signal, even when the figure is expanded 200%.

Figure 9: Show the sea mount location in panel (a). All panels are lacking axes labels. What is the contour spacing?

Figure 11: The interpolation in the salinity section looks peculiar. The density section goes down to 1000m, not 1200m.

Figure 15: Again, change "slope" to "tendency".

Figure 16 and 17: Is the whole domain shown? If not, indicate the full size of the model domain. Specify the model used in the caption (ROMS?).

Figure 19: Again, is the x-axis time (units?) on the amplitude plots?

Figure 20 and 21: Are these quantities calculated for each of the 4 months averaged over the 6 years of analysis? How different do the different years look?

Figure 21: Add outflow transport to this figure. Also, instead of plotting temperature and salinity on the same scale (which leaves most of the panel blank, and puts the interesting signal at the top or bottom) use different axes for each field, so that the signal takes up the maximum space. I found it very difficult to see any significant difference in the end point salinity or temperature in these plots.

2 Anonymous Referee #2

Received and published: 27 April 2015

In this paper the authors try to describe characteristics of mesoscale variability in the Arabian Sea and to reveal their role in the spreading pathway of Persian Gulf Water by utilizing observational datasets and a numerical model. In my opinion, there are three critical deficiencies in the current manuscript, as listed below. The modeling itself looks successful, and additional analyses of the existing modeling results could make the manuscript suitable for publication in Ocean Science. However, the necessary revision is very extensive and would make the manuscript totally different from the current version. Therefore, I recommend rejection of this manuscript at this point and encourage resubmission after thorough rewriting.

1. Information is critically lacking in the setup and basic performance of the utilized numerical model. Model domain is not described, nor is the treatment for open lateral boundaries. In particular, it is not described how PGW formation is modeled (if the Persian Gulf is included in the model domain) or how its water mass property and formation rate are imposed (if the Persian Gulf is not included). The authors should also note how stable the analyzed six-year period is. Isn't there any noticeable inter-annual variation or trend (or model drift) in the modeled mesoscale features and PGW transport?
We provide more information on the model in the section "data and method". We compare here model and data and the stability of the model. The two basins of evaporation (Persian Gulf and Red Sea) are modeled, lateral boundaries are forced by MERCATOR. The PGW formation is therefore not imposed, nor parameterized. The atmosphere fluxes drive this formation.

2. The authors conduct EOF analysis in trying to describe mesoscale features and their seasonal variation in the Arabian Sea. However, I don't think EOF analysis is suitable for describing characteristics of moving disturbances. Associated with this point, I wonder how long the data interval used in the EOF analysis is. Judging from

Fig.4, I guess the interval is monthly. But I think the monthly data interval is too long to capture mesoscale eddies, whose evolution time scale is well shorter than that as the authors describe in Section 4.
Indeed, the annual analysis via the EOF is too long. The aim of this analysis was to output the seasonal cycle in this region, under the influence of the monsoons. We instead use time series of the altimetric signal to underline the different seasons and monthly or bi-monthly averages to show the structures that occur during these periods.

3. The EOF analysis is also used to show the pathway of PGW, but I'm in doubt about the validity of using this method, again. EOF analysis well depicts temporal oscillations of standing (non-moving) spatial patterns but is not suitable for tracking moving signals. Furthermore, the dynamical role of mesoscale eddies in transporting PGW is not clarified. The authors simply describe distribution of PGW simulated in the model. Its relationship with the eddy properties investigated in Section 4 is not discussed at all.
We add diagnoses of the impact of the eddies on the outflow characteristics, using calculations of the transport (principally in the Sea of Oman) and correlations between the gradient of temperature and salinity at PGW level and the deformation field at mesoscale. EOF are removed for PGW analysis, as in section 3 and we provide the seasonal patterns of the outflow.