Comment at Refeer Comment #3

Thank you for your review; it highlights a lot of points to be added or elaborated for a significant improvement of the paper, and to make the article more understandable. Thank you for all the notes.

I think the paper needs a major revision, involving possibly new experiments, before it can be considered for possible publication.

The paper deals with the question of 'targeting' observations to improve the analysis and, as a consequence, the ensuing forecast of the oceanographic flow. The authors choose to perform the targeting on the basis of the dominant Singular Vectors (SVs) of the flow, which are the perturbations that, in the tangent linear approximation, grow most rapidly according to a given norm and over a given period of time. This approach is classical, and has been used for a number of applications, more meteorological actually than oceanographical.

We need to be more precise in describing the goal of this work: what our strategy aims at is slightly different than exactly the classical targeting observations. At our knowledge the goal of targeting observations is to set additional observations to an already existing observation network in order to improve the accuracy in reproducing specific circulation features in a particular area. In oceanographic studies most of these additional observations come out from some moving measurement tool, such as gliders. Instead, our strategy aims at identifying the fixed positions where to install a given number of instruments, such as mooring buoys, in order to give the best benefits for DA. In this case, the model solution starts from the climatological state and it is very far from the real solution.

The experiments are performed with synthetic observations extracted from a relatively simple model (Double Gyre, DG) of the circulation in a midlatitude oceanic basin. The authors obtain results which seem of interest, particularly as concerns the need to avoid redundancy that could result from concentrating too many observations in the most active regions of the flow.

But I consider the paper must be substantially improved on two aspects before it can even be considered for publication.

1. The first aspect is relative to the dynamics that underlie the experiments. The authors write that they have performed assimilations, and computed the ensuing forecasts, over five-day temporal windows. That is very short in comparison to the typical time scales of oceanic variability, which are of the order of weeks or months. A very surprising fact can also be seen in the right panel of Figure 8. The dominant singular value is larger than 60, which is impossible for a realistic oceanographic perturbation evolved over 5 days (I understand the Singular Vectors are computed over a 5-day period).

   This aspect needs clarification. In any case, a much more detailed description must be given of the singular vectors that are used in the experiments (it is not clear to me if the authors have used only one, or several vectors). This description must include, in addition to the corresponding singular value, the corresponding initial and final structures of the singular vectors, as well as a brief discussion of the physical instability process which is at the origin of their rapid amplification.

   And if, as I think, no significant evolution is observed over 5 days, then new experiments must be performed with longer assimilation windows.

   We choose a relatively short (5 days) temporal windows because this is close to the typical assimilation window used in the models adopted in operational oceanography (7-10 days)

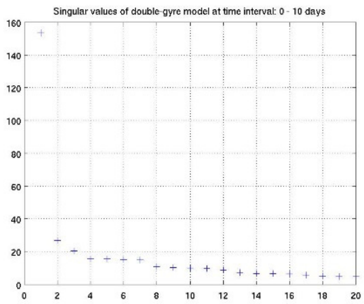
   The strategy was implemented by using the projection onto the horizontal velocity components of all the first 200 initial singular vectors computed with the optimization time (T_op) of 5 days on the free run starting from the climatological solution. The observation positions were identified on the sum, weighed by their corresponding singular value, of the 200 initial singular vectors, although just the first 10 (or even less) would have been enough.

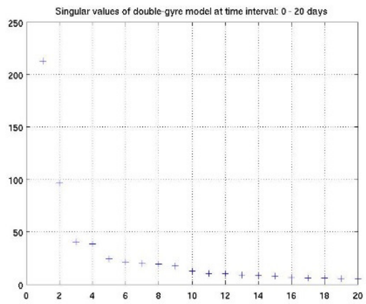
Looking at our background model (sometimes we call it “Free Run” in the paper), as we compute SVs from it, the initial period of simulation is subject to a strong tendency to change the circulation structure, especially close to the convergence area of the two branches of currents where most of the vorticity is originated. The structure of the dominant singular vector when projected onto the horizontal velocity components is in Fig. 8 of the original paper.

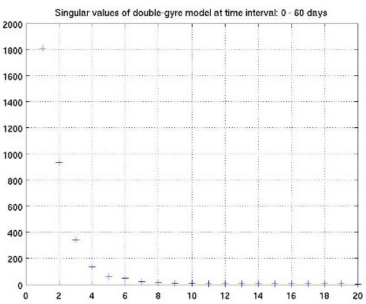
To answer to your questions, we will add a description about the SVD results, with a critical description of the dominant SV at initial and final times,

The structure of the initial and final singular vectors computed with T_op of 5days are similar to each other and they are concentrated where there is convergence of the currents along the western edge, where the two branches of the currents joint to each other and change their direction (from meridional to zonal).

The growth of the singular vectors in the convergence area is quite large. We suppose that the large value of the dominant singular value is due to the configuration of the test: the simulation starts from a climatological condition as initial state (fig.1b), similar to the steady solution but characterized by a much lower viscosity. For this reason the circulation of the background model, especially within the initial period, is subject to strong variations which are concentrated in the area around the central parallel, especially at the western half of the domain.



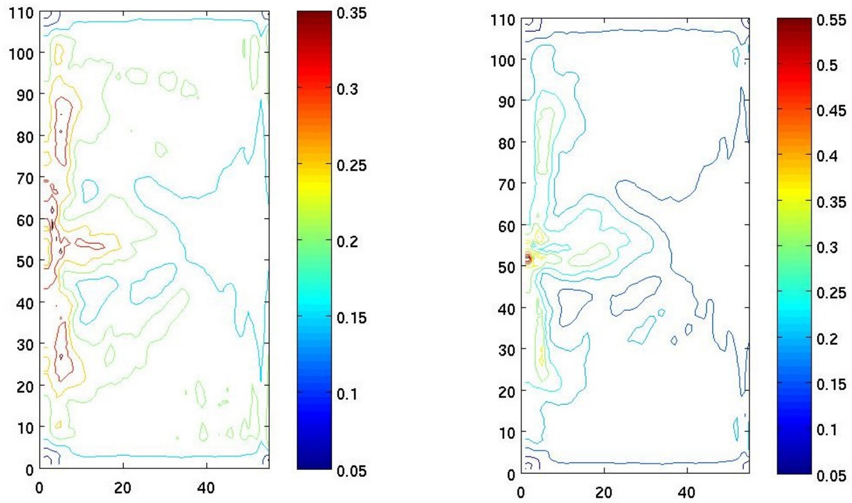




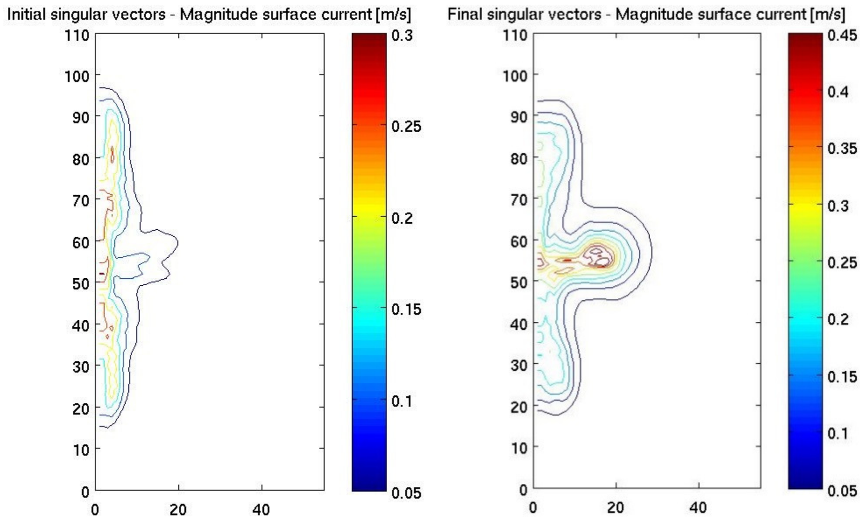
Following your suggestions we performed additional tests.

Below we reported the singular values, and the initial and final maps of the weighted sum of the first 20 singular vectors computed with different, increasing optimization times (T_op = 10 days; 20 days and 60 days). Both the singular values and the difference between the initial and the final maps grow for increasing T_op.

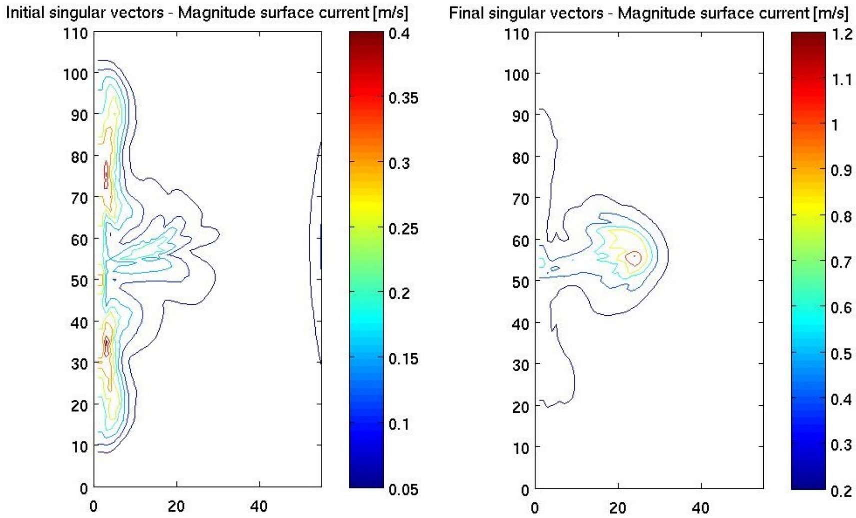
Fig. dominant SV at initial time (op_time=4d) and at final time(op_time=5d):



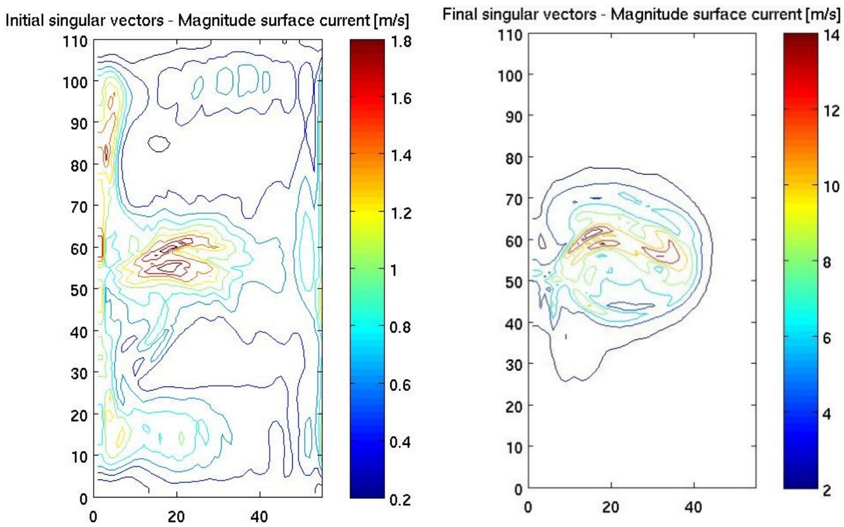
Sv computed within a optimization time equal to 10 days:



Sv computed within a optimization time equal to 20 days:



Sv computed within a optimization time equal to 60 days:



In the figures we reported the weighted sum of the first 20 initial and final singular vectors. As you suggest, the perturbations computed on 5days evolve very little, while those computed on 60 days evolve much more. Anyway the strategy we adopted is based on the initial singular vectors since our goal is to stop these perturbations from growing, and the weighted sum of initial singular vectors computed on different T_op are quite similar to each other. For these reasons, we expect no significant improvement come from adopting a longer T_op.

2. My second point has to with the assimilation themselves. The authors write they have used variational assimilation in association with the DG model ROMS. Variational assimilation consists in minimizing a scalar objective function which usually consists of two terms. First, a background term which measures the misfit between the model fields and a background estimate of the control variable (in the present case, as usually done, the control variable is the model state at the initial time of the assimilation window). And an observation term, which measures the misfit between the model fields and a number observations distributed over the assimilation window.

It is not clear what the background term was in the present case. The authors describe the associated error covariance matrix (subsection 2.2, pp. 7-8), but it does not seem to be clearly said what the background itself was. What is extracted from the Free-Run (FR), or was it climatological?

The background is the 5-days-long free run initialized from climatological values. In all repeated tests on different time windows, the background evolves in the same simulation since the initial conditions are the same, as well as the atmospheric forcing and the boundary conditions (closed). A consequence of this, is that even the SVD computation is the same. Maybe the word "climatological solution" is not correct and may lead in error. We will replace "climatological solution" with climatological state" or values.

And what was its dimension? It must the dimension of the model state, i.e., according to the numbers given 1.36, p-5, 56 x110 x 4 = 37840 times the number of variables per grid point. Assuming 4 for that number, that gives 151360. That is not very different from the value 10^5 given 1.2, p.9 for the number of SVs (the two numbers must of course be equal). Clarification would be useful.

The dimension of the background state is 56 x 110 x 4 (vertical levels) x 5 (prognostic variables) = 24640 x 5 = 123 200= 1,232 x 10^5. The number of the SVs are exactly the same. Indeed the singular values are the eigenvalues of the matrix LL^T (and L^TL, see the section 2.2) whose dimension is N (model state dimensions). During the computation we will not computed all of them; instead we computed the Svectors characterized by the highest Svalues.

We will rephrase the sentences in the section 2.2 to make it clear.

The dimension of the state vector has importance in comparison with number of observations. If the paper is clear as to the spatial distribution of the observations, it is not as to their temporal distribution. Were observations repeated at the same locations over the assimilation window? And with which time frequency? The value of the number of individual scalar observations has some observations has some importance in comparison with the dimension of the state vector, since it gives a first measure of the relative influence of those two components of the data used in the assimilation.

The influence of observations depends also strongly on the weights that they are given in the definition of the objective function. The authors write the observations were taken from the Nature Run without being noised. But which weight were they given in the objective function (giving them a weight is equivalent to assuming that they are affected by errors)?

Yes, we investigated the geographical position in which to install the instruments; their positions are fixed during the assimilation window. The sampling period is 15 minutes.

The temporal distribution is interesting as well but we did not investigate this parameter in the article.

The number of observations used in each assimilation run is the same in all the esperiments and it is 20 (obs tools) x 2 (horizontal velocity components) x 4 (vertical levels) x 5(days) x 24*4 (sampling freq 1/days) = 76800. We will add this information in the text.

For what concerns the weights of the observations in the cost function, you are definitely right: considering an observation error covariance matrix means the observations have errors. We did not add any noise components and the observations are the same extracted from the Nature Run. So the only way by which we have considered the presence of observation errors is through the observation error matrix as we assume only implicitly that observations are affected by error.

Finally, the interpretation of the results is somewhat confusion. Most readers will not be familiar with the Taylor diagram for visualizing results. Is the diagram relative to one particular time over the assimilation window, or is it a form of integral over time? What is exactly the standard deviation plotted in the diagram? And, concerning the correlation coefficient, it is clearly a correlation with the Nature Run (NR). But a correlation over space only, or what? How can the root mean square error read on the diagram (it is not an unambiguously defined function of the standard deviation and the correlation coefficient).

Speaking of the root mean square error, it a simple and standard measure of the quality of assimilations and forecasts, and could be presented in a more explicit format. And it should be associated with an appropriate evaluating scale (for instance the climatological standard deviation of the model fields).

The similarity between the virtual reality (nr) and the others simulations (fr and all the analyses) is quantified by Taylor diagram. This tool describes mutually two or more series of data through their correlation, their centered root-mean-square error (which represents their difference) and their standard deviations (which represent the amplitude of their variations). The Taylor diagram is mostly used in the operational field because it allows to condense the result of the comparison of two sets of data using few statistical parameters (STD, RMSE, correlation). When the comparison is, for instance, between two time series (for example, between the record of a wave buoy and the result of a wave model at one point) the correlation is temporal, etc.

In this case we compare two maps of values at the same time, and therefore we are evaluating a spatial correlation.

These numbers are computed on the surface velocity magnitude field at the final time of assimilation window, in the following way (where the upper-line indicates mean value):

- the standard deviation

$$STD = \sqrt{\frac{\sum\left(vel - \overline{vel}\right)^2}{N}}$$

- the centered root mean square error

$$RMSD = \sqrt{\frac{\sum\left[\left(vel - \overline{vel}\right) - \left(vel_{nr} - \overline{vel}_{nr}\right)\right]^2}{N}}$$

- the correlation

$$Corr = \frac{\sum\left[\left(vel - \overline{vel}\right)\left(vel_{nr} - \overline{vel}_{nr}\right)\right]}{N\ STD\ STD_{nr}}$$

The metric of Taylor's diagram is quite handy since the similarity between two data series can be evaluated by the proximity of their representative points on such diagram.

About the Taylor diagram, there is a short description from page 9, line: 24; we will rewrite it clearer, by reporting the formulas above. We have reported just the similarity between the surface velocity magnitude fields; we could add the similarity of the velocity direction as well; we found the diagrams computed on the two horizontal components u and v were very similar, so we reported just the velocity magnitude. We can compute these statistics on the whole assimilation (and forecast) window instead of just at its final time.

The above are only a number of points that should be corrected concerning the description of the assimilations presented in the paper and the evaluation of the results they produce.