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.

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.

2. My second point has to with the assimilations 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 ?

And what was its dimension ? It must the dimension of the model state, *i.e.*, according to the numbers given l. 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 l. 2, p.9 for the number of SVs (the two numbers must of course be equal). Clarification would be useful.

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 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) ?

Finally, the interpretation of the results is somewhat confusing. 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 on 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 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.