Ocean Science
Discussions

Interactive comment on "A simple predictive model for the eddy propagation trajectory in the South China Sea" *by* Jiaxun Li et al.

Anonymous Referee #2

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The submitted work proposes a regression model to forecast the trajectories of eddies in the South China Sea. The method is based on using the velocity field obtained through the Maximum Cross Correlation technique applied to sea level anomalies as capturing the combination of several dynamical components (self-propagating beta effect, advection by mean flow, etc..). The basic dynamical idea of applying the MCC to altimetry to analyze trajectories of eddies was introduced several years ago (e.g. L.L. Fu JGR, 2006). The novelty here is to go a step forward to develop a linear regression model to forecast such trajectories and assuming some dynamical elements affecting eddies propagation. The authors compare their approach against forecasting using a "persistence" approach.

From my point of view, there are not great concerns on the scientific content of the

paper. The authors discuss quite adequately the main assumptions leaving for future work potential refinements of their methodology. However, the major drawback when one is trying to provide a forecast method is to analyze with major detail the robustness in the choice of parameters. There are some aspects the authors should present more carefully:

* The different choices of window search and time lag. The authors indicate the upper sizes of such values for the mesoscale in SCS (lines 107-109) but some quantitative indications on how it affects the results is necessary.

* The regression coefficients are computed over a limited temporal interval (1992-2008). The authors should analyze the stability of these coefficients as a function of the chosen time interval and how the results depend on it.

* The regression model introduces a climatological term U_CLIM estimated from the MCC. How this climatology is built remains unclear? Is a mean over the same regression period (weekly, monthly, seasonal, ...)? How it depends on such climatology?

Such questions should be clarified in a quantitative way.

Finally, the structure of the paper is decompensated with a central section ("Results") that mixes the methodological approach, the results and discussions. The "Data and Methods" section (section 2) include two subsections devoted to present datasets and to explain the MCC respectively, while the forecasting model is presented in detail in subsection 3.2 "Model Development" as part of the Results section (section 3). MCC is relevant for the system they propose but is just one of the elements of their methodology and is a well-known classical method in the context of the oceanography. Thus my suggestion is that the regression model should immediately follow the MCC description, both elements more coherently integrated in the "Data and methods" section and leaving the Results section to show the performance of the forecasting system.

Apart from these considerations I have the following list of small comments:

Section 2.2 and 3.1 (but also in the whole manuscript): In the text there is an abuse of the "eddy" word. Sometimes "eddy" is used in the context of deviation respect to a statistical mean while sometimes is used to refer to a dynamical coherent structure (an eddy). The MCC velocity field as applied to SLA maps is not only representative of the evolution of coherent eddies but also include many other structures as waves, filaments, fronts, etc. that may also evolve and propagate. Thus the velocity field are not necessarily the velocity of eddies understood as coherent structures alone. This must be mentioned and a careful use of the word "eddy" along the whole manuscript should be checked to avoid misinterpretations.

Section 3.1 (fig. 2b, e and lines148-165): Perhaps I'm wrong but I don't appreciate much differences between the winter and summer distributions of the phase speed of the first baroclinic Rossby wave. I may suppose that it is because at such latitudes the seasonal stratification does not change too much? A small comment may guide the general readers.

Section 3.2 Model Description. It is needed to introduce the opportune equations representing the linear regression model with the variables involved besides of listing them in table 2.

Section 3.2: If I have understood, the MCC fields introduced into U_CLIM and V_CLIM are the characteristic mean from the whole altimetric period computed at intervals of 1 week? Please may you clarify it?. See the remark above.

Section 3.2: The initial step in the forecasting procedure is to provide an initial starting point of a given eddy. How is this provided, manually upon a first visual inspection of maps or using some method to automatically identify coherent structures in SLA maps? Please precise.

Line 207: How the predictands and predictors are normalized? please explain.

Line 217: "There are a total of 8 regression equations...", please see the comments

above.

Lines 232 and following and Table 3: The table caption and the description of the parameters listed in the table are not enough detailed. Why the RMSE is given in degrees? The use of parentheses in the table whether they mean latitudes, persistence or predicted may somehow confuse the reader. Please clarify it and try to make a more detailed description in the table caption which is extremely synthetic. An interesting way of presenting the differences between the persistence method and the proposed method could be to normalize distances with the Rossby radius o grid size in order to see if their differences are relevant or not.

Summary and Discussion: The authors only test the performance on seasonality and polarity but perhaps other processes as dissipation, merging or splitting which can be quite common and linked to eddy dynamical parameters as for example vorticity may affect the performance. Some comments or discussions on that should be welcome in this section but some examples on how the forecast are in such cases could also be illustrated.

Figures:

In fig 1 the subplot C is part of the results and I recommend to move it to the results section.

Fig 2 is very small in size and hard to appreciate the velocity fields.