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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A data assimilative perspective of oceanic mesoscale eddy evolution during VOCALS-REx" *by* A. C. Subramanian et al.

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Interactive Comment

Response to Reviewer Comments

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29 November 2012

Response to Interactive comment on "A data assimilative perspective of oceanic mesoscale eddy evolution during VOCALS-REx" by A. C. Subramanian et al.

We would like to thank the referee 2 for the very constructive and thorough review of our manuscript. The comments and suggestions have helped greatly to improve this manuscript.

Q1: *I find the assimilation exercise very interesting and the results worthy of publication. However, in its present form, the paper fails to convey the "data assimilation perspective": more discussion is required to clarify the role of data assimilation and of the different data sources besides showing reduction in data misfit. Differences between analyses and no assimilation experiments would help a lot, including heat budget estimates with and without assimilation. Comparing results with data not assimilated by the model also indicates assimilation impact, comparing analyses results with assimilated data is not very informative unless the method is not capable to fit some data.*

A1: We agree with the reviewer and have included plots of unadjusted forcing fields and adjusted forcing fields after assimilation in the revised manuscript. We have

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also included this as a supplement file to this response. Since the focus of this study is the description of the ocean state and particularly the structure of the eddy during the cruise, we do not plan to show the impact of assimilation on the analyzed eddy fields. We do compare the model fits to independent SST data, but there are no independent in-situ data for comparison. We also have included a section detailing the data assimilation procedure and the parameters and assumptions required for the analysis to be complete.

Q2: *Given that the purpose of the observational study is to understand air-sea-land interaction processes, it is surprising the authors do not mention anything about how assimilation changes the surface fluxes, which are part of the control variables. This should be addressed/commented.*

A2: We agree with the reviewer and have now included figures of the air-sea fluxes and changes in them due to assimilation.

Q3: *There is no discussion on background error covariance and how it is estimated. There is no mention either of the vertical mixing scheme used in the model. Please fix that.*

A3: We have included a discussion on the assimilation scheme including a discussion on the background error covariance fields and the corresponding assumptions in creating this field. Errors in the forcing are taken from variance maps of QuikSCAT and ECMWF fields. Spatial correlation is estimated from the covariance of the forcing fields. The background error covariances are computed from a long-term model run forced by climatological surface fluxes and lateral boundary conditions. The details are given in the revised manuscript. The vertical mixing scheme used in the model runs is the KPP scheme by Large, McWilliams and Doney (1994).

Q4: *Special attention is given to a particular eddy and its evolution, but figures do*

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not properly indicate eddy location, etc. This should be corrected. Authors say analyses qualitatively reproduce the observations but the purpose of the assimilation is to be more quantitative and go beyond qualitative resemblance. Please explain. How long is data information retained? Is the forecast for the second 15 day period (after assimilating the first 15 days of data) very different from the analysis after assimilating the whole month?

A4: We agree with the reviewer and now indicate the analyzed eddy by an arrow. We have also included a discussion on the forecasting skill of the model for the second period compared to assimilation of data into this time period. The forecast skill of the second 15-day period can be assessed as the initial model misfit for the second fit. The model misfit was indeed smaller than the misfit produced by a forecast for 30 days starting with the initial conditions on Nov 1. We have included a discussion on this in the revised manuscript but do not go into details as it is not very relevant to our analyzes of the eddy structure during this period.

Q5: *Figure 6, for example, does not show substantial corrections in the vertical temperature structure after assimilation. In fact, visually, data misfit appears to be larger on average after assimilation (compare panels d and f in the figure). However, authors suggest a large data misfit reduction (second paragraph page 8). Please explain.*

A5: The surface temperature misfit is reduced, but there are some deeper regions in the profiles where the misfit was increased. The reduction in misfit is further quantified by different variables. The reason the misfit is much lesser than the first half of the cruise is because this assimilation fit started from a better initial condition, which was the ending state of the previous assimilation fit. Hence, the reduction achieved by assimilation further into this system was less.

Q6: *Abstract (lines 9-10) incomplete or incorrect.*

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A6: We have changed this to the cruise period instead of 76^o W , 19^o S.

Q7: *Figure 8, model velocities overlaid??*

A7: We haven't overlaid the velocities in this plot as they are present on the same domain in the previous figure (Fig. 7) over the SSH contours.

Q8: *Beware of caveats regarding the use of Okubo-Weiss for eddy tracking highlighted by Chelton, Schlax, Samelson, Progress in Oceanography (91) 2011.*

A8: We agree with the reviewer. This was only one measure used to confirm eddy activity and not to quantify and compare any fields with one another. We only used this as a measure of the shear versus strain in the flow and have clarified this in the revised manuscript highlighting the caveats as mentioned in Chelton et al (2011).

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