Atmos. Chem. Phys. Discuss., 12, C9945–C9956, 2012
www.atmos-chem-phys-discuss.net/12/C9945/2012/

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.

A. C. Subramanian et al.

acsubram@ucsd.edu

Received and published: 29 November 2012

ACPD

12, C9945–C9956, 2012

Interactive Comment

Response to Reviewer Comments

Aneesh C. Subramanian, A. J. Miller, B. D. Cornuelle, E. Di Lorenzo, R. A. Weller, F. Straneo

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 wish to thank the referee for the very detailed and thorough review of our manuscript. Their comments and suggestions have helped greatly to improve this manuscript.

Q1: *Although the subject may be appealing, this paper is very disappointing. On the point of view of data assimilation, the method is not clearly explained. It is for instance not clear how the in situ data is used and what the error covariances in the initial state and forcing. It is not clear how the forcing is modified by the assimilation procedure.*

A1: We agree with the reviewer that including more details of the assimilation procedure and a discussion of the changes in the forcing fields will improve the manuscript. We have included a section on the data assimilation procedure used for the experiment in the revised manuscript. We also explain the computation of the error covariances used for the assimilation procedure. With regard to the changes in the forcing, we have included new figures in the manuscript showing

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

the changes to the heat fluxes and wind stress due to the assimilation procedure. This reveals large scale adjustments to the heat fluxes and windstress fields. We have also included the same figures as a supplement pdf document to this response.

Q2: *Although the assimilation seems to work in the sense that the misfit between data and model output decrease during the assimilation process, the interpretation of the results is not very informative. For instance, it would have been interesting to compare the structure of the eddies (or mainly the eddy discussed in the paper) with and without data assimilation to show how it is modified.*

A2: This would potentially be true if we had a data-rich field that could be initialized with an objective analysis of the observations, or sequentially initialized with 4DVAR, whereby we could compare "forecast" eddy fields from previous fits with "improved" eddy fields after a new fit. However, we only have one shot at assimilation, so we couldn't do the "before and after" for the eddy field. One might also consider comparing a pure wind/heat flux forced run with an assimilated run, but our focus is on the mesoscale itself not the large-scale atmospherically forced part of the flows. Since 4DVAR fits allow the model to evolve freely after the obtaining the adjusted initial conditions and forcing fields, there is no unphysical forcing during the freely evolving time periods discussed in the paper. Our focus here is simply to quantify and describe the eddy structure during the cruise period in the dynamically consistent framework of 4DVAR.

Q3: *Overall, I am not convinced by the role of the in situ data with respect to the satellite data. The in situ data is relatively scarce and it should be clarified how it is able to improve the model solution.*

A3: We have shown using the relative misfit reductions due to in-situ data and its impact on the deep structure of the isotherms at the observation locations as in figures 3, 5 and 6 of the submitted manuscript. If subsurface observations are left

ACPD

12, C9945–C9956, 2012

out, the fitting procedure fails to generate the proper subsurface structures associated with the in situ hydrography especially in the eddying region of interest.

Q4: *In conclusion, I think the authors do not make a convincing job that their data assimilating simulation is a useful one, in comparison to a more classical ROMS simulation. Besides, the paper focuses mainly on one particular eddy, but all the figures show the entire domain. It is thus very difficult to appreciate the quality of the results. Moreover, the focus hardly reveals any particular information about this eddy.*

A4: We use the fitting procedure to better represent the structure and evolution of the eddy than could have been done with studying observations alone. This is the main objective of the paper. We also discuss other features observed in situ and via satellite in our representation of the flows with the fit. We believe these results are in fact useful.

Q5: *There is no comparison to the recently published literature on mesoscale eddies (see for instance Chaigneau et al., 2012, JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 116, C11025, 16 PP., 2011 doi:10.1029/2011JC007134, on this subject). There is often erroneous citation of published articles, which shows that the authors have not read carefully the literature. A lot of the interpretation is based on a paper which is not published yet (Holte et al., 2012), and which in my opinion will be difficult to publish if it reaches the same conclusions as presented here. What strikes me as a major flaw of the paper are the conclusions on the heat budget. They are wrong in the sense that the authors draw conclusions from a very short simulation (1 month) and compare their results with model diagnostics from long term simulations (e.g. several years in Colas et al., 2011). These results can certainly not be generalized to a long-term, seasonal, heat budget.*

A5: We agree with the reviewer that a short model-data fit cannot be generalized to a longer-term heat budget. But we believe the quasi-instantaneous depiction of the heat budget is a useful result. We have rewritten the discussion of the heat budget analysis to state the limitations of this analysis and the benefits of the model fits in this context. We have also tidied up the references and left out any mention of the submitted manuscript by Holte, instead mentioning his results as "private communication" where essential.

Q6: *Besides, there is no comment on the impact of data assimilation on the heat budget. How are the heat fluxes corrected in the assimilation process? How does it affect the heat budget?*

A6: The assimilation merely optimizes the trajectory of the model evolution to be "close" to the observed variables by adjusting the initial ocean state and the forcing fields. The trajectory of the model evolution itself is the same as a free-running model given the adjusted initial and forcing fields. In so doing, it gives a clear depiction of how the upper ocean thermal structure evolved over this observational time period. The dynamics are consistent for the short, l5 day period fit for the heat budget analysis. Hence, we did not comment on the impact of the data assimilation on the heat budget, since the model trajectory before assimilation is a similar free-running simulation with the unadjusted initial and forcing fields.

Q7: *The use of a data constrained simulation is completely bypassed at the end of the paper. In conclusion, given these remarks, I cannot recommend this paper for publication.*

A7: The model fields analyzed for both the eddy structure and heat budget analysis are from the data constrained simulation. This state estimate is a free forward run of the model given the adjusted initial conditions and forcing fields. Hence, the data constrained forward model run is the one used for the analysis of the ocean state during this period and is not bypassed.

1 Specific Comments

Q8: *P20902,L26 : what do you mean by geometry? Coastline geometry?*

A8: Now clarified in the text.

Q9: *P20903,L4 : waves ? do you mean Rossby waves?*

A9: Yes, we did mean Rossby waves (westward propagating). We have corrected this in the new manuscript.

Q10: *L8 : ocean biology (upwelled, recycled ,..): useless here*

A10: Suggestion accepted. We have removed the parenthesis in this sentence.

Q11: *P20904,L4: the effect of eddies on biology and DMS has unfortunately nothing to do with the Albert et al. paper. Please read it carefully and do not cite it here.*

A11: We agree with the reviewer. This was an oversight and typo as the reference to Albert et al was included initially in our discussion regarding various forcing mechanisms for the chlorophyll variability in the. We have removed this reference from our discussion in the revised manuscript.

Q12: *P20904: Reference to "Holte et al. 2012". The problem is that this paper is not available. "quantify the level of nonlinearity in the system and offer a dynamical view" are very vague terms. Be more specific.*

A12: We agree to remove Holte et al as a reference and only mention their work as personal communication while restricting our reference to their work only when essential. We have also reworded the referred sentence to read as: "We can also quantify the relative strength of strain versus vorticity in the flow field of the system and offer a dynamical view about dominant processes that could have influenced the biogeochemical processes during the campaign."

ACPD

12, C9945–C9956, 2012

Interactive Comment

Q13: *P20904: Mention of "Biogeochemical processes". The coupling with a biogeochemical model would have been interesting. Although it is not done, this aspect is not discussed in the rest of the paper. "Combes et al. "paper: not available! What is this simulation ? Is it realistic ? Is it interannual?? This is not convincing.*

A13: We agree that a study including the biogeochemical model would be very interesting, but it is beyond the scope of this study and would be glad to collaborate with other groups, which plan to work on this. Since the Combes et al paper is still under review, we have chosen to refer to it as "private communication". We have instead included a brief description of the model used to generate the boundary conditions for our assimilation model fits.

Q14: *P20906,L16-17 "a strong Peru-Humbodlt Current system and a vigorous mesoscale eddy field, indicating the suitability of ROMS in this framework." What do you mean by a "strong current system" and a "vigorous mesoscale eddy field"? These terms are very vague and do not provide any information about the model's realism. In my opinion the model should be carefully validated prior to data assimilation. If the model is too far from the observations, the data assimilation will not be efficient.*

A14: We agree with the reviewer and have included a discussion regarding the model's ability to represent the ocean state in this region realistically in the revised manuscript. The ROMS model run forced by climatological winds and heat fluxes produces a mean Peru current velocity of about 0.2 ms-1 which is 20% weaker than the estimated maximum velocity of 0.25 ms-1 as shown in Strub et al (1998). The eddy kinetic energy in the climatological run is 20% to 30% lower than in observations. The ROMS model has also been used previously by Colas et al. (2011) to study the mesoscale dynamics in this region and they show a very good fidelity in the model to reproduce the climatology and the spatial statistics of the flow in this region.

ACPD
12, C9945–C9956, 2012

Q15: *P907, L5: a IS4DVAR fit - please do not use an acronym here and explain. What is the misfit shown in Figure 3? Is it the misfit at initial conditions or is it the misfit of the model and observations at the real dates of the observations? What are the errors in the initial conditions and forcing? Is the wind forcing corrected? What are the a priori errors for the forcing? Are there spatial correlations in the errors? All this should be detailed or at least described in a previously published paper.*

A15: We dropped the acronym as recommended. Misfit is defined as difference between the model and observation at the specific time and location. Initial condition misfit is an unknown, of course, since we are dealing with real data and not an identical twin experiment. Forcing error is also unknown. Adjustments to the wind and heat fluxes are part of the solution for the data assimilation fitting procedure. 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. These results are now described in the paper.

Q16: *Figure 5: it is very difficult to see any change in temperature misfit during the second part of the cruise. A comment here: one interesting experiment would have been to test if the subsurface observations provide any constraint, in comparison with the SSH and SST data. For instance , some of the mesoscale structures seem to compare well with altimetry, and others not. It would be interesting to analyse that aspect of the solution.*

A16: The reason the assimilation fit for the second part of the cruise shows a minimal change in the temperature misfit is because we start from an assimilated initial condition, which is already in balance with the observations for the first fortnight of Nov 2008. Hence, the normalized absolute error (NAE) for the subsurface temperatures corresponding to the model fit for the second fortnight shows low values prior to the fit (which are comparable to the values of the NAE for the first fortnight). This also represents the ability of the data assimilation to improve the forecasting skill for this period by providing improved initial conditions for the second fortnight by assimilating observations from the first fortnight. In experiments when only the surface observations were assimilated, the subsurface ocean was not well constrained and hence, there was a large misfit to the observations below the surface. We do not include the discussion on this, since that would be a separate study on the observation impacts for the various datasets used.

Q17: *Another aspect in Figure 8 that would have been interesting to analyse is to investigate if the data assimilation is able to constrain in some way through the physical constraint the small scales that are not assimilated in SSH and SST.*

A17: This is potentially a good suggestion for another independent evaluation of the assimilation scheme, although there are no high resolution observations to validate the analysis of small scale SSH and SST.

Q18: *P908,L25: Which eddy in Figure 9? This is difficult to locate the eddy on the figure. Please make a zoomed figure on the eddy you are focusing on, and show all velocity vectors on figure.*

A18: We agree with the reviewer and have changed the figures to point to the eddy discussed with an arrow.

Q19: *P20909, line 30: Reference to Echevin et al (2004). This paper does not refer to eddies as it analyses only cross-shore sections. It would be more appropriate to compare the eddy structure with results in Chaigneau et al. JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 116, C11025, 16 PP., 2011 doi:10.1029/2011JC007134 .*

A19: This is a very good suggestion and we have replaced the discussion of Echevin et al (2004)'s work with the discussion and comparison of the model eddy properties to those found in the observations by Chaigneau et al (2011).

ACPD

12, C9945–C9956, 2012

Q20: *P20910: please show the ridge on Figure 9d. I am not aware of any study showing the influence of the Nasca ridge on the eddy activity. This is mainly an hypothesis. Please cite a reference there or prove this by performing a process study.*

A20: We agree with the reviewer that this is merely a hypothesis and would require a further detailed study to understand the influence of the ridge on the eddy dynamics in this region. Hence we have removed this sentence from the manuscript.

Q21: *P20911: Vertical diffusion term: please separate the surface (Qnet) and subsurface entrainment at the bottom of the layer. "Clearly the vertical mixing processes contribute significantly to cooling broad-scale averages of the upper ocean in this region and dominates over the lateral advection effects of the smaller-scale eddies." : I see really no proof of that in this very short (2x15 days) numerical experiments. I do not see how you can con- clude anything about the mean effect of the smaller scale eddies here. Why there is cooling/warming on either side of the eddy near 76?W, 19?S? I also do not understand how canHolte et al (2012) (a paper that is not published yet, and I guess, is not likely to be published very soon if the main message is that "cyclonic and anticyclonic eddies effects on the mean heat budget cancel out") draw any conclusions on the heat budget using data from a short cruise. To conclude , I find it difficult to review this paper without having access to a paper under review, which seems to disagree with the conclusions from previous modelling work focusing on heat transport by eddies (e.g. Colas et al., 2011).*

A21: We agree with the reviewer here and have rewritten the entire discussion on the heat budget analysis of the model runs to state that the model fits are only a quasi-instantaneous heat budget analysis and hence the results on the heat budget analysis cannot be extended to interpret the seasonal or long term upper ocean heat balance in this region. Yet, this result is helpful to understand the

ACPD

12, C9945–C9956, 2012

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

relative importance of the different heat budget terms just for this short period. We acknowledge the fact that to understand the long term heat balance in this region, we would need a long term consistent model simulation or large spatial observations that represent the region's ocean state realistically.

Q22: *P20912: "Areal averages, however, around the eddies or around the cruise tracks suggest that vertical mixing processes generally balance the surface heating." This a very vague statement and I do not understand it. "around the cruise tracks", particularly. Be more specific and prove what you say!*

A22: We have now included a description of the analyses done to quantify the heat budget terms averaged over the eddying regions to qualify this statement. We averaged the four heat budget terms over a box centered on the eddy located at 76o W and 19o S for the period of the cruise and compared the relative magnitude of the terms.

1.1 Comments on Figures

Q23: *Figure 1: is the SSH anomaly a 5 day mean? It would be more interesting to shown how the mesoscale structure propagate westward during the cruise.*

A23: The mesoscale structure does not evolve significantly over the cruise period. We do show the instantaneous snapshots of the eddy field for a representative period of the cruise in figures 7a and 7c.

Q24: *Figure 2: not necessary.*

A24: We choose to disagree with the reviewer and believe that this figure is important to indicate the bathymetry and structure of the modeling domain and give perspective to the study of the region.

ACPD
12, C9945–C9956, 2012

Q25: *Figures 5e,6e: what is the use of this figure? What is the depth of the profiles?? Show the position of the eddy on this plot please.*

A25: These figures are only present to indicate the horizontal location of the CTD casts taken for the two fortnights of November 2008 and show the surface temperature values measured. The depth of the profiles are shown on the y-axis of figures 5a-c and 6a-c.

Q26: *Figures 7 and 8: please show the cruise track on the SSH map in order to clearly visualize where the profiles were assimilated.*

A26: We have shown the cruise track in figure 1 and since the figure 7 and 8 are on the same longitude and latitude axis, we choose to not plot the cruise track on these figures and clutter them further.

Q27: *Figure 13: problem with legend. SLA overlaid with tendency. SLA should be also on other figures. Please zoom on the eddy that is discussed in the text and do not show the entire domain.*

A27: We have corrected the legend on this figure in the revised manuscript. We have now pointed to the eddy discussed with an arrow to highlight its position.

ACPD

12, C9945–C9956, 2012

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper