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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.

Anonymous Referee #1

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This paper adresses the evolution of the mesoscale eddy field off the Peru Chile region during the VOCALS rex experiment. The mesoscale eddy field is analysed using a regional model numerical simulation, in which satellite data and in situ data collected during the vocals rex cruise are assimilated unsing a four dimensionnal data assimilation procedure. A heat budget is performed to identify the main physical terms contributing to the transport of heat in the system, and to enlighten the role of mesoscale eddies in exporting coastal waters offshore.

Major comments: 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 as-

similation procedure. 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. 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. 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 focusses 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. Moereover, the focus hardly reveals any particular information about this eddy. 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. 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? The use of a data constrained simulation is completely bypassed at the end of the paper. In conclusion, given these remarks, I can not recommand this paper for publication.

Specific comments: P20902,L26 : what do you mean by geometry? Coastline geome-

try? P20903,L4 : waves ? do you mean Rossby waves? L8 : ocean biology (upwelled, recycled ,..): uselesshere 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. 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. 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. 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. 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.

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. 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.

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. 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 . 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. 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 conclude 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 Holte 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”) can 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).

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!

Comments on Figures: 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. Figure 2: not necessary. 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. Figures 7 and 8: please show the cruise track on the SSH map in order to clearly visualize where the profiles were assimilated. 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20901, 2012.