

Reply to the Review of the paper:

Towards high resolution mapping of 3D mesoscale dynamics from observations: preliminary comparison of retrieval techniques and models within MESCLA project.

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Reviewer #1

Recommendation

In my opinion, this paper is interesting and timely but results are still too preliminary to be published. Therefore, I would not recommend it for publication in Ocean Science in its present state. On the contrary, I encourage the authors to resubmit it when they will have maturer conclusions.

Though the study is preliminary in the sense that it is based on the analysis of test cases (which already required a significant amount of work) and not on a routine application of the techniques considered, we feel that the reviewer's comment is due to a poor presentation of the background and discussion of the results in this first version of the paper. In fact, looking at our answers to the specific questions in the following, it will be clear that substantial and relevant conclusions were already obtained. These results would be described in a more complete and detailed way in a revision of the manuscript.

Specific questions

1. Does the paper address relevant scientific questions within the scope of OS? Yes. The reconstruction of 3D density and velocity fields (including vertical velocities) is a key question for many fields of oceanography.
2. Does the paper present novel concepts, ideas, tools, or data? Not really. From my understanding of the paper, it does not present particularly new concepts or ideas. Indeed, their approach consists on the reconstruction of subsurface fields using EOF and then apply the omega equation to retrieve vertical velocities, which was already proposed by e.g. Ruiz et al. 2009. The details

of the used techniques have been already published (e.g. Buongiorno Nardelli and Santoleri 2005, Buongiorno Nardelli et al. 2001) or have been submitted for publication to this special number (e.g. Guinehut et al. 2012). The novel ingredient of this paper is its application to different datasets and numerical simulations. However, the results they present are still too preliminary to reach a significant conclusion.

We realize that the novel aspects of the work done should be made more evident and clarified in a revised version of the paper. In fact, though the reviewer is right saying that each individual technique was developed in previous studies, and also that some of the techniques were already combined in previous works (as in Ruiz et al. 2009), there are several novel aspects in this work:

- 1) none of the techniques considered was ever applied at high resolution (namely resolving mesoscale dynamics) to retrieve data that could be produced routinely within an operational system (namely from NRT, freely available data, and potentially with global coverage);*
- 2) it was the first time that a high resolution SSS product (as this developed within the MESCLA project and described in Buongiorno Nardelli, JTECH, 2012) could be used to retrieve 3d fields. The combination of HR SSS, SST and ADT data is thus absolutely novel;*
- 3) similarly, it was absolutely the first time that QG vertical velocities were retrieved from HR observation-based 3D fields that could be produced from data available daily within operational programs (i.e. within Myocean catalogue).*

It is also worth noting that the applicability and tuning of the retrieval methods depends on the area understudy, so that the application of the multivariate reconstruction techniques to the Gulf Stream area represents by itself a novel result.

About the significance of the results, we realize that the reviewer's comment is probably due to the lack of background and discussion in the present version of the paper. On the opposite, our work shows that a more advanced dynamical framework (compared to simple geostrophic approximation) can be considered also when analyzing purely observation-based products (see also next comment), which is, in our opinion, a quite relevant result. This aspect would clearly be kept into account in submitting a revision (see also comments below).

3. Are substantial conclusions reached? No. It is a preliminary study.

Though the study is preliminary in the sense that it is based on the analysis of test cases (which required a significant amount of work) and not on a routine application of the techniques, we think that some substantial and relevant conclusions were obtained. Our work confirmed that QG dynamics accurately approximates the PE vertical velocities at 1/12° resolution. However, while comparing geostrophic currents estimated from altimeter data with PE model velocities is commonly accepted as a standard procedure, more advanced dynamical frameworks are not generally considered when looking at observations. Our work aimed to demonstrate that purely observation-based dynamical analyses can be provided routinely and used for comparison to model output going beyond the simple geostrophic framework.

Unfortunately, from both reviewers' concerns, it is clear that the background and conclusions were not written clearly, making it more difficult to understand the significance of the work done. We apologize for this. In a revised version of the paper we would thus clarify both the objectives and the main findings as well as the novel aspects of the work performed and described in this paper.

4. Are the scientific methods and assumptions valid and clearly outlined?
Yes.

5. Are the results sufficient to support the interpretations and conclusions?
In my opinion, this study requires additional work to reach significant conclusions.

Unfortunately, the reviewer does not give any detail on what he/she has in mind. On the other hand, we agree that a substantial revision of the text is needed to better show all the work that has already been done.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Results are reasonably traceable.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes.

8. Does the title clearly reflect the contents of the paper? Yes.

9. Does the abstract provide a concise and complete summary? Yes.

10. Is the overall presentation well structured and clear? Yes.
11. Is the language fluent and precise? Yes.
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? The description of the experiments could be slightly clarified but, in general, their work can be easily understood.
14. Are the number and quality of references appropriate? A lot of effort has been devoted to the estimation of vertical velocities in the ocean from observations. In my opinion, the authors miss some important references in this field. In addition, they should improve the discussion of their results and compare them with some of these previous works.

Though we felt to have sufficiently covered all the fields with appropriate references, it is clear that we might have missed some, so that a revision of the previous works to be cited would be done. Any suggestion would also be welcome.