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 #2

This discussion paper presents a series of techniques for mapping the 3-D mesoscale dynamics from satellite and in-situ observations, and then estimating vertical veloci-ties based on these gridded fields. The 3-D reconstruction techniques used in this analysis have all been developed and analysed in previous studies (eg ARMOR 3D, Guinehut et al., 2012; and the mEOF-reconstruction, Buongiorno Nardelli and Santoleri, 2005). The vertical velocity calculation is based on the QG omega equation (eg Tintoré et al., 1991). The originality of this present paper is to compare these different vertical reconstruction techniques with each other, and with the outputs of an ocean general circulation model with assimilation (the Mercator 1° global model and $1/12^\circ$ North Atlantic model). Although this could be a valid objective for a research paper, unfortunately there is very little interpretation or discussion of the results in the present paper. As such it is not suitable for publication, in its present form.

We apologize for the lack of details in the description and interpretation of the results of the test cases considered. However, we also feel that it would be possible to describe more precisely the work done in a revised version of the manuscript. This would clearly require also a clarification of the objectives and innovative aspects of the significant amount of work already done. Evidence of the significance of the results obtained should emerge from the answers to the specific comments in the following.

1. Does the paper address relevant scientific questions within the scope of OS?

The question of how well the different vertical reconstruction techniques and the models can represent the ocean's vertical velocity field could be informative, even though there is no intercomparison with fine-scale in-situ data estimates, and the errors in the reconstruction fields will have a large impact on the vertical

velocitiy field.

Actually, we agree that the errors in the reconstruction of the field have an impact on the retrieval of the vertical velocities, as well as the resolution at which the retrieval is carried out. This is one of the motivations why we decided to compare different retrieval techniques. The same, however, clearly applies to 3d reconstructions through data assimilation in numerical models. Unfortunately, it is then clear that estimating the accuracy of the vertical velocities in absolute terms is not really possible, given the impossibility to measure them with in situ data (even in the eventuality that fine-scale T/S measurements were available).
On the other hand, a significant amount of work has been done to compare the performances of different techniques in retrieving the tracer distribution, through both hindcast and independent data validations. More details on this work would be added in a revised text.
As a second comment: 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/adopted 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, it is clear from both reviewers' concerns that also the background and conclusions were not written clearly and 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.

2. Does the paper present novel concepts, ideas, tools, or data?

No. The 3-D reconstruction techniques used in this analysis have all been developed and analysed in previous studies (eg ARMOR 3D, Guinehut et al., 2012; and the mEOF-reconstruction, Buongiorno Nardelli and Santoleri, 2005). The vertical velocity calculation is based on the QG omega equation (eg Tintoré et al., 1991).

Again, 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, there are a number of novel results described in this paper:

1) *none of the techniques considered was ever applied at high resolution (i.e. fully*

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 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 multivariate reconstruction techniques to the Gulf Stream area represents by itself a novel result.

3. Are substantial conclusions reached?

No. The paper lacks discussion and interpretation of all of the case studies that are presented.

It would be possible to add a more detailed discussion and interpretation of all the case studies presented, or concentrate and fully describe only some selected cases (depending also on the editor and reviewers' suggestions), in a revision of the manuscript.

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

The scientific methods and assumptions are based on previous analyses, and only a brief overview is provided the details of these methods are not fully explained.

More details could be easily added in a dedicated Appendix or in the text.

5. Are the results sufficient to support the interpretations and conclusions?

No. Although an overview of the different case studies is presented, the results are poorly analysed, there is not much description of the Figures, and

very little interpretation and discussion of the results.
See comment to previous point 3

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

No. The methods are mainly described in other publications, and the overview given in this discussion paper does not allow the reader to understand the details of the technique.

See comments to point 4

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

Yes. the references to previous studies is provided, and the "originality" presented here is in comparing the different techniques.

Actually, as already written in response to point 2), the originality resides in comparing the various techniques, in combining 3D retrievals and QG diagnostics, but also in applying them to new data and new areas.

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

Yes.

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

The abstract covers the philosophy behind this study, but does not give any information on the results of this discussion paper, to quantify how well the different 3D reconstruction techniques perform in different regions, test cases, etc.
More details on the results of the work would be added in a revised version of the paper.

10. Is the overall presentation well structured and clear?

No. There are many case studies which are listed but not discussed, and the

lack of background on the different reconstruction techniques means that it is difficult to piece together the different case study results.
See all the above.

11. Is the language fluent and precise?

The english is reasonable good, however, the lack of background and discussion content means that the paper is quite difficult to read. There are many acronyms mentioned, which create unwieldy sentences. As an example, see the SST description on p 1051 lines 15-24.

As already said, we apologize for the lack of clarity in presenting the objectives and main results of the work. A revision on the use of acronyms would be easily performed.

3. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes – ok.

4. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Yes –a number of case studies listed in sections 5.1 and 5.2 are never presented.
In section 5.1, the differences between the 5 configurations are not explained in a dynamical or statistical sense. Figure 2 is not explained adequately, for example, what is the explained variance for the 3 cases presented here?
All of the results section should be worked on to improve the discussion and interpretation.

All case studies (or a selection of them) could be fully detailed and corresponding sections improved in a revised version of the paper.

5. Are the number and quality of references appropriate?

Yes – although they are used mainly to introduce the work, and not to interpret the results.

Is the amount and quality of supplementary material appropriate? N/A