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Interactive comment on “Computation of a new Mean Dynamic Topography for the Mediterranean Sea from model outputs, altimeter measurements and oceanographic in-situ data” by M.-H. Rio et al.

Anonymous Referee #2

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Review of OSD manuscript: computation of a new Mean Dynamic Topography for the Mediterranean Sea from model outputs, altimeter measurements and oceanographic in-situ data by Rio et al

The paper describes the computation of a new Mean Dynamic Topography (MDT) in the Mediterranean Sea. The methodology was already used for a previous calculation but this new estimation uses both hydrological data and drifter velocity data. This required some methodological adjustments. Several sensitivity studies are carried out (first guess, correlation scales). The new MDT is compared to independent in-situ observations. It is shown to provide a significantly improved solution compared to the Rio et al. (2007) MDT.

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This is an important topic as a precise MDT is essential for altimeter data assimilation and to interpret altimeter observations and compare/merge them with other observations. The paper is well written and provides a very convincing demonstration that the new MDT model is (much) better than the previous one. Although the method itself is not new, it is the first attempt to merge recent hydrological data and drifter data to estimation a MDT in the Mediterranean Sea. The paper thus definitively deserves to be published in Ocean Science. This being said, the authors should significantly improve the paper on the following aspects:

1.It should be reminded that the use of optimal interpolation or objective analysis for the mapping of mean fields does not rely on good theoretical grounds. Defining a covariance model (in a statistical sense, i.e. an average over many statistical realizations) for mean fields is questionable (see discussion in Davis, JGR, 1985). This is fine though to use it as any other interpolation method but this cannot be considered as an “optimal” method. This cautionary note should be reminded.

2.Covariance models. Taking into account cautionary note above, it should be noted that the model used in this study was defined for mesoscale variability statistical characterization. It is likely that better models should be defined for mapping of mean fields.

3.Handling of errors for hydrological data. Errors do not include systematic errors in the model mean dynamic topography. Do you really believe that CTD observations corrected from altimetry can give you local MDT as precise as 1 cm or less ? It seems to me that you are underestimating the errors on hydrological data. I understand this is difficult to estimate and that this would require taking into account correlated errors in the mapping procedure but this must be better discussed.

4.I would have liked to have a discussion on the potential of GOCE. Given the fact that correlation scales of MDT (e.g. figure 14) and of existing/remaining errors are often much larger than 100 km I would expect/hope that GOCE will make substantial improvements for MDT estimation in the Mediterranean Sea.

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Specific remarks:

Page 5: isotropy. You are not using an isotropic covariance model.

Page 5: removing a large scale field (first guess) does not mean that the mean field is zero. Do you take this into account (as proposed by Bretherton et al., 1976) ?

Page 8 : a map showing the values of alpha (and their seasonal variations) would be quite useful to discuss. Any error bars on the alpha estimations ?

Page 8: adding an estimation of the missing component to mean dynamic height at 350 m will add errors and biases in the hydrological MDT observations. This must be discussed (see point 3 above).

Page 9: you estimate the reduction of drifter velocity variance due to the altimetry derived correction. I assume you did a similar calculation for hydrological profiles (page 8) but this is not discussed. Why ?

Page 9: you should explain the 30% (resp. 40%) figures.

Page 10: why not using both Eq. 3 and Eq. 4 to adjust x_0 and y_0 ? Why not using a first guess MDT model and estimate from it x_0 and y_0 ? see also point 1 above.

Page 10: why not giving also the Eq. for $\langle U V \rangle$?

Page 11: Tables 1, 2 and 3 should also give the rms of drifter velocities.

Page 13: why not using your alpha estimations to compare CTD profiles to the MDT ?

Interactive comment on Ocean Sci. Discuss., 11, 655, 2014.

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