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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 #1

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Review of 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, A. Pascual, P.-M. Poulain, M. Menna, B. Barceló, J. Tintoré

Manuscript Number: os-2013-88

This manuscript is devoted to the computation an improved Mean Dynamic Topography (MDT) of the Mediterranean: an update of the previous MDT computed by Rio and coauthors. This new version of the MDT will likely permit to calculate better absolute altimetric heights and geostrophic currents, resolving spatial scales not resolved by the previous Rio05 MDT (or 2007?) currently provided by AVISO. The opportunity to re-

solve smaller spatial scales is crucial in a basin like the Mediterranean Sea where, as correctly pointed out by the authors, the value of the Rossby radius of deformation and the presence of coasts, island and straits represents a further challenge for the ocean altimetry. For this reason it would be very helpful for future users of this new MDT to know how close to the coast this product can still considered valid and an order of magnitude for the smaller spatial scale that, reasonably, an absolute sea level computed using the new MDT will resolve. As a general comment I found that the results described by the authors look quite promising for a better exploitation of altimetry data in the Mediterranean Sea. In this sense, it would be interesting to find in the conclusions of the article, a few more words about a possible future use of this new product in the context of the operational oceanography of the Mediterranean Sea.

I would recommend publication of the manuscript provided revisions are made to address the above general comments and the specific comments below (very minor revision).

I have not checked the paper also for misspellings or other English errors but I have seen the os-2013-88-comments-to-the-authors that includes most of the misspellings errors I have noted. The most important is add units in the three tables.

Page 3, line 13: ". . . the time variable (ha'(t,r), ua'(t,r), va'(t,r)) component as measured by altimetry". Do you mean the SLA (Sea Level Anomaly)?

Page 3, line 18: here the "multivariate objective analysis" is mentioned for the first time but equation 2 looks like the equation for a univariate OI of the single variable h. The approach multivatiates between lines 8 and 11 of page 4 when the relation between the covariance function of h the covariance functions of the geostrophic velocities is introduced. A similar multivariate approach has been described and used (for instance) by Schlatter et al. (1976) or Schlater (1975) for geopotential height and u and v component of the wind.

"Schlatter, Thomas W., Grant W. Branstator, Linda G. Thiel, 1976: Testing a Global Multivariate Statistical Objective Analysis Scheme with Observed Data. Mon. Wea. Rev., 104, 765–783. doi: http://dx.doi.org/10.1175/1520-0493(1976)104<0765:TAGMSO>2.0.CO;2 “

or

Schlatter, Thomas W., 1975: Some Experiments with a Multivariate Statistical Objective Analysis Scheme. Mon. Wea. Rev., 103, 246–257. doi: http://dx.doi.org/10.1175/1520-0493(1975)103<0246:SEWAMS>2.0.CO;2

I suppose that the multivariate approach used in this paper should be similar. Can the authors confirm my supposition or discuss differences? A full description of the multivariate objective analysis theory is not needed here but few sentences that indicate the basic concept of a multivariate objective analysis will certainly help the reader.

page 5, lines 2 to 7, MFS model was averaged between 1993 and 1999. What about the interval used for NEMO? At page 9, line 8 is written that the same time interval has been used also for NEMO. Why not anticipate this information also here?

Page 5, line 18: Please define “geostrophic drifter velocities”.

Page 5, line 26: Justify the choice of 350 m.

Page 11, line 30: “The SST pattern also show TO cold cores” should be “The SST pattern also show TWO cold cores”

Page 12, line 30: Future work: “For the future, further work about the definition of the correlation scales is needed. . ...” This indication for future work seems to be in contrast with the “moderate impact” found for choice of the correlation scale (see abstract of the paper). Is this moderate impact caused by the lack of data or it is an intrinsic effect due to the OI strategy for the selection of influential points respect to the choice of the correlation function and its parameters?

Table 1, 2 and 3: Specify units.

Figure 17: First row: mean SST patterns corresponding to the annual 2007 average for the 6 Ligurian basin (left) and Thyrhenian basin (right). Second row: mean circulation as derived 7 from the previous SMDT solution. Third row: mean circulation as derived from the SOCIB8 CLS-MDT solution. . .. . .. IN MY FIGURE 17 THERE IS ONLY A ROW (THE LIGURIAN SEA)

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