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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.

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The reviewer will find below the response to his questions/comments:

C73: ... 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.

Response: The validity of our product in coastal areas will certainly depend on the area. In strong coastal current areas, we are confident that the new MDT is quite realistic, as it benefits from numerous accurate drifting buoy velocity estimates. The

altimeter anomalies used to remove the temporal variability of the currents may indeed be less accurate in coastal areas than in the open ocean. However, as shown on Figure 11, the use of these anomalies in strong current areas still allow to efficiently reduce the standard deviation of the velocities in the 1/8° boxes. This is confirmed by the realism of numerous coastal currents in the obtained field (Algerian current, Ligurian current. In less energetic coastal areas, the confidence may be reduced, but still Figure 11 does not show any specific areas where the variability of the synthetic velocities is significantly increased compared to the drifter variability. Regarding the final resolution of the product, we computed the MDT on a 1/8° resolution grid, to be consistent with the size of the boxes chosen to compute the synthetic velocity means from the drifters. This is a lower resolution compared to the resolution of the grid model from which the first guess was calculated (1/16°), but the 1/8° grid was chosen to keep enough velocity measurements in each box so as to end up with statistically significant estimates of the velocity mean and variance. In addition, while the MFS model resolution is indeed 1/16°, the average other the 1993-1999 period seems to feature lower resolution scales (top plot of Figure 2). These considerations will be added in the revised paper.

C73: 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

Response: The following sentence will be added at the end of the conclusion: "Accurate knowledge of the MDT is known to strongly improve the performance of ocean modeling and forecasting system assimilating altimetry data (Haines et al, 2011). We therefore expect this new, improved MDT product to be highly useful in the context of the operational oceanography of the Mediterranean Sea." Haines, K., J. A. Johannessen, P. Knudsen, D. Lea, M.-H. Rio, L. Bertino, F. Davidson, and F. Hernandez (2011): An ocean modelling and assimilation guide to using GOCE geoid products. Ocean Sci., 7, 151–164.

C73:... The most important is add units in the three tables

Response: This will be done in the revised paper.

C73: 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)?

Response: Yes indeed. This will be specified in the revised paper.

C73: 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.

Response: Actually, equation 2 is generic: "<h>(r) is obtained at the spatial position r as a linear combination (Eq. 2) of the observations O(ri)". Observations may be either heights or current. This will be specified in the revised paper.

C73: 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?

Response: This is indeed the case. The two references given by the reviewer have been added for information in the revised paper.

C73: 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.

Response: Actually, the basic concept is indeed described in the 2 pages of the method section, and for further information, the reader is invited to read appendix A from Rio and Hernandez (2004) paper. I am not sure to understand which kind of additional information the reviewer is expecting.

C73: 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?

Response: This information will be added in the revised paper: "The first modeled MDT was computed averaging over the 1993-1999 period outputs from the MFS model (Adani et al., 2011), while the second MDT uses outputs from a NEMO model configuration (Beuvier et al., 2010) over the same 1993-1999 time period."

C73: Page 5, line 18: Please define "geostrophic drifter velocities".

Response: This will be done in the revised paper by adding the following sentence: "The drifting buoy velocities have been processed by Poulain et al. (2012) in order to extract from the drifter total velocities the geostrophic component (i.e. the component of the current resulting from the balance between the pressure gradient forces and the Coriolis force)."

C73: Page 5, line 26: Justify the choice of 350 m.

Response: Actually, this choice was justified already as resulting "from making a compromise between the number of profiles available (the deeper the reference depth, the less the profiles available) and the dynamical content of the calculated dynamic heights (the deeper the reference depth, the more complete the captured baroclinic content)." The following sentence will be added in the revised paper: "A deeper (450m) reference depth was tested but led to a reduced number of profiles in the Western part of the Ionian basin, which is already poorly sampled using the 350m reference depth (Figure

5)."

C73: Page 11, line 30: "The SST pattern also show TO cold cores" should be "The SST pattern also show TWO cold cores"

Response: This has been corrected

C73: 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?

Response: This consideration about the refining of the correlation scales is justified by the fact that the model used in this study (Arhan and Colin de Verdiere, 1985) was defined for mesoscale variability statistical characterization. It is likely that better models should be defined for mapping of mean fields. This is in part highlighted by the fact that we found different results for the correlation scales starting from Eq.3 or Eq.4. We will discuss in more details this point in the revised paper, adding the following paragraph in section 5.1: "Different results were obtained using the analysis of the zonal drifter velocities (Eq. 3) or the meridional drifter velocities (Eq. 4). In particular, the zonal correlation scales are lower (50-150km). Also, meridional correlation scales estimated from the meridional drifter velocities are much lower than those derived from the zonal drifter velocities (20-100 km instead of 60-120km). This discrepancy between the results obtained from <U,U> and <V,V> leads us to the conclusion that the correlation functions F(r) and G(r) may not be appropriate for characterizing the mean circulation of the Mediterranean Sea. They are obtained assuming a correlation function C(r) of the Sea Level that was defined by Arhan and Colin de Verdiere (1985) for statistical characterization of the mesoscale variability in the North Atlantic. It is likely better models should be defined for mapping of mean fields."

C73: Table 1, 2 and 3: Specify units.

Response: This will be done

C73: 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).

Response: This is strange, I don't understand, it is complete on the OSD website.

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