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Interactive comment on "Design and validation of MEDRYS, a Mediterranean Sea reanalysis over 1992–2013" *by* M. Hamon et al.

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Review n°1

Reviewer: « Review of the manuscript "Design and validation of MEDRYS, a Mediterranean Sea reanalysis over 1992–2013 " by Hamon et al. The manuscript describes and validates a Med Sea regional ocean reanalysis for the altimetry era that uses a new high-resolution atmospheric forcing downscaled from ERA-Interim. The manuscript details aspects of the configuration and presents the validation of the reanalysis with focus on the improvement borne by the data assimilation system.

I found the manuscript well-written and interesting for the ocean community. However, I ask the authors to clarify some aspects of the reanalysis system and of the discussion

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on the quality of the reanalysis before the manuscript can be accepted for publication. Although I have many comments below, I recommend a minor revision as all my concerns do not require many efforts but rather aim at improving the readability of the manuscript.

General points :

It should be mentioned clearly in the text that the atmospheric forcing is not the only responsible for the temporal homogeneity of the ocean reanalysis. Reanalyses intrinsically suffer from inhomogeneity in the observing network: this applies to ERA-Interim, ALDERA (through downscaling of ERA-Interim) and MEDRYS. While the strategy of this paper aims at limiting this compared to other products, there is no evidence about the temporal homogeneity of this reanalysis and this approach compared to others.»

Answer : We agree with the reviewer, there is no evidence of the temporal homogeneity of the reanalysis. We cannot overcome observation coverage issues, but as we mentioned in the introduction, we pay a special attention to the homogeneity of the atmospheric forcing. This specificity differentiates MEDRYS from other cited products. we will clarify this point, pointing out that we worked on atmospheric forcing in order to reduce as most as possible the sources of inhomogeneities.

We will replace the sentence (P1819 L29) “We pay a special . . . of the Mediterranean circulation and trends” by “Even if we cannot overcome other homogeneity issues resulting from the coverage of the observing network (applying in both MEDRYS and ALDERA), we pay a special attention to the consistency of the atmospheric forcing (same resolution, same model physics) in order to reduce as most as possible the sources of inhomogeneities in MEDRYS. This reanalysis then allows a better description of the interannual to decadal variability of the Mediterranean circulation and trends.”

Reviewer: « The description of SLA assimilation should be improved especially in Section 2.4. From Section 2.4, it seems that a correction is applied to the MDT to account for the barystatic effect. This does not look sensible, I guess the model SSH is rather

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corrected for the mass intake. It is also important to specify the reference period for MDT and SLA (1993-1999?) and how the regional steric effect is included: are data assimilated as they are? How are formulated the boundary conditions in the Atlantic to conserve or not the volume ? This should be clear also when commenting Figure 3. The mismatch is attributed to ORAS4 but it is not clear why. It might also be due to mismatch in the seasonality of the steric signal.»

Answer : It is true in principle that the addition of 0.85mm/y should arrive into the model via the runoff. In practice, the average SLA over the domain in imposed in the buffer zone and this is the technical way to cope with the model free surface constraints. The reference period for SLA is 1993-2012, this has been added in the text. The model is not sensitive too the “global” steric effect. The steric change is imposed by the assimilation over the temperature and salinity. The SSH is imposed in the buffer zone, meaning that the volume changes according to ORAS4. We attribute the mismatch of the seasonal cycle to ORAS4 for the following reason, that we did not want to detail. When we look at the innovation in temperature and salinity at 700m or 1200m, the largest innovations are in the buffer zone (e.g. more than 0.5psu at 1200m). It means that the strong restoring to ORAS4 may somewhat degrade MEDRYS.

Reviewer: « Some results need better explanation and discussion: for biases in the top 150 m at least (Section 3.2.2) and for the seasonal cycle of bias (Figures 9a 10a) the authors should provide an explanation or at least a guess, in order to provide a justification and ideas for the next release. This won’t reduce the manuscript to a validation exercise but will make it a useful summary for the interested readership. The speculation on the salinification 2000-2005 (Figure 10) in section 3.2.2 and 3.2.3 (end of 2nd paragraph, starting with "This suggests") appears confused. The authors say that it may depend only on the atmospheric forcing, propagating in depth, and not on the data assimilation system but it is not present in NM12-FREE. In Section 4 it is mentioned a possible problem in SLA assimilation.There is no evidence the data assimilation system is not responsible for that, and probably issues in SLA data assim-

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ilation appear more convincing. The authors can compare NM12-FREE and MEDRYS in more details to reach a conclusion.»

Answer : The explanation for the bias in the top 150m is given only in the Discussion paragraph. We agree that this could be inserted in the chapter 3.1 (and 3.2.2) for a better understanding. As we wrote it, the surface bias could be explained by a bad adjustment of the SLA model equivalent. We noted that the mean SLA innovation (obs-model) was decreasing, meaning that the simulated sea level trends to rise too quickly compared to the observations. In response thereto, the system tends to compensate by densifying surface waters. As the assimilation system is more constrained on temperature (due to better data coverage) it has a strong effect on salinity. We think that a better accuracy on the SLA model equivalent computation should improve bias issues in the surface layers.

Concerning the seasonal bias, we think that you refer to the lack of stratification in MEDRYS during summer. This point is discussed at the end of section 3.1. It can be explained in one hand, by the too dense surface layers, and in the other hand, by the strong gradients that occur at this period. Indeed as we explained, a small variation in the forecast trajectory of the ocean model is then more likely to drift from observations and the RMS naturally increases during summer.

A fuller explanation for the surface bias has been added in section 3.1 in particular the SLA model equivalent computation issue.

Concerning the salinization in the early 00's, there is a misunderstanding. Only the variability of surface salt content can be related to atmospheric forcing. The strong evaporative period leads to similar variability in the surface salt content in both MEDRYS and NM12-FREE. However, as MEDRYS presents a lack of stratification due to biases issue (and so, due to a problem in the assimilation system), we think that the dense water anomaly is transfered toward the bottom (mostly by convective processes). Moreover, as we did not found any salt anomalies neither in the CLASS4 validation exercise nor in

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the reference hydrographic products (IMEDEA and EN3) in the bottom layers, this suggests that this spurious anomaly is not present in in situ data, confirming the surface source of the anomaly.

We rephrased the 2nd paragrah of section 3.2.2, detailing that only the variability of the surface salt content is due to the atmospheric forcing (noticeable in both MEDRYS and NM12-FREE), but because of a problem in the assimilation system, the too dense surface waters in MEDRYS plunge toward the bottom.

Specific points :

Reviewer: « P1817 L28 to P1818 L3 : Please rephrase as it is not clear: not clear why "especially in ocean modelling", and not clear in general the link between the persistence of small scale fields and the inaccuracies in ocean models.»

Answer : Indeed, the sentence is not clear and we replaced it by : “Modeling the different time and spatial scales of this circulation is still challenging because for example of approximations and uncertainties on non-linear dynamical balance, atmospheric forcing or the bathymetry (Sorgente et al., 2011; Pinardi et al., 2013).

Reviewer: « P1819 L23-L25 : This is not clear. First, why the NEMOMED12 with ORCA should give a resolution close to NEMOMED16 ? Second, why ORCA grid (never introduced), which is tripolar, is used in the Med Sea? »

Answer : NEMOMED12 is a regional configuration of the Mediterranean sea based on the ocean general circulation model NEMO using the so-called 1/12° ORCA grid. That’s why we used the same tripolar grid (in order to clarify the ocean model configuration section 2.1, we choose to not introduce this standard configuration of the NEMO grid). The NEMOMED12 grid is not regular and corresponds to a varying grid cell size between 6 and 7.5 km whereas the NEMOMED16 grid has a regular cell size of about 7km. As the difference is not significant, we suppose that the resolution is similar in the two models.

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The sentence has been replaced by : “Our ocean model used is NEMOMED12 (Beuvier et al., 2012a), a Mediterranean configuration of NEMO (Madec and the NEMO team, 2008; an update version of the OPA code) with the ORCA12 standard grid. The ORCA12 grid shows a varying resolution around 1/12° over the world ocean. Within our numerical domain, the ORCA grid has a horizontal resolution ranging between 6 and 7.5km. Note that this spatial resolution is similar is to the the 1/16° regular horizontal grid used in Adani et al. (2011).”

Reviewer: « P1822 L8-10 : "a period known..." please provide a reference because this statement is not obvious »

Answer : We agree with the reviewer that this statement is vague. It is based on past analyses we performed with ARPERA and ERA-40 datasets and this statement is shared within the Mediterranean community using ERA-40 as RCM lateral boundary conditions. However no published reference do exist to illustrate it. We therefore remove this statement.

The sentence will be rephrased in: “It may include temporal inhomogeneities especially in 2001 when the large-scale driving fields changes from ERA-40 to ECMWF analysis.”

Reviewer: « P1823 L4-6 : ERA-Interim does not have an independently generated SST/SIC analysis, which is taken externally (NOAA). By using this, it should be noted that there is a degradation in the resolution of the SST fields »

Answer : We agree with the reviewer. We modified the sentence in: “The sea surface temperatures and the sea ice limit (Black Sea) are updated every month with a seasonal and interannual variability using the same SST and sea ice analyses as the one used to drive the ERA-Int reanalysis (Dee et al 2011)”

Reviewer: « P1823 L6-9 : The sentence "As ERA-Interim constitutes..." appears quite subjective in this way. The authors should cite proper work where ERA-Interim appears as the best atmospheric product over the Med Sea. Otherwise the sentence should be

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drop. »

Answer : We agree that the statement is subjective and we rephrased in: “As atmospheric reanalyses constitutes today the best knowledge of the 4-D dynamic of the atmosphere available over the last decades, such a simulation is often called “perfect-boundary simulation” or “poor-man regional reanalysis”.

Reviewer: « P1824 L27 : Introducing ALDERA, it should be earlier mentioned that (as it seems from the text on this point) that no spectral nudging nor data assimilation is used. The gain in resolution is balanced by the loss in "day-today chronology" as the authors say at this point. »

Answer : This clarification has been added in the description of ALDERA, in the section 2.2.

Reviewer: « P1826 L19-23 : This sentence seems in contradiction with the title of the Section 2.2.3 and with the fact that the authors are actually comparing with lower resolution atmospheric products. I suggest dropping or replacing with a sentence indicating that the qualitative comparison highlights the superiority of ALDERA in representing the small-scale features. »

Answer : We agree that this sentence and the title of the section are in contradiction. Following this remark and comments from reviewer 2, we decided to modify the title of section 2.2.3 in “Illustration of the small-scalle features in the ALDERA forcing”. Indeed we can not consider that we demonstrate clearly here the added-value of this new forcing. This is actually outside the scope of this study. In section 2.2.3, we mostly (1) use the published literature to underline that dynamical downscaling of reanalysis were shown to bring added-value to ocean forcing and (2) illustrate the superiority of ALDERA in representing small-scale features with respect to lower resolution twin runs.

Reviewer: « 1826 L26- : "The Mercator Ocean .... has especially demonstrated" appears as a subjective speculation without corroborating references and examples. It is

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not proper for a general readership that can be not aware of MyOcean. »

Answer : We refer here to Lellouche et al. (2013) that describes and validates the global reanalysis within the Myocean program (reference in the next sentence). As we use the same assimilation system, we think that it is relevant to point out that the system demonstrated skills in a global configuration. However, the fact that it has been done within the MyOcean program is not essential and has been removed.

Reviewer: « P1827 L11-12 : Not clear if the anomalies which represent the background error covariances are flow-dependent or only collected and grouped by season. Please rephrase to state it clearly. »

Answer : We rephrased L10 to L14 in : "The forecast error covariance is based on the statistics of a collection of 3D ocean state anomalies. For a given cycle centred on the Nth day of a given year, ocean state anomalies computed from NM12-FREE within the window [N – 60 days ; N + 60 days] of each year are gathered and define the covariance of the model forecast error. For the Mediterranean configuration, we computed about 900 anomaly fields from the NM12-FREE free simulation for a given assimilation cycle"

Reviewer: « P1827 L29 : Perhaps it would help the reader to say that while in the original formulation of SAM SSH increments are analytically computed from T,S increments through barotropic / dynamic height balances, in the Med Sea implementation they are purely statistical and derived by the covariances between SSH and T,S implied by the ensemble of anomalies (if this is the case). The fact that wind component is included (also in Section 4) is misleading. »

Answer : You are totally right concerning the computation of the SSH increments in the MEDRYS assimilation system. However, there is a misunderstanding about the wind. The wind component is not included in the computation of SSH increments but, in order to take into account the effect of the wind on it, we did not use the barotropic plus baroclinic approximation, but the surface elevation prognostic variable of the model.

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we rephrased the last paragraph of section 2.3 in : “The original formulation of SAM SSH increments are analytically computed from T,S increments through barotropic / dynamic height balances (Lellouche et al., 2013). This assumption is only valid far from the coast and in open seas, where the local SSH variations due to the remote wind are negligible. In the Mediterranean Sea, strong regional winds occur in areas with low bathymetry and near important straits like Gibraltar and Sicily. A significant part of SSH is then driven non locally by the wind. Shelf surge and hydraulic control effects are typically 10 times larger in the Mediterranean Sea than in the middle of the ocean. In our regional configuration, SSH increments are purely statistical and derived by the covariances between SSH (the prognostic variable of the model) and T,S implied by the ensemble of anomalies.

Reviewer: « P1828 L7-10 : It should be understood why there exists a bias between ERA-Interim and NOAA since ERA-Interim uses the NOAA SST. Maybe NOAA is corrected in ERA-Interim since the former is a foundation SST ? »

Answer : You are probably right. It could also come from interpolation processes. The SST maps used by ERA-Interim have undergone several interpolation processing. From a native resolution of 0.8°, they have been interpolated on ALADIN 12km grid and finally on the ORCA12 grid whereas the assimilated SST AVHRR-AMSR in MEDRYS is 0.25° resolution. We thought it was not appropriate to detailed it in the paper.

Reviewer: « P1828 L 13-14 : Not clear if the filtering/subsampling is performed by AVISO or it is specifically performed for MEDRYS »

Answer : In order to clarify this aspect, the sentence “The along-track SLA is provided by AVISO (SSALTO/DUACS handbook, 2009) and comes from . . . to avoid redundant information (Dufau et al., 2013).” has been changed by: “Along-track SLA delayed-time products, specifically reprocessed for Mediterranean Sea, and distributed by AVISO (http://www.aviso.altimetry.fr) in April 2014 in the framework of MyOcean project, has been assimilated in MEDRYS. These products include along-track filtering (low pass

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filtered with a cut-off wavelength of 65km for the whole domain) and along-track sub-sampling (only one point over two is retained to avoid taking into account redundant information). For these products, the reference period of the SLA is based on a 20-year [1993-2012] period. Names and acronyms used in this paper as well as the measurement period of each satellite are summarized in Table 3."

Reviewer: « P1829 L7 : what does it mean "validated" ? I guess they are the measurements flagged as "good" by CORIOLIS »

Answer : Yes. We replaced "validated" by "flagged as good".

Reviewer: « P1830 L15 : what does the "RMS of observations" mean? It is the variability, ie its standard deviation over time? »

Answer : Exactly, we talk about the standard deviation over time of observations. It has been replaced.

Reviewer: « P1830 L20 : are this insitu data coming from CORA? Please specify. »

Answer : Yes, as we mentioned it at L17, we both assimilate in situ surface data and gridded maps from satellite measurements. It has been precised "in situ surface data from CORA".

Reviewer: « P1832 L20- : This sentence, along with short description on criteria adopted for characterizing observational errors should go in Section 2.4. Here it is not clear how obs error are augmented and if this applies only to the Ionian sub-basin.»

Answer : A precise explanation of how and why obs error is increased has been added in section 2.4, for both SLA and surface salinity (not mentioned in the actual paper). Actually for SLA, we choose not to trust observations near the coastal areas. The obs error is then increasing within 50km of the whole Mediterranean coast. As the Ionian sub-basin has farthest points from the shore, costal errors correction has here, less effect. That's why more energy and feature are injected by the assimilation process, particularly in this area.

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In section 2.4, we rephrased the sentences L18-21 : “The assimilation of SLA . . .with GOCE and reanalysis data (Lellouche et al., 2013)” by “The assimilation of SLA observation requires the knowledge the observation error and of a Mean Dynamic Topography (MDT). Concerning the observation error, we choose to not trust observations near the coastal areas. The observation error is then artificially increased within 50km of the whole Mediterranean coast. The mean surface reference used is a hybrid product between the CNES-CLS09 MDT (Rio et al., 2011) adjusted with the data from the Gravity field and steady-state Ocean Circulation Explorer (GOCE) and from the Mercator-Ocean 1/4° Reanalysis GLORYS2V1(Lellouche et al., 2013) representing the 1993-2012 period.”

Moreover, we added that we apply a similar methodology for the SSS. As runoffs from the 33 main rivers are added as precipitation at mouth points and the averaged values of the inputs of the other rivers are gathered as a coastal runoff, we apply a coastal mask in order not to assimilate salinity data near the coast.

In section 2.4, we will add the following sentence at the end of the paragraph : “As for SLA, we choose to not assimilate surface salinity observations near coastal areas. Due to how we model the continental freshwater intake along the coast (section 2.1), we apply a coastal surface mask within which the salinity observations are artificially replace by the hindcast value. This concept of pseudo observation near the coast has already been used in Lellouche et al. (2013) to overcome the deficiencies of the background error, in particular for poorly observed variables.”

Reviewer: « P1835 L25 : To me the blue curve tells us only that with the increase of observations there is also an increase in the observation sampling, leading to worse skill scores for MedAtlas climatology. It does not necessarily mean an increase in the oceanic interannual variability. »

Answer : We think there is a misunderstanding about the MedAtlas climatology we used for this comparison. What we refer to MedAtlas-1998 is actually a single field

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representing the ocean state in 1998 (as we explained in the previous paragraph). As the reference for the computation of the temperature (or salinity) difference at the place of observation is not time-dependent and assuming that the data coverage is sufficiently homogeneous, we consider that this difference represents the magnitude of the oceanic variability.

Reviewer: « P1837 L 17 : I think correlation difference for SSS 0.785 vs 0.783 is not significant and should be used "slightly better" or "neutral" »

Answer : We agree with the reviewer, the sentence has been replaced by “Regarding the SST, MEDRYS has a better correlation with LION buoy than NM12-FREE (respectively 76% and 31%). However, MEDRYS and NM12-FREE show a similar correlation for SSS of 78%.

Reviewer: « P1838 L 22-23 : The consistency with ORA-S4 should be by construction of the boundary conditions. I think this sentence is not needed. »

Answer : We agree, as temperature and salinity are relaxed to ORAS4 fields in the buffer zone. The sentence has been removed.

Reviewer: « P1839 L5 : Not clear how the authors compute transport from EN3. What is the assumption for the velocity as EN3 and IMEDEA provide S only? Does it affect the comparison? »

Answer : Indeed, we added a clarification. Assuming that the Mediterranean volume is constant, the evolution of Mediterranean salinity is directly linked to the net transport of salt through the Strait of Gibraltar. Thus, we evaluate a reference salt transport through Gibraltar from the salinity trend of our reference climatologies (EN3 and IMEDEA reconstruction) taking NetSaltFluxref = Δsref . From this, we computed an averaged trend of about 1.7.10-3 psu.yr-1. The MEDRYS net salt flux is comparable whereas in NM12-FREE, it is almost twice larger. However, as we mentioned it, the net transport at Gibraltar is not representative of the evolution of the salinity in MEDRYS because of

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the addition of increments coming from the assimilation.

Reviewer: « P1839 L25 : "allow us" appears too strong since MLD and surface circulation variability were not presented. I suggest replacing with "suggest" or similar »

Answer : We agree.

Reviewer: « P1841 L12 : "We conducted" : I don't think it is correct to present preliminary tests in the "Summary and discussions". Idea and future plans should be included; preliminary results not corroborated by anything should be taken out. »

Answer : OK, it should be presented in an other way. We replaced the sentence by "This new parametrization will be also more efficient when strong wind occurs. For example, it should improve the assimilation of SLA in the Gulf of Lions during a Mistral event. We noticed that, at those particular dates, the configuration of the assimilation system were not optimal, the proportion of injected increment was too low, compared to other assimilation cycles."

Reviewer: « Table 1 : the fact that ALDERA has better net heat flux appears more as a compensation of errors in the individual heat components rather than an improvement. It should be discussed in the text. »

Answer : We agree with the reviewer. Over the 20-year period considered, ALDERA shows compensating errors between an overestimated shortwave and an overestimated latent heat loss when compared to the observation-based estimates (Sevault et al. 2014). This sentence has been added in the text when commenting Table 1. We are currently working on the following version of the ALADIN physics in order to understand the sources of the biases and to improve the model behaviour.

Reviewer: « Figure 14 is actually a Table and should be moved to Table 4 »

Answer : Actually, colors are not allowed in Tables. As we want to edit it with colors, in order to emphasize the good and the bad points of the different products, we preferred

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to move it to a Figure.

Technical points

We agree with all comments and the manuscript will be corrected.

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