

## ***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°2

Reviewer: « General comments :

The paper presents the description of MEDRYS reanalysis covering the time period October 1992 - June 2013 and its validation results, that considers also the NM-12 hindcast. A long part is dedicated to the description of the atmospheric forcing ARPERA, a dynamical downscaling of ERAInterim reanalysis from ECMWF. The paper presents old and not precise bibliographic references in the introduction and it does not mention the Mediterranean Sea reanalysis available through the Copernicus Marine Service. »

Answer: we agree that our paper should include more references and compar-  
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isons with the works from MFS group. To date, the new version of the MyOcean/MyOcean2/CMEMS Mediterranean Sea reanalysis is available online and we prefer to use published references. So, we re-wrote the paper including comparisons with the 1985-2007 Mediterranean reanalysis. The link between our paper and the one from Adani (JAOTECH, 2011) is actually strong, and some results put side by side are useful (see attached figures 1 and 2). In the same manner, the next MEDRYS version will be compared to the reanalysis from Copernicus.

Concerning the atmospheric forcing, the reviewer seems to mix two datasets (ARPERA and ALDERA) all along his/her review. ARPERA (Herrmann and Somot 2008, Herrmann et al. 2010) is now an old and well-published forcing for Mediterranean Sea models and we do not use it to force neither the MEDRYS reanalysis nor the twin free run. ARPERA is mentioned in the text only as a reference to underline the novelties of the ALDERA forcing. In addition, we would like to clarify that describing the dynamical downscaling techniques of ERA-Interim by RCMs (here by ALADIN-Climate) and demonstrating their added-value with respect to ERA-Interim or with respect to lower-resolution RCMs are not in the purposes of the current study. It is done in an extensive literature and we use the results of those previous studies when designing the ALDERA forcing applied for the MEDRYS reanalysis. The sections dedicated to the ALDERA forcing only target to describe the reanalysis air-sea forcing in details and to illustrate some of its characteristics (heat and water budget, small-scale features) that are important when driving long-term ocean simulations.

Reviewer: « There is no the objective of the paper in the introduction and what is its substantial contribution to scientific progress. »

Answer: we try to put forward in the new version the three following objectives: physical consistency of the state estimate: we want to produce a high-frequency meso-scale simulation coherent with scarce in situ and climatic signals captured by the satellites. We use atmospheric forcing and satellite sea surface temperature that are homogeneous all along the period. we try to progress in the closure of the mass, heat and

salinity budget. We aim to set atmospheric forcing, Black Sea runoff and Gibraltar transport that are coherent with satellite altimetry corrected by the Post-Glacial Rebound. It is still out of reach, but we made some progress with the salinity budget. we use a bias correction to get inter-annual signals that are less affected by model errors (vertical physics and turbulence closure in particular)

Moreover, we are aware that regional reanalyses of the Mediterranean Sea have been already carried out in the context of the SESAME and MyOcean projects (Adani et al. 2011, Pinardi et al. ). However we consider that this scientific field (regional reanalysis) is only at its starting point with still plenty open issues. We consider that exploring different experimental set-ups (forcing, assimilation scheme, etc) to carry out regional reanalyses is important in order to enhance the diversity of the method and to finally lead later to scientific progress.

Reviewer: « The scientific approach is superficial and applied methods are not described properly in a dedicated section but inserted in the analysis of the results. For these reasons the paper looks more like a technical report and I suggest to include some scientific analysis, that you already started as mentioned in the conclusion. »

Answer: The manuscript was a sum of various contributions. We now introduce methods and materials that are used. Some sections have been reorganised. We added comparisons with Adani (JAOTECH 2011) that are interesting (see attached figures 1 and 2)

Reviewer: « The part of the atmospheric forcing could be highly improved with a more precise explanation of the methodology you applied to validate ALDERA and its comparison with other atmospheric model outputs or observational datasets. »

Answer: As explained above, the purpose of the article is not a thorough analysis of the methodology leading to the ALDERA forcing and its evaluation. This has been done in previous articles by the regional climate model community as a whole including the CNRM team who has produced the ALDERA simulation. However, in the new version

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of the manuscript (section 2.2), we tried to improve the literature review concerning dynamical downscaling of atmospheric reanalysis and their use as Mediterranean Sea forcings. We also improve the description of the previous articles leading to define the ALDERA forcing in particular the comparison with other atmospheric model outputs and observational datasets.

The following text “In order to overcome the main deficiencies of the ARPERA2 dataset (relatively coarse spatial and temporal resolution, temporal homogeneity issue), we have developed a new forcing dataset for the MEDRYS reanalysis. This dataset called ALDERA is based on a dynamical downscaling of the ERA-Interim reanalysis (Dee et al., 2011) over the period 1979–2013 by the RCM ALADIN-Climate (Radu et al., 2008; Colin et al., 2010; Herrmann et al., 2011). We use here the version 5 of ALADIN-Climate firstly described in Colin et al. (2010). [...]” has been modified in: “In order to overcome the main deficiencies of the ARPERA2 dataset (relatively coarse spatial and temporal resolution, temporal homogeneity issue), we are using a new forcing dataset for the MEDRYS reanalysis. This dataset (called hereafter ALDERA) is based on a dynamical downscaling of the ERA-Interim reanalysis (Dee et al., 2011) over the period 1979–2013 by the RCM ALADIN-Climate (Radu et al., 2008; Colin et al., 2010; Herrmann et al., 2011). The dynamical downscaling technique is commonly used to overcome the lack of atmospheric regional reanalysis over sea and to improve locally the resolution of the air-sea forcing in areas dominated by small-scale atmospheric pattern as the Mediterranean Sea (Sotillo et al. 2005, Herrmann and Somot 2008, Beuvier et al. 2010, Herrmann et al. 2010, Herrmann et al. 2011, Josey et al. 2011, Beuvier et al. 2012a, Lebeaupin-Brossier et al. 2012, Solé et al. 2012, Vervatis et al. 2013, Auger et al. 2014, Harzallah et al. 2014). In ALDERA, we use the version 5 of ALADIN-Climate firstly described in Colin et al. (2010). [...]”

Solé J., Ruiz S., Pascual A., Somot S., Tintoré J. (2012) Ocean color response to wind forcing in the Alboran Sea : a new forecasting method. *J. Mar. Syst.* Vol 98-99:1-8. Doi: 10.1016/j.jmarsys.2012.02.007.

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Vervatis, V. D., S. S. Sofianos, N. Skliris, S. Somot, A. Lascaratos, and M. Rixen (2013), Mechanisms controlling the thermohaline circulation pattern variability in the Aegean-Levantine region. A hindcast simulation (1960-2000) with an eddy resolving model, *Deep-Sea Research Part I*, 74:82-97, doi:10.1016/j.dsr.2012.12.011 (<http://www.sciencedirect.com/science/article/pii/S0967063713000198>)

Auger P.A., Ulses C., Estournel C., Stemman L., Somot S., Diaz F. (2014) Interannual control of plankton ecosystem in a deep convection area as inferred from a 30-year 3D modeling study: winter mixing and prey/predator in the NW Mediterranean. *Progress in Oceanography*: 12-27, DOI: 10.1016/j.pocean.2014.04.004

Harzallah, A., Alioua, M., & Li, L. (2014). Mass exchange at the Strait of Gibraltar in response to tidal and lower frequency forcing as simulated by a Mediterranean Sea model. *Tellus A*, 66.

Reviewer: « However you did not prove the added value of introducing the ALDERA forcing instead of ERAInterim in NM12 results, but you only gave it as obvious. »

Answer: The goal of the paper is not to prove the added-value of the ALDERA forcing with respect to ERA-Interim or to previous forcing. We consequently changed the title of section 2.2.3 that was misleading: New title is "Illustration of the small-scale features in the ALDERA forcing". We also modified the last sentence of section 2.2 in "the same setting as ALDERA in order to illustrate the small-scale features of the 12km ALDERA forcing (see later comments for Tables 1 and 2 and Figs. 1 and 2)." Concerning the reanalysis air-sea forcing, our goal in the current manuscript is to describe the forcing chosen for MEDRYS, to describe the motivations behind our scientific choice, to describe the technical aspects leading to ALDERA forcing and to illustrate some characteristics of ALDERA that are important for Mediterranean sea modelling (heat and water budgets, small-scale features, trends). We consider that the added-value of high-resolution air-sea forcings for the Mediterranean Sea modelling has been shown in a series of published articles in particular concerning the wind field close to the coast

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and islands and the events of strong air-sea fluxes (Sotillo et al. 2005, Ruti et al. 2008, Herrmann and Somot 2008, Langlais et al. 2009, Beuvier et al. 2010, Elguindi et al. 2011, Josey et al. 2011, Herrmann et al. 2011, Lebeau-pin-Brossier et al 2011, 2012, Vrac et al. 2012). In addition, to our knowledge (we may have missed some very recent papers), no Mediterranean Sea modelling groups are using the ERA-Interim reanalysis in published literature as a forcing. Indeed ERA-Int has two main drawbacks: its low spatial resolution (80 km) and its known overestimate of the total net Mediterranean Sea heat budget with a long-term positive value as shown in section 2.2.1. We then naturally discarded ERA-Interim as a possible forcing for MEDRYS as most of the modelling groups involved in long-term simulations of the Mediterranean Sea.

Elguindi N., Somot S., Déqué M., Ludwig W. (2011) "Climate change evolution of the hydrological balance of the Mediterranean, Black and Caspian Seas: impact of climate model resolution" *Clim. Dyn.*, 36 (1-2), 205-228, doi: 10.1007/s00382-009-0715-4

Vrac M., Drobinski P., Merlo A., Herrmann M., Lavaysse C., Li L., Somot S. (2012) Dynamical and statistical downscaling of the French Mediterranean climate: uncertainty assessment. *Nat. Hazards Earth Syst. Sci.*: 12, 2769-2784, doi:10.5194/nhess-12-2769-2012

Reviewer: « The ocean model description lacks some fundamental detail. The paper misses a clear description of the validation methodology (one of the topics of the paper) that is hidden between the lines of the results. The data sets considered for the data assimilation and the validation are not the state of the art of the available data sets in the Mediterranean Sea. The author might improve the quality of the paper at least using them for the validation. The use of a low quality climatology computed for 1998 as a reference for the MEDRYS validation is weak and does not improve the analysis results. The sections about the high frequency validation should be more robust considering different stations in the basin and covering a longer time period. It is hard for the reader to follow since there are many missing details given as obvious. In many parts there are repetitions that denote a superficial approach. The repartition in sections

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and subsections might be simplified. Many figures might be improved in the quality or redone excluding data that are not statistically significant. Captions contain details that are missing in the text. All the figures and subplots should be introduced in the text and motivated before starting the analysis of their content. Overall the english is good quality even if there are some typing errors that should be corrected. I suggest a major revision of the paper before publication. »

Answer : We added missing details like the rivers runoff. The methodology for the validation has been separated and it appears in a new section. MEDATLAS is not recent but is not such a poor reference since MEDRYS does not deviate a lot from it. Adani's paper also uses it so it is good to stay with it for this version of MEDRYS. If you think about a particular product, please let us know. Considering the in situ data, we rely on CORIOLIS data center to provide it. Again, if you think about a better data, let us know. For the satellite sea surface temperature, we plan to use MyOcean product in the next reanalysis. If you have a better idea, please let us know. The section with the Lion mooring is re-written. It does not demonstrate anything general. It simply illustrates the range of temporal scales that can be simulated by MEDRYS. It is indeed the only high resolution mooring that we have. We would be happy to use more data if available.

Specific comments :

Reviewer: « page 1817, lines 7-8: heat loss reference is really old, more recent papers should be referenced. Mariotti 2008 is not the right citation for the the fresh water budget since I could not find it anywhere. Please revise this part of the introduction with updated literature. (Later you use other references) »

Answer : the reference value for the heat loss is indeed old, but it has never been changed since, and we wanted here in the very beginning of the article to describe the long-term behaviour of the Mediterranean Sea; more recent references (such as Sanchez-Gomez et al 2011 and Sevault et al 2014 mentioned in the article) are referring to shorter periods, but they are still in agreement with the [-6.5 ; -3.9] W.m-2 esti-

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mate given by MacDonald et al. 1994, or even with the [-10 ; -4] W.m-2 estimate from Béthoux 1979. The paper of Mariotti et al 2008 (Mediterranean water cycle changes: transition to drier 21st century conditions in observations and CMIP3 simulations, Environmental Research Letters) deals indeed with freshwater budget estimates and is available online (<http://iopscience.iop.org/article/10.1088/1748-9326/3/4/044001/pdf>).

Reviewer: « page 1817, lines 11-25: This part of the introduction is very superficial and not pertinent with the paper. You say : "we must improve our understanding of the water cycle ..." but the paper does not. »

Answer : This sentence is probably too general for our purpose. we should have mentioned that this part is describing the HyMeX international program objectives, in whose framework this work is done. By giving a best-as-possible combination of observations and 4D modelling, an oceanic reanalysis is expected to give a reference for the evolution of the heat and salt contents of the Mediterranean Sea, giving thus the oceanic part of the regional water cycle. This goal of the reanalysis has been mentioned in the revised version of the paper.

Reviewer: « page 1818, lines 7: "Several techniques...." Which ones? Insert some reference here. Please explain better why regional reanalysis are challenging because observational data sets are scarce(r?). You should also specify that the high resolution model needed for the surface fluxes are atmospheric models. The advent of atmospheric reanalyses covering a long time series continuously updated to present time has been a first limiting factor together with others that you should mention. »

Answer : We were thinking about OI and variational techniques, and not reconstruction. We added some references. "Regional reanalysis are challenging because of scarce data" sounds strange as if it is OK for a global reanalysis. We meant that we go to thinner scales with a regional domain, and it is less and less possible to control these scale with the same amount of old scarce data. This is the problem consistency that we tried to outline in the new manuscript.

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Reviewer: « All MyOcean/Copernicus regional reanalyses (exempt the Baltic) did not use high-resolution atmospheric forcing and were accepted. »

Answer : We agree that the current generation of regional ocean reanalyses does not use high-resolution air-sea forcing. Based on our previous experience, we however think that this is an important point to test at least for the Mediterranean Sea and it is one of the novelty we would like to propose in the current study.

Reviewer: « page 1819, lines 8-20: Oddo et al. 2009 does not present any reanalysis. The author should not cite a paper without reading it. What follows should be revised. You did not mention the Mediterranean Sea reanalysis available in Copernicus since April 2014, I suggest to include it here. »

Answer : the way we formulated this sentence was indeed misleading; the Mediterranean reanalyses done in the framework of MyOcean projects were initiated by the work done on the operational configurations in the framework of the MFS program, that is why we quoted Oddo et al 2009.

Reviewer: « page 1819, lines 25-26: ...“giving a close resolution to of NEMOMED16”. What is NEMOMED16? Please revise the english. »

Answer : We agree with the reviewer that it is not clear. NEMOMED16 is not defined in the current paper and we modified the sentence in : “Our ocean model used is NEMOMED12 (Beuviel 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  $1/12^\circ$  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  $1/16^\circ$  regular horizontal grid used in Adani et al. (2011).”

Reviewer: « end of page 1819 The time consistency of the atmospheric forcing is not the only factor that determines the time consistency of an ocean reanalysis, please

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revise this part. What is the improvement of MEDRYS reanalysis with respect of the previous Mediterranean Sea reanalysis? Considering your approach in the introduction you might include some considerations in the conclusions. What is MEDRYS added value? »

Answer : The sentence (P1819 L29) has been modified in “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 the most as possible sources of inhomogeneities in MEDRYS. This reanalysis then allows a better description of the interannual to decadal variability of the Mediterranean circulation and trends.” Regarding the added value of MEDRYS, in the international context, a new system with a different assimilation scheme, different assimilated data, different forcings and/or different ocean model (see general comment) is already a new product. Having different options can provide a better framework to understand and improve in the future the products.

Reviewer: « page 1820 The title “Experimental set up” is not appropriate you never talk about experiments. »

Answer : We follow common plans as it can be found in Adani et al. (2011) for example. We think it is acceptable.

Reviewer: « page 1820 Why did you title a section “Ocean model configuration: NM12-Free” to describe your reference hindcast? Up to now you mentioned this hindcast only in the abstract. Is the MEDRYS model configuration the same of the free run? »

Answer : The reviewer is right : the fact that MEDRYS and its associate free run are using the same oceanic configuration is missing. This has been added, as well as a justification about why showing results of the free run in this paper.

Reviewer: « What are the lateral boundary conditions? Is it the model nested within a

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global model, how? Please revise this section and include some more details that are not obvious for the reader. »

Answer : the sentence “The exchanges with the Atlantic basin are performed through a buffer zone” was maybe not clear enough. We modified it in the new version with “As lateral boundary conditions and to represent the exchanges with the Atlantic ocean, a buffer zone is used: from 11 to 7.5 °W, 3D temperature and salinity fields are relaxed towards ORAS4 monthly fields”.

Reviewer: « page 1821 line 9 What is MEDATLAS1979? How did you compute it? I believe that a citation of Rixen et al 2005 here is not enough since you computed a different product. What is ORAS4? How did you blend the two data sets? The following description of the buffer zone is not clear to the reviewer, could you please specify it? »

Answer : We agree that the statement is vague. The MedAtlas monthly climatology (MEDAR/MEDATLAS Group, 2002) has been combined to the 3-years filtered inter-annual fields from Rixen et al. (2005) to produce the initial state of NM12-FREE in October 1979. The use of the 3-year filtered product has been motivated following Rixen et al. (2005). This methodology has been used in order to reduce the impact of large spatio-temporal gaps in the data distribution. The ORAS4 fields, used both for initialization and relaxation in the buffer zone come from a global ocean reanalysis produced by the European Centre for Medium Range Weather Forecast (ECMWF) covering the 1958-2013 period. We propose to rephrase the two paragraphs (1820 L21 to 1821 L18) as it follows :

“The NM12 configuration covers the whole Mediterranean Sea and a buffer zone including a part of the Atlantic basin, but not the Black Sea. The horizontal resolution is 1/120° and corresponds to a varying grid cell size between 6 and 7.5 km (the distance between two points varying with the cosine of the latitude). NM12 has 75 vertical stretched z levels (from  $\Delta z = 1$  m at the surface to  $\Delta z = 135$  m at the bottom, with 43

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levels in the first 1000 m) in a partial step configuration. The bottom layer thickness is varying to fit the bathymetry (Mercator-LEGOS version 10 bathymetry at 1/120° resolution). The no-slip boundary condition is used and the conservation of the model volume is assumed. The mean tidal effect of the quadratic bottom friction formulation computed from a tidal model (Lyard et al., 2006) has been taken into account leading to significant additional bottom friction in the Strait of Gibraltar, Channel of Sicily, Gulf of Gabes and the northern Adriatic sub-basin. As a lateral boundary conditions and in order to represent the exchanges with the Atlantic ocean, a buffer zone is used : from 11° to 7.5°W, 3D temperature and salinity, as well as the Sea Surface Height (SSH) fields are relaxed toward ORAS4 global ocean reanalysis monthly fields (Balmaseda et al. 2013), produced by the European Centre for Medium Range Weather Forecast (ECMWF). For temperature and salinity, the restoring term in the buffer zone is weak west of Cadiz and Gibraltar areas and increases westwards. As the Mediterranean Sea is an evaporation basin, the model volume is conserved through the damping of the SSH in the buffer zone toward prescribed SSH anomalies with a very strong restoring. The SSH from ORAS4 is set in the Atlantic according to a strong damping with a very small characteristic time-scale ( $\tau = 2$  s).

The free simulation NM12-FREE starts in October 1979 and ends in June 2013. In the Mediterranean side, initial conditions are provided by a monthly mean potential temperature and salinity 3-D fields based on the MedAtlas climatology (Rixen et al., 2005). A field representing the state of the Mediterranean sea in October 1979 has been produced combining the MedAtlas monthly climatology (MEDAR/MEDATLAS Group, 2002) to the 3-year filtered interannual fields from Rixen et al. (2005). Following Rixen et al. (2005), the filtered interannual product is used in order to reduce the impact of large spatio-temporal gaps in the data distribution. In the buffer zone, potential temperature and salinity are initialized from ORAS4 global ocean reanalysis fields in order to maintain consistency with the relaxation. In the initial condition fields, a linear transition between 7.5°W and 6°W is applied between the ORAS4 and the MedAtlas fields.”

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Reviewer: « page 1821 line 19 How many rivers did you implement totally? What does it mean that the dataset of Ludwig is split in two parts? ...“the others (how many?) rivers are gathered in each sub-basin (which one?)...The Black Sea is not included in the NM-12-FREE configuration but...” What about the MEDRYS? The Marmara Sea is in your domain? Please explain better. »

Answer : Concerning the Black Sea, there is here a typo. In this section, we speak about the configuration of the ocean model NM12. This configuration is common to both simulations NM12-FREE and MEDRYS (see comment above). Moreover, we need to be more precise on how we implement rivers in the NM12 configuration. We rephrased it as it follows : “The 33 main rivers of the NM12 domain are added as precipitation at mouth points (29 in the Mediterranean Sea and 4 in the buffer zone). As the Ludwig et al. (2009) dataset consists in 239 mouth points : the inputs of the 210 other rivers in the Mediterranean basin are gathered as a coastal runoff in each subbasin (following the same dividing as in Ludwig et al. 2009). Until 2000, we use the interannual values from Ludwig et al. (2009) and then the climatological average representing the 1960-2000 period is used. The Black Sea is not included in the NM12 configuration but taken into account with a monthly average one layer net flow across the Marmara Sea and the Dardanelles Strait. We assume that the flow is a freshwater flux (Beuvier et al., 2012a). Until 1997, we use the interannual values from Stanev et Peneva (2002) and then the climatological average representing the 1960-1997 period is used. ”

Reviewer: « page 1822 line 20 What are bi-periodization (11) and the relaxation (2x8) zones? »

Answer : The relaxation zone is the name given in the limited-area models to the coupling zone in which the large-scale forcing is imposed to the regional model. In ALADIN-Climate, it is a 8-point wide zone. The bi-periodization zone is more specific to limited area models with spectral dynamical core as ALADIN and is necessary to compute the Fourier Transform. Those information are probably not relevant in the

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context of this article and are redundant with previous publications. We then simplified the text in: “For the model definition, we used a Lambert conformal projection centred at 14 E, 43 N for pan-Mediterranean area at the horizontal resolution of 12 km with 405 grid points in longitude and 261 grid points in latitude excluding the coupling zone.”

Reviewer: « page 1823 lines 5-6 ..“ the sea ice limit (Black Sea)...” what does it mean??? Why sea ice is needed? The reader should not go on the MEDCORDEX webpage and look for the definition of the cordex domains and then the Mediterranean. A figure of the MEDRYS and ALDERA domains with LION buoy location and others geographical references mentioned in the text would be useful for the reader. »

Answer : We don't really understand the point of the reviewer. Atmosphere models need surface lateral boundary conditions over sea. For ALADIN-Climate, this includes SST, sea ice limit and temperature over the sea ice. Over the studied area, only the Black Sea and the Baltic Sea can show sea ice covered areas in Winter. We remove the words “(Black Sea)” from this sentence in order to avoid misinterpretations.

Reviewer: « page 1823 lines 10-end What is the bulk formula used in ARPERA2? ... »

Answer :We have added the reference of the bulk formula: “[...] same bulk formula as in ARPERA2 (Louis, 1979) to compute the turbulent [...]”

Louis, J.-F. 1979. A parametric model of vertical eddy fluxes in the atmosphere. Boundary Layer Meteorol. 17, 187202.

Reviewer: « “To our knowledge...” I would avoid this kind of statements. I would use consistent instead of “homogeneous” which is misleading. »

Answer :We remove the sentence “To our knowledge, ALDERA is the longest and finest homogeneous atmospheric forcing available for the Mediterranean Sea.”

Reviewer: « Which conservative interpolation scheme? »

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Answer :For the ALDERA fluxes we perform a 2 nearest-neighbour interpolation weighted by their distance and a gaussian function. For this version we voluntarily chose not to smooth the fields at all.

Reviewer: « The last two sentences are not pertinent here, moreover you mention 2 tables and two figures without any explanation. »

Answer :The sentence “All the ALDERA outputs are openly available through the Med-CORDEX database ([www.medcordex.eu](http://www.medcordex.eu))” has been removed from this section of the manuscript and put in the acknowledgments. The second sentence is important to introduce some of the later results and we decided to keep it with some modifications. “Within the frame of the Med-CORDEX initiative, the RCM ALADIN-Climate is also run at lower spatial resolutions (150, 50 km) with exactly the same setting as ALDERA in order to illustrate the small-scale features of the 12km resolution model with respect to lower resolution models (see below comments on Tables 1, 2 and Figs. 1, 2.”

Reviewer: « Section 2.2.1 should start with the second paragraph, which is the continuation of the previous section! I suggest to describe at the beginning the data sets you consider and the methodology. I suggest to revise all this section and the table captions, they contain informations that should go in the text. »

Answer : Section 2.2.1 has been reorganised, starting with “Tables 1 and 2 compare the spatially and temporally averaged values of the Mediterranean Sea surface heat and water budget terms of the ALDERA forcing with past studies and observed-based references (flux are positive downward in W/m<sup>2</sup> and mm/day). ALDERA shows values within the range [...]”. Following a comment from the other reviewer, we also add the following sentence when commenting Table 1: “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)” After re-reading section 2.2.1, we decided to keep it short with the description of the main results and let the side information in the Table labels for

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the readers interested in this specific topics.

Reviewer: « Section 2.2.2 The author statement at the end of page 1824 and the beginning of page 1825 is approximative maybe some analysis could be added. An introductory phrase to justify the sub-section title could help the reader to follow. I cannot understand how the phrase ending at line 5 of page 1825 is linked to the previous one. At line 6 you talk again about inter annual variability of the heat flux components. This is confuse, there are numbers that the reader cannot follow, are you still referring to table 1 or 2? Please revise it. »

Answer :As no new results are exposed in the first sentences of section 2.2.2, we decided to remove the first paragraph of section 2.2.2 and to change the title if the section in “Interannual variability and trends” The section 2.2.2 now starts with a clearer first sentence: “At the basin scale, the interannual variability of the various terms of the Mediterranean Sea heat budget can also be evaluated for the period 1985-2004 of the reference dataset of Table 1 (Sevault et al., 2014).”

Reviewer: « page 1825 lines 17-end You introduce the trends in the last sentence of the section, without any explanation of the methodology or without showing any plot. I suggest to insert some plot to compare ARPERA fluxes components to ERAInterim, which is the parent model. »

Answer :Following the reviewer comment, the paragraph concerning the trends has been reorganised. However we didn't add dedicated figures as figures are rarely informative concerning trends. Values of the long-term trends and test of significant level are more relevant. The new paragraph is: “Trends in the surface forcing are relevant in long-term simulations as they can induce long-term trends in the water mass characteristics. Concerning the surface heat flux terms in ALDERA, only the trend in latent heat flux is significant with an increase in the heat loss by the sea equal to +4.1W/m<sup>2</sup>/decade over the 1979–2012 period. This trend is similar to the one obtained in Mariotti et al. (2008) and is mostly driven by the SST trends (Sevault et al. 2014). Note that ALDERA

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does not include the observed trend in European anthropogenic aerosols and therefore does not reproduce the shortwave trend identified in Nabat et al. (2014).”

Reviewer: « Section 2.2.3 This section is superficial. The last phrase is in contrast with the title and maybe it is the only needed. I'm not sure about the usefulness of both figure 1 and figure 2, they show the same thing. »

Answer :We changed the title of section 2.2.3 in “Illustration of the small-scale features in the ALDERA forcing” in order to be more consistent with the fact that we illustrate the ALDERA small-scale features but we don't target to demonstrate its added-value. The goal of this section is twofold (1) to use the published literature to justify the use of dynamical downscaling of reanalysis to force Mediterranean Sea model and (2) to illustrate the ALDERA small-scale features with respect to lower resolution twin runs. We clarified the literature review in the new section 2.2.3 of the article. Besides, we feel that figure 1 and 2 are necessary as they illustrate the ALDERA forcing in two regions that will be studied later in the paper, the North-Western Mediterranean Sea for the WMDW formation and the Cretan Arc for the Ierapetra eddy. Text has been modified in: “Over the Mediterranean Sea, the added-value of high-resolution models has been shown in particular concerning the representation of the heat and water budget terms (Elguindi et al. 2011, Josey et al. 2011), of wind field especially close to the coast and islands (Sotillo et al. 2005, Ruti et al. 2008, Herrmann and Somot 2008, Langlais et al. 2009, Herrmann et al. 2011, Vrac et al. 2012) and of the events of strong air-sea fluxes (Herrmann and Somot 2008, Béranger et al. 2010, Lebeaupin-Brossier et al. 2012). Dynamical downscaling of reanalyses have therefore been used to force long-term hindcast simulations (Beuvier et al. 2010, 2012, Herrmann et al. 2010, Solé et al. 2012, Vervatis et al. 2013, Auger et al. 2014, Harzallah et al. 2014). Figure 1 illustrates the role [...]”

Reviewer: « Section 2.4 Why didn't you use high resolution SST products specific for the Mediterranean Sea free available in Copernicus marine service catalogue? You developed a 12km horizontal resolution atmospheric forcing but you assimilated

C1184

a 0.25deg resolution SST which is well known in literature it is not the best product for the Med Sea, please justify it. The Copernicus marine service provides delay mode and reprocessed satellite SST datasets specific for the Mediterranean Sea that have been created for reanalysis and validation purposes, why didn't you consider them neither for assimilation and validation purposes? »

Answer : We used Reynolds data in global for many years because of its overall quality, and its coherence with previous ECMWF reanalysis. It is also homogeneous in time. Hence, we used it here again in MEDRYS so we could compare with our global reanalysis GLORYS. Thanks to this, we realised that we had to adapt the Reynolds grid to the Mediterranean Sea. Reynolds data is probably too smooth in space and time for this region, but it fulfills the both conditions of being available from the beginning to the end of our simulated period, and is extended in near real-time. In the next reanalysis, we plan to use the MyOcean OSTIA data. Our constraint is that the product must be global and available in real time because a real time extension of MEDRYS is under debate. For the next MEDRYS, we plan to use an additional dataset like Marullo (2007), at least for validation in a first step.

Reviewer: « page 1828 line 11 Which specific along-track SLA did you use? Reprocessed, delay mode, real time? Please specify better which data set you used, there are many available. »

Answer : 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.avisio.altimetry.fr>) in the framework of MyOcean project, has been assimilated in MEDRYS. These products include along-track filtering (low pass 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]

C1185

period. Names and acronyms used in this paper as well as the measurement period of each satellite are summarized in Table 3.”

Reviewer: « page 1828 lines 18-end Why didn't you use the existing specific and higher resolution MDT for the Med Sea? Ex. Rio et al 2007, 2014? Your product is at 1/4 of a degree, very smooth. Please justify it. I checked the CNES-CLS09 documentation and it looks like it does not contain the Mediterranean Sea, please see AVISO website here and clarify. What is GOCE? Which reanalysis data are used to adjust your mean surface reference? »

Answer : Sorry, this was not detailed enough, and it gets very confusing. The mean surface reference used is a 1/12° hybrid global product between the global CNES-CLS09 MDT 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 & al, 2013). There is a similar procedure in Adani (2011). This clarification has been added.

Reviewer: « page 1829 line 5 How many data have been discarded from observation thinning, which is the percentage referred to the overall dataset? Why did you apply a data thinning? »

Answer : the data thinning applied on the CORA4 database (for each platform, only one profile per day and within a 0.1° is kept) is done in order to not over-constrain the data assimilation system, and moreover with correlated and redundant observations. It mostly concerns high-frequency data, such as hourly data from the tropical TAO moorings, from gliders or from sub-surface drifting buoys . The same method is applied for the global reanalyses performed with the SAM2 data assimilation system; over the 1990-2012 period and with respect to the whole CORA4.0 database, about 13% (for T) and 30% (for S) of the vertical profiles are kept after the data thinning; these relatively small values mainly come from the last years, during which a huge amount of high-frequency profiles are in the CORA4.0 database. These values increases up to 53%

C1186

(for T) and 46% (for S) when the profiles coming from moorings and drifting buoys are not considered (and again, values are smaller in the most recent years). We can assume that the same values of amount of data kept after the data thinning can be applied to the Mediterranean area.

Reviewer: « page 1829 line 10 How many observations have been discarded from the further SAM2 check? Which climatology do you use? Seasonal climatology check might discard a lot of useful observations. »

Answer : on average over the whole reanalysis, 79 observations of temperature per year and 16 observations of salinity per year are rejected by the supplementary quality control performed by SAM2. Observations are here part of vertical profiles; on a same profile, observations can be kept or rejected. The quality control uses informations from the differences between the observations and the MEDATLAS-II climatology, as well as differences between the observations and the model; the error values, which determine if an observation is kept or not, depend on the month, the location and the depth.

Reviewer: « Section 3 I believe that the first sentence is not appropriate for a scientific paper. Which is the methodology you applied? You should also start saying that you use the hindcast in your validation. The title of the section is Validation and scientific assessment, but you start immediately talking about the assimilation statistics. Innovation is not the observation minus the forecast, could you please insert a formula? Then you mention Crosnier and Le Provost paper, containing Class1-Class4 diagnostics developed in GODAE. You should describe which one you use and you are going to present together with the formula and their meaning. Please insert here a comprehensive description of the validation methodology and the reference data sets you use and why. »

Answer : OK, we re-wrote this section so that one can get a clearer idea of the set of diagnostics before they are discussed. For the innovations and other formulae, we will

C1187

remind them or refer to Ide & al. ("Unified notation for data assimilation, operational, sequential and variational, JMSJ, 1997) We re-titled section 3 "Validation methodology and scientific assessment" and divide it in two sub-sections "3.1 Validation methodology" and "3.2 Scientific assessment".

In the new 3.1 sub-section we added, as you requested, the validation methodology used in this paper: "During the MyOcean project, scientists have defined validation metrics by region and type of product, including observational products. Many efforts were made to synthesize and homogenise quality information in order to provide quality summaries and accuracy numbers. All these rely on the same basis of metrics that can be divided into four main categories derived from Crosnier and Le Provost (2007). The consistency between two-system solutions or between a system and observations can be checked by "eyeball" verification. This consists in comparing subjectively two instantaneous or time mean spatial maps of a given parameter. Coherent spatial structures or oceanic processes such as main currents, fronts and eddies are evaluated. This process is referred to as CLASS1 metrics. The consistency over time is checked using CLASS2 metrics which include comparisons of moorings time series, and statistics between time series. Space and/or time integrated values such as volume and heat transports, heat content and eddy kinetic energy are referred to as CLASS3. Their values are generally compared with literature values or values obtained with past time observations such as climatologies or reanalyses. Finally, CLASS4 metrics give a measure of the real time accuracy of systems, by calculating various statistics of the differences between all available oceanic observations (in situ or satellite datasets before data thinning and online quality check) and their model equivalent at the time and location of the observation.

The validation procedure thus involves all classes of metrics. It checks improvements between versions of a system, and ensures that a version is robust and its performance stable over time.

In the following, results from both NM12-FREE and MEDRYS (daily outputs for all C1188

variables and additional hourly outputs for sea surface variables) are then presented. We first focus on the assimilation of SLA data (only for the reanalysis) and its impact on surface circulation. Then, the assessment of the interannual variability is made using integrated heat and salt contents. The high frequency surface variability is presented through a comparison to a fixed mooring in the Gulf of Lions. Finally, we show the effect of the assimilation in terms of transports through the Strait of Gibraltar."

Reviewer: « page 1829 last phrase I believe that you cannot say that you assess a 21 years of reanalysis at high frequency only showing 2 months of hourly data (since one month of data are not trustable due to sensor failure) at one location in the Mediterranean. This is a weak part of the paper, I suggest to add more stations in different part of the domain and provide some comprehensive statistics. »

Answer : Yes, it is poorly said. The purpose was to illustrate the range of phenomenons that could be reproduced by MEDRYS, from hourly to inter-annual. This mooring is important regarding HYMEX. We would be happy to put more comparisons, in particular in the Eastern basin. Yet, this is the only high frequency mooring data that we have. If you know some available data, please let us know.

Reviewer: « page 1830 lines 5-16 Could you explain better why the linear decrease of the mean innovation should be related to your volume correction? Could you explain why the seasonal signal should be related to the runoff? What do you mean that the RMS of observations is 8 cm? Which observations, referred to what? »

Answer : The reason is that the dynamic height (HDYN) is controlled by the assimilation, and not the volume. The residuals are small as can be guessed from the innovation in figures 5 and 6. The third possibility can be a change in barotropic motion and height (HBAR), but it appears to be small, especially on average over the basin. We diagnose the change in volume by using the change in SSH, HDYN and HBAR. We can add this figure if you think it is useful. We tried to make this section clearer anyway. We do not know what is responsible for the seasonal cycle of the innovation. It can be

a pressure issue in the buffer zone (a seasonal in ORAS4), or an issue with the water budget. In the later case, the climatological runoff (after 2000) is a good candidate. We actually suspect the runoff from the Black Sea to be the major contributor for this fault. We will test these hypothesis in the future new version of MEDRYS, but we do not have the answers yet. The 8cm RMS of the observations is the RMS of the along track SLA data that is assimilated in the system. The objective is to give a reference for the 6.5cm RMS in one week forecast innovations of SLA. Filtering or binning, or data rejection on the shelf can actually reduce the RMS of the SLA used in the system. So, it does not say much but everyone can compute the reduction in RMS brought by assimilation.

Reviewer: « page 1830 line 20 You are showing that almost none in situ SST value has been assimilated, why??? Is this because of your thinning procedure or the climatological check? page 1830 line 25-27 If the statistics are not significant before 2005 why do you show them? I recommend to split Fig.4 for satellite and in situ validation. The bias between in situ and satellite data could derive from your SST satellite data correction? Before applying the satellite SST correction did you compare it with CORA4 in situ data? »

Answer : The plots comes naturally from the validation of the system. Of course, we could hide what is not significant, but we prefer to show everything and say when is not significant. This was beneficial with your review since you noticed that the number of in situ data is very small at the surface. This is because the present figure 4 concerns in situ observations from vertical profiles that are between the surface and 1m-depth and these are not numerous in CORA4 (see attached figure 3). The 0.12°C difference between in situ and satellite is due to a network effect. The regions where MEDRYS has a cold bias (between Greece and Turkey, East and South-West of Crete. . .) are less sampled than the region of warm bias. Hence, this sentence has little interest, and it will be dropped. Reynolds SST includes a bias correction with in situ. Careful comparisons between SST and in situ are difficult because of night/day skin effects.

C1190

To date, we relied on Reynolds products for consistency because we never noticed significant gaps. We will use Copernicus SST in the future.

Reviewer: « page 1831 lines 3-4 The statement that there are few profiles deployed in the Mediterranean Sea before 2005 is wrong and misleading, especially if not supported with numbers. You should again explain why your assimilation or preliminary quality check or thinning procedure discard almost all in situ data. »

Answer : When we say that few profiles were deployed in the Mediterranean Sea before 2005, it is relatively to the north Atlantic or the Pacific. The average number is shown in Fig 5 and 6 (top). This does not reflect the distribution of the observations, which is very uneven in space and time. For instance, there is hardly any data along Lybia and Egypt, especially in summer.

Reviewer: « page 1831 I would avoid repeating the word innovation, mean innovation should be the MEDRYS bias. I suggest to revise this part that is confusing and superficial. What are the maximum and minimum temperature RMS values? FIG 5. is misleading since the color palette is saturated and hides the maximum values. RMS should be reasonable with the bias by default, what about the relationship with the specified observation error? Please explain it better. Did you consider the significance of the statistics? »

Answer : The word "bias" is often associated to the model. The seasonal signal of the innovation in the top 100m clearly corresponds to a lack of mixing in late summer, which is a model bias (turbulence closure). Trends in the innovation correspond for a good part to the inaccurate water and heat budget closure. It is not strictly a model bias. This is why we would prefer, if you mind, to avoid the word "bias", until the closure problem is solved. The maximum RMS (not shown on the submitted version of figures 5 and 6) has been added. Over the whole period, it reaches 1.38psu for salinity and 5.45°C for temperature. We do not consider the significance of the statistics, because we prefer to show everything, even if data is rare. At depth, there are not enough data over a cycle

C1191

to be significant. Yet, the data aggregation over a longer period can provide valuable information. Of course, a real analysis of the signals at depth needs a proper analysis, like in Purkey & al., (J. of Climate, 2010). The very simple figures 5 and 6 give a hint on that. We have also added two more figures similar to Adani's figures 8 and 9. Most signals at depth are coherent between the two reanalysis and we will stress that point.

Reviewer: « Section 3.2 Why did you select a global product at 1/4 of a degree to validate the model MSSH? Moreover you use CNES-CLS09 in your assimilation of SLA, thus this is not a validation but rather a verification of our assimilation procedure. I believe you should repeat this part with a Med Sea specific product. Moreover you spend 14 lines talking about the free run and just 3 for the MEDRYS. I suggest to focus on the MEDRYS which is the topic of the paper. »

Answer : The idea behind this section was to show the strong impact of the assimilation on the mean circulation. The use of the original CNES-CLS-09 (not the hybrid) is logical because this field was used here and there, and it serves as a reference. Yet, the difference of the reference period makes things too complicated. So, we dropped the figure 7 and concentrate on the currents.

Reviewer: « page 1833 line 17 The author should introduce Fig.8 and the analysis of the mean kinetic energy. The outcome is in lines 25-26, the assimilation introduces smaller scale features increasing the energy, as expected. »

Answer : OK

Reviewer: « sub-section 3.2.2 The author should introduce EN3 and IMEDEA reference data sets in section 3. EN3 gridded products is not climatological, please clarify it. Why didn't you use EN4? What is IMEDEA reconstruction? The reference is not in the bibliography and the reviewer did not find it on the web! Lines 8-9-10 are a repetition and demonstrate a superficial approach. Fig. 9-10 starts from 1980, why? I suggest to focus on the MEDRYS time period which is the topic of the paper. Did you consider the mapping error? The author should investigate more deeply the salinization in early

C1192

2000s, is it due to stronger evaporation, as you state at line 1 of page 1834 or is it a spurious positive anomaly (line 8) due to the fact that in 2000 you start assimilate a lot of temperature profiles? What about the temporal consistency of MEDRYS since the number of data assimilated varies enormously during the considered time period? Considering what you say at line 15 I believe that your validation on the deepest layer is not solid. »

Answer : We agree with the reviewer, both EN3 and IMEDEA are not climatologies but hydrographic gridded products. About the missing reference, the reviewer is right as well, as the paper on IMEDEA is not submitted yet. Finally, concerning EN4, at the time of doing this study we were not aware of the existence of this new product. We propose to modify the first paragraph 3.2.2 as follows : "Integrated temperature and salinity from two hydrographic products are compared with MEDRYS and NM12-FREE. The two products are EN3 (Ingleby and Huddleston, 2007) and IMEDEA (Jordà et al., submitted paper). Both products differ in the details of the mapping algorithm and the quality control applied to the observations. The difference between them can be viewed as a first estimate of the uncertainties linked to the observational products, which cannot be neglected (Jordà and Gomis, 2013 ; Llasses et al., 2015). Basin integrals of the various products are compared whatever real data is present or not. Monthly evolution over three different layers representing surface (0-150m), intermediate (150-600m) and deep (600m-bottom) waters are shown in Fig. 9 and 10."

Considering the mapping error, to infer the error of integrated quantities from the error maps is not straightforward. The error covariances among different locations would be required. In the IMEDEA product, the errors have been estimated using synthetic data and comparing directly the basin average quantities.

We chose to show Figs 9 and 10 starting in 1980 since we want to show the behaviour of the free run as well, so we start the figure from the beginning of its simulation period. Moreover, this gives useful information for potential users of NM12-FREE.

C1193

The salty bias at surface 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 tends 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. As MEDRYS presents a lack of stratification due to biases issue, we think that a dense water anomaly is advected toward the bottom. Moreover, as we did not find any salt anomalies neither in the CLASS4 validation exercise, nor in the reference hydrographic gridded 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.

As we said in the answer to a general comment, the varying number of assimilated in-situ data is an inherent problem of reanalyses.

Reviewer: « sub-section 3.2.3 Here you introduce a MEDATLAS1998 climatology computed over three years of time period as a reference product for a MEDRYS validation. I believe that this is not a good reference field, since it has to be very smooth due to the lack of data. What's the meaning of this choice? I would avoid this and concentrate on the CLASS4 diagnostic analysis. Could you briefly specify in the validation methodology what is a CLASS4 diagnostic and the different meaning with the innovation diagnostic previously presented? The formulas are welcome. »

Answer : as for the initial state and following Rixen et al. 2005, we built here with the MEDATLAS-98 data fields representative of the state of the Med Sea at the end of the 1990's. The comparison with a climatology is a way to evaluate if the reanalysis improves the results with respect to a constant state, so we think it is useful. CLASS4 diagnostics are now described in the "Validation methodology" sub-section (see before).

C1194

Reviewer: « page 1835 line 22 You say that MEDRYS is very close to observations, what is the mean MEDRYS RMSD? CLASS4 is meant to measure the performance of the MEDRYS and its capability to reproduce the ocean state consistently with observations. Does it? Which observations did you use for this validation? This should go in section 3 or at least at the beginning of this subsection. Each figure and subplot should be introduced, referenced, motivated and analyzed. Please revise the text accordingly. The text is confusing the reader, I suggest to describe fig 11, temperature bias and RMS then fig. 12 salinity bias and RMS. I suggest to introduce a table summarizing mean bias and RMS with the relative standard deviations to demonstrate the MEDRYS performance. A comparison with Copernicus reanalysis results could be useful for the reader and to show the added value of MEDRYS. Why the number of data is so high ( $10^4$ ) in the deep layer? Are those sample, profiles? Numbers are not clear please enlarge the font. »

Answer : The observations used for the computation of the CLASS4 diagnostics are the entire CORA4 database, before data thinning and online quality check (see specific comment). We agree with the reviewer: we have precised that the numbers of data do not correspond to profile numbers but to sample numbers. However, the comment of the reviewer regarding the fact that the Fig.11 and Fig.12 are neither introduced, nor referenced, motivated or analyzed is subjective. We think that adding a table with numbers would not give any supplementary information. A comparison with other reanalysis products is beyond the scope of this study.

Reviewer: « page 1836 lines 10-13 Your results suggest that the salinization is not in the gridded reference products (which are at this point) but in the dataset you use here. Please clarify this aspect. »

Answer : Assuming that the major part of the salinity observations are used in both MEDRYS and the reference gridded products, the hypothesis that the salinization in the deepest layers in the reanalysis is not observed by the observation network but a consequence of the propagation of a surface anomaly is expected. However, as

C1195

we are not able to show that we use exactly the same salinity dataset, we present it as a suggestion. In order to clarify it, we also added that the uncertainties in the salinity products are large (Llasses et al., 2015) so it cannot be discarded that the observational product missed that change.

Reviewer: « page 1836 lines 15-end The analysis of the 6 RMS time series should be more detailed. Compare the RMS of MEDRYS to MEDATLAS98 does not make sense to me and I recommend to take it out. »

Answer : The comparison with a climatology is a way to evaluate if the reanalysis improves the results with respect to a constant state, so we think it is useful. This precision has been added in the text. We do not see how to detail the analysis of the 6 RMS time series without making it too dense or without repeating what is said for the analysis of the mean time series.

Reviewer: « page 1836 lines 26-27 You assess the ability to reproduce the high frequency surface temperature and salinity at one location over a 2 months period. This sub-section should present the same statistics for other fixed stations in other parts of the domain over a more extended time period, results might be very different in other parts of the domain. Where is the lion buoy? I suggest to take out the part of the time series affected by the sensor failure and present only bias and RMS (please add RMS) over the trustable period. Please revise accordingly. The anomaly could show better the difference between the two model solutions in figure 13. »

Answer : as said in the answer to the general comments, we do not demonstrate anything general. The comparison with the LION mooring data simply illustrates the range of temporal scales. We will clarify this aspect in the text. We can also explain the interest of this particular buoy and period as MEDRYS has been developed in the framework of the HyMeX program, for which a field campaign took place during winter 2013 in the North-Western Mediterranean Sea.

Reviewer: « Section 3.2.5 The first phrase up to line 6 of page 1838 should go in the

C1196

introduction. Formulas could explain better how you compute the transports. »

Answer : We agree with the reviewer, we reorganised some sections, introducing methods and materials that are used (see general comments). Formulas have been added at the beginning of the sub-section "Transport through the Strait of Gibraltar".

Reviewer: « page 1838 line 15 The reader should know approximately the ranges of values present in literature, could you please include it? »

Answer : OK. We modified the sentence in : "Despite the realistic value of the net flow through the Strait of Gibraltar, outflow and inflow are underestimated in NM12-FREE in comparison with recent results published (Soto-Navarro et al., 2010, 2015). According to those studies, the acceptable range for inflow and outflow at Gibraltar Strait are respectively [+0.76 ; +0.86]Sv and [-0.84 ; -0.72]Sv."

Reviewer: « page 1838 lines 22-23 Temperature and salinity are consistent since your relaxation to ORAS4 in the buffer zone, I would not use this to corroborate your results. I would also avoid to compare MEDRYS to a simulation without including details of it, are they relaxing to ORAS4 temperature and salinity as well? »

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

Reviewer: « page 1839 lines 19-20 The referee does not agree with the statement that MEDRYS uses all available observations of CORA 4 dataset, you excluded a lot of observations without providing a clear explanation in the text. Please clarify this aspect introducing more details in section 2.4 and here. »

Answer : OK with the reviewer, see above.

Reviewer: « page 1839 lines 25-end The time consistency of ALDERA does not assure the time consistency of the MEDRYS since there are other factors affecting it that the authors should take into consideration. Please clarify this aspect. »

C1197

Answer : we agree and this point has been answered in a previous specific comment. The text has been clarified.

Reviewer: « page 1840 lines 1-7 I do not agree, the verification of the MSSH presented in section 3.2.1 says that your assimilation methodology modify the solution with respect to the reference hindcast. Moreover the reference field you selected is a coarse resolution global product used in the assimilation and not an independent and specific one. The choice of the reference fields for the validation of integrated temperature and salinity is questionable, as you report also at page 136 line 3, and not documented in literature (IMEDEA reference is missing). »

Answer : See above about the MSSH, MEDATLAS and IMEDEA. The sentence “The validation process has highlighted the good results of the reanalysis in terms of mean circulation and integrated heat and salt content” has been changed. Basically, we showed that the assimilation has imposed to the model a mean circulation which is coherent with a recent estimate of the MDT, when the same model without assimilation was not able to maintain this circulation.

Reviewer: « page 1841 lines 1-15 You present here preliminary results of additional work, this is not needed. »

Answer : OK, it should be presented in an other way. We reformulated this paragraph and present it as ideas and future plans, which can be included in such a discussion/conclusion part, as the results of these new work does not exist yet and are beyond the scope of this current work.

Reviewer: « page 1841 lines 16 In spite what you say I suggest to repeat the analysis in this paper with the best version of MEDRYS and to include some of the scientific results you already identified in the Med-CORDEX/HyMeX framework to improve the quality of the paper that now looks more a technical report. »

Answer : the last paragraph has been reformulated : we do not mention different ver-

C1198

sions anymore, and we put the stress on the scientific results showed in this paper (heat and salt contents evolutions; mean sea surface circulation and variability; water, heat and salt transports at the Strait of Gibraltar), and which already help to answer HyMeX/MED-CORDEX scientific key topic. To our mind, adding supplementary results on other processes would make the article too dense. Studies on specific processes should preferentially be done in dedicated articles.

Technical corrections:

Reviewer: « page 1816, line 13: you talk about “The first version” of MEDRYS but the paper never talk about different versions. I suggest to eliminate “first version”. »

Answer : As we do not speak about a second version of MEDRYS (see the last specific comment), we eliminated “first version”.

Reviewer: « page 1816, line 17: I would eliminate the word “simulation”, it is misleading, MEDRYS is a reanalysis and what you call free simulation is an hindcast. »

Answer : OK

Reviewer: « page 1816, line 19: ...“at intermediate layers” instead of “in” »

Answer : OK

Reviewer: « page 1816, line 21: please correct the with the correct symbol »

Answer : OK

Reviewer: « page 1819, line 20: Please start a new line. »

Answer : OK

Reviewer: « page 1822 line 24 Please take out “conditions”. »

Answer : OK

Reviewer: « page 1822 lines 23-end Please revise this sentence. The last line is a

C1199

repetition you already said that ALDERA covers the period 1979-2013. »

Answer : OK

Reviewer: « Section 3.2 I would eliminate subsections titles 3.1 and 3.2 that repeat the same titles of the parent section 3, they are redundant. »

Answer : We agree with the reviewer and the Section 3 has been reorganised (see specific and general comments).

Reviewer: « Fig. 9-10 start from 1980, why? I would insert a legend. The central blue line should be solid, I suggest to redo it only for the period of MEDRYS. In some figures MEDRYS becomes MEDRYSV1, please correct. In figure 13 the legend should use the same acronym used in the paper. »

Answer : We do not agree with the reviewer as we wanted to show the behaviour of the free run as well (see specific comment). Typos have been corrected.

Reviewer: « Fig. 14 should be a table. »

Answer : 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 have preferred to move it to a Figure.

Reviewer: « page 1839 lines 22-25 I suggest to rephrase. »

Answer : We agree and this point has been answered in a previous specific comments.

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Interactive comment on Ocean Sci. Discuss., 12, 1815, 2015.

C1200

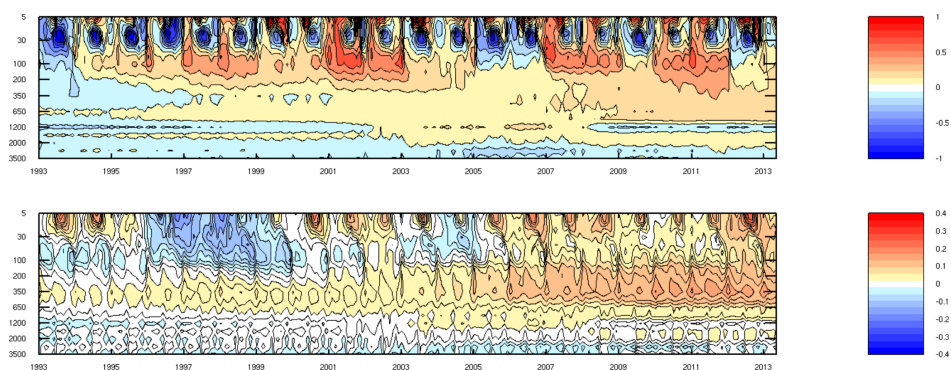


Fig. 1 : NM12-FREE basin mean temperature (°C, above) and salinity (psu, below) anomalies with respect to MedAtlas-1979 (initial state of NM12-FREE).

**Fig. 1.**

C1201

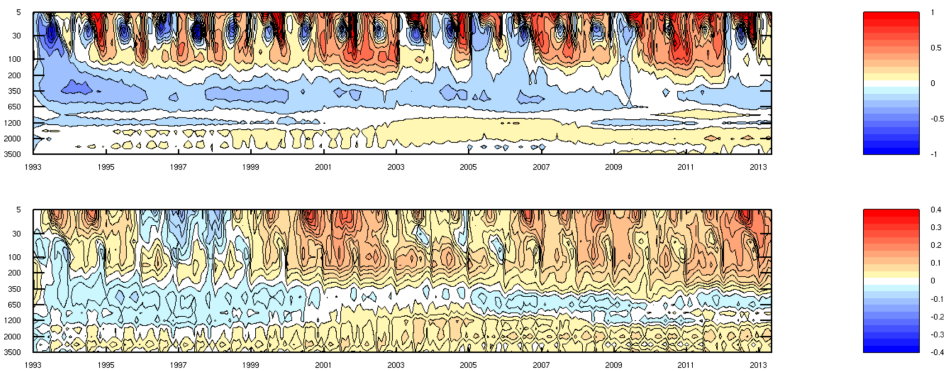
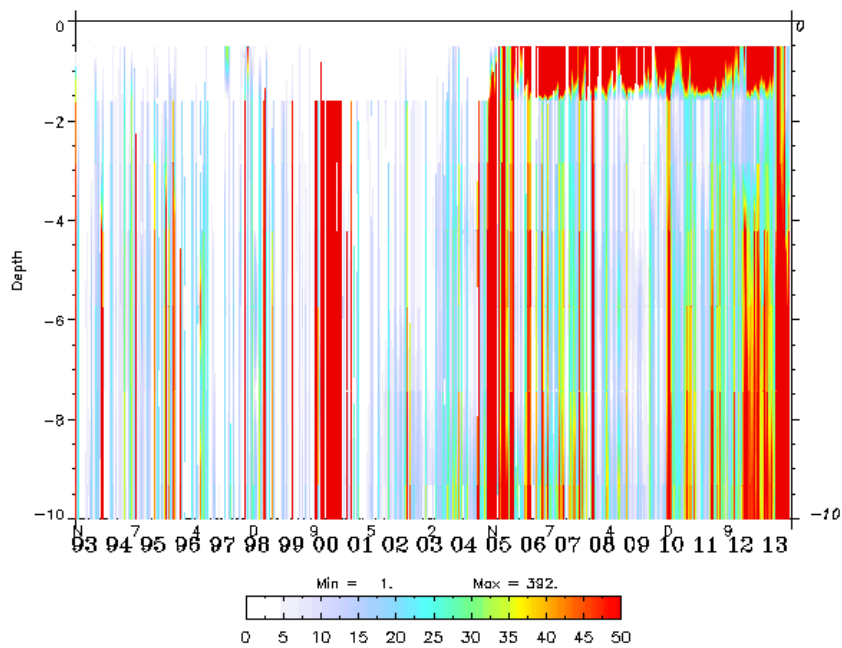


Fig. 2 : MEDRYS basin mean temperature ( $^{\circ}\text{C}$ , above) and salinity (psu, below) anomalies with respect to MedAtlas-1979 (initial state of NM12-FREE).

**Fig. 2.**

C1202

### Mediterranean : Temperature Profile Number (region 2)



**Fig. 3.**

C1203