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Interactive comment on "Dense water formation in the coastal northeastern Adriatic Sea: the NAdEx 2015 experiment" *by* Ivica Vilibić et al.

Anonymous Referee #1

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This is a review of the manuscript "Dense water formation in the coastal northeastern Adriatic Sea: the NAdEx 2015 experiment" by Vilibic et al. The paper provides a modelling and observational study focusing on dense water formation (DWF) in the Northwestern Adriatic Sea. The main scientific questions formulated by the authors are: 1. is DWF frequent or exceptional in this basin ? 2. What are the thermohaline changes associated ? 3. What are the exchanges between this basin and the open-sea Adriatic, and therefore its importance for the Adriatic thermohaline circulation ? 4. What are the associated mesoscale and frontal processes ? For that purpose, the study focuses on the well-documented 2014-2015 case study based on the NADEX experiment. The relevance of the observing design and of the numerical modelling tool to address those scientific questions is appreciated. The problematic is also well-introduced with a comprehensive bibliography, and the general quality of writing is good.

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Discussion paper

Out of the 4 main scientific questions, the authors actually address questions 1 to 3, the mesoscale and frontal processes being omitted. However, major concerns arise from the irrelevance of many diagnostics with respect to the authors' commentaries and conclusions. As a consequence, several key conclusions of the study are not demonstrated at all or fragile. I therefore recommend a major review addressing the following points:

- the authors give limited support that winter 2014-2015 was milder than average, Fig.2 not being adapted to this purpose ;

- they do not provide a clear demonstration that DWF occurred within the NADEX area, with respect to the hypothesis that dense waters were imported to this area from the open-sea Adriatic. The lack of any mixed layer depth estimate is a major drawback in the characterization of DWF.

- the velocity section doesn't illustrate, contrary to the authors' claim, that lateral exchanges at the basin boundaries are mostly baroclinic with outcoming waters at the surface and incoming waters at depth ;

- the model evaluation omits major elements of observed variability: is the bottom temperature decrease and density increase during winter trend accurately represented ? Does the model have a baroclinic circulation structure at the transects with incoming waters at depth and outcoming waters at the surface, at least at A2 and A7 locations ?

- the authors don't convincingly show that in the model, DWF occurs locally in the NADEX area and results from Bora wind events. The lack of any mixed layer depth estimate is again a major drawback in the characterization of DWF.

- a major concern arises in the methodology used to calculate residence times within the basin: by separating along-basin and cross-basin fluxes, the authors largely overestimate the residence time which should include altogether all boundary fluxes. Also, the probability density function is not defined and probably not adapted (too noisy) to get a robust residence time estimate: instead, a temporal mean residence time estimate would probably give a more convincing result.

Here is a detailed description of major concerns.

M1 There are too many plots: Fig.8b is unnecessary, especially because glider data is not shown, and an average bias for both temperature and salinity would be sufficient ; model-observation comparisons could easily be merged with observation-only plots ; Fig.9 and 10 are probably unnecessary (see comments below).

M2 Fig.2 doesn't prove that winter 2014-2015 was milder than average. For that, an anomaly with respect to an interannual mean should be displayed. Also, a computation of the thermal and saline buoyancy flux at the surface would help to relate surface conditions to DWF. The only support for a mild winter is given with percentiles of surface temperature. However, several points are not clear: the percentiles are computed with respect to what period ? A 30-year climatology ? Is it the near-surface atmospheric temperature over the sea or the sea surface temperature ? Also, the presence of mild periods doesn't prove that the whole winter was on average milder: for that, a time average needs to be calculated.

M3 Fig.3 is valuable but it doesn't prove that DWF occurred within the NADEX area (which corresponds to the nested ROMS area) as the dense waters could also have been imported from the open-sea. The same is true for Fig.4 as the dense and salty anomaly could have been imported from the lateral boundaries. Finally, Fig.5 doesn't prove either that DWF occurred in the NADEX area. Unfortunately, no mixed layer depth was computed in this study, but using a classical density anomaly criterion of 0.01 to 0.05kg/m^3 with respect to surface density (e.g. De Boyer Montegut et al 2004, Houpert et al 2015) would show clearly that the mixed layer is shallow throughout the transect. A comparison of the Stratification Index at the bottom with typical surface buoyancy fluxes modelled by ALADIN could be an approach to suggest (but not demonstrate) that DWF occurred during the winter.

M4 Fig.6 doesn't illustrate a general incoming of dense water at depth and outcoming of lighter waters at the surface. Contrary to the authors' statement, only stations A2 and A7 show a predominantly baroclinic vertical structure of velocity. The stations A3, A4 and A7 to A9, which cover the main connecting section between the NADEX area and the open-sea also show a strong barotropic flow. The interpretation of this figure should be more cautiously done.

M5 The model shows a large temperature and salinity bias in the open-sea Adriatic which therefore comes from the low-resolution ROMS model. It should be at least documented and interpreted, or at best reduced in order to produce realistic lateral exchanges between the NADEX and open-sea areas.

M6 If 20 sigma levels are not sufficient to simulate dense water transformations in the NADEX area (the bottom dense water layer is not modelled), how many would be necessary ? What was the rationale in the choice of only 20 sigma levels when regional and coastal Mediterranean Sea models exhibit typically between 50 and 100 vertical levels (e.g. Mediterranean Sea reanalyses in Pinardi et al 2013 and Hamon et al 2016) ?

M7 Fig.10 gives a too raw evaluation to be insightful because there is no spatio-temporal information, and it should be removed as it is currently. Some elements of observed bottom hydrology and velocity profiles were noted in the observations section and not even commented in the model evaluation section. In particular: is the bottom temperature decrease and density increase trend accurately represented ? Is the salinity front also visible at the same area ? Does the model have a baroclinic circulation structure with incoming waters at depth and outcoming waters at the surface, at least at A2 and A7 locations ? By the way, it would have been insightful to also evaluate the current directions.

M8 Fig.11 is also too raw, it could be improved to relate hydrological transformations to DWF and Bora wind events. Once again, no mixed layer depth was computed and the large range in the colorbar makes it impossible to identify the period of DWF: it would require to read vertical density variations of typically 0.01kg/m^3. The Bora wind was mentioned to explain the stepwise cooling, but a comparison between the vertically-integrated heat loss and the surface heat flux would give a much more convincing relation between cooling and Bora events.

M9 Without mentioning the depth at which the SI is computed, it is impossible to interpret Fig.13. Assuming that the SI was computed at the bottom of the water column for each location, Fig.13 could have provided an SI-based mixed layer depth map (threshold at 0.01m^2/s^2), already used in several Mediterranean studies (e.g. Waldman et al 2017). The link with the Bora event is not clear because: 1. most of the domain already has an SI<0.01m^2/s^2, the signature of DWF, before the Bora occurs ; 2. only the comparison between the ocean SI loss and the integral surface buoyancy flux would allow to attribute buoyancy losses to the Bora. As a consequence, the authors' conclusion of 6.1 has not been demonstrated.

M10 In Fig.18 the methodology to compute the probability distribution of residence time is not explicited. I assume the residence time is computed for each model day and the probability distribution is therefore temporal and daily based on the winter period (which period ?) Results are very noisy which makes them difficult to interpret: I strongly recommend to compute a temporal mean residence time and not a probability distribution function in order to increase the robustness of results. Also, even though it is interesting to differentiate between along-basin and cross-basin fluxes, the residence time of a water parcel is impacted by all boundary fluxes, which cannot be considered separately: by doing so, the authors artificially increase the residence time. I therefore recommend the authors to compute a residence time including all boundary fluxes, and to compare along-channel and cross-channel fluxes without deducing residence times from them.

Here is a description of minor concerns:

Abstract

m1 Do not use the term one-way coupling. There is no feedback of the ocean models toward the atmospheric model, therefore there is no ocean-atmosphere coupling.

2.3.

m2 What is the consistency between the sea surface temperature at the boundary of the forcing atmospheric model and that of the ocean model ?

m3 What is the time resolution of the atmospheric forcing and how does it impact the representation of extreme heat fluxes associated with Bora events ?

m4 Be more explicit about how bathymetry smoothing ensures the run stability.

m5 The mention of an operational integration for the large-scale ROMS model usually implies that data assimilation was done within the model domain. Is it the case ?

m6 Was the nesting 2-way (coupling between both ocean models) or 1-way (forcing of the nested model by the large-scale model) ? If it was 1-way, do you believe that the absence of any feedback toward the large-scale model alters the estimated lateral transports at the boundary of the NADEX domain ?

3.

m7 Missing mention top/middle/bottom in Fig.2.

m8 No critical assessment of the gust wind event was made. Which is the Bulk turbulent flux formulation used in the ALADIN configuration ? Most state-of-the-art Bulk formulations have not been calibrated for winds >20m/s because of the scarce observations (e.g. Fairall et al 2003), which makes them unrealistic and divergent between each other at such wind regimes. Therefore interpretations of ALADIN outputs should be more cautious and critical.

4.

m9 Regarding the presence of a salinity front between A7 and A9, the large high-frequency variability gives some doubts about the significance level of such a signal. Could you provide it please ?

5.

m10 Quantify the model bias decrease between the winter and spring cruises.

m11 Comment the persistent low salinity bias around station 18.

m12 When assessing the model bias, it is clearer to display the difference model minus observations, so that Fig.7 to 10 can be interpreted directly as model bias.

m13 Fig.9 is unnecessary as it reiterates the evaluation done in Fig.7 and 8, without a clear added-value in the interpretation of using Q-Q plots or box whiskers diagnostics. Also, the mention of an ideal distribution in the Q-Q plots is unclear, as all observations should fall within this slope 1 line (e.g. Herrmann and Somot 2008).

m14 It would be more relevant to compare temperature and salinity biases to their variability for instance, rather than to their absolute value, in order to compare biases to the typical hydrological variability in the area.

6.1

m15 Fig.12 shows repetitive timeseries: instead, an area-averaged surface buoyancy flux timeseries (in m^2/s^3/day, and not m^2/s^3) would give qualitatively the same result, but more integrated. The difference between locations should be documented quantitatively (how does each location compare to the area-average ?) in the text, without displaying the 7 plots.

6.2.

m16 The so-called temperature flux should be named heat flux.

m17 Specify when mentioning Fig.14 and in its legend that it is modelled results.

m18 Fig.14 is misleading because of a factor up to ∼30 between transect widths: a constant scaling would be more appropriate to compare heat and salt fluxes. Same comment for the vertical scaling.

m19 How do you interpret that the NADEX area is losing heat through its lateral boundaries despite the stronger Bora heat loss than in the open-sea (Fig.2 middle panels) ?

m20 It is not intuitive that dense water mass fluxes should be directed outward as Fig.3 suggests that the densest waters with a high salinity signature came in from the open Adriatic and were not formed locally. How do you explain this appearent discrepancy between the model and observations ?

m21 The interpretation of some high-frequency transport variability in Fig.15 being driven by Bora events is interesting and could be tested by calculating the transports induced by Ekman currents.

m22 How do you interpret that the residence time for dense waters is lower than that for the total volume, when it is visible from Fig.6 that in observations (but the same is probably true for the model), velocities are lowest in the deep layers where dense waters are located ? One should therefore expect a longer residence time for those dense waters.

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