Response to the Reviewer #1 comments on the manuscript "Dense water formation in the coastal northeastern Adriatic Sea: the NAdEx 2015 experiment" by I. Vilibic et al.

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.

- We appreciate the words by the reviewer.

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:

- We plan to substantially revise the manuscript following reviewer's suggestions, taking into account all provided comments. A small number of comments on which we disagree are largely result of bad explanations and insufficient details provided in the text. Robust arguments for these statements will be provided in the revised manuscript. Also, we will add more material – like glider profiles – which are addressing mesoscale and frontal processes in the area.

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

- There are two official reports of the Meteorological and Hydrological Service of the Republic of Croatia which are quoted as references (MHS, 2015, 2016), which classify the winter of 2015 (DJF) as warmer-than-average compared to the base period of 1961-1990. The classification is also available at http://klima.hr/ocjene_arhiva.php. Yet, both reviewers bring out as a weakness in our analyses, so we concentrated on the model period (2008-2015) and estimated average wintertime heat fluxes in the NAdEx area from the model for all winters between 2008 and 2015. For these estimates we used average January and February fluxes (as the DWF occurs in the northern Adriatic exclusively in these months) over the nested ROMS domain (marked by dots in Fig. 1). These estimates put the winter of 2015 (January-February) to be ranked as a normal (see Table 1 below) in terms of average heat losses in the NAdEx area. Relevant changes in the text and additional explanations will be inserted in the manuscript.

  Table 1. Cumulative January-February net heat losses (in MJ/m^2) estimated from the Aladin/ROMS model for winters between 2008 and 2015.

2008	2009	2010	2011	2012	2013	2014	2015
-0.53	-0.76	-0.78	-0.75	-1.20	-0.80	-0.49	-0.80

- 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.

- Thanks for this comment, raised also by Reviewer #2, which is a result of insufficient presentation of our results. Namely, the NAdEx region is a shallow region (up to 100 m) and is completely mixed over the most of the domain during wintertime, particularly if there are strong bora events as occurring in winter of 2015 (see details in Section 3.1). Also, previous modelling studies indicate that the DWF is occurring in the area and that dense waters are outflowing towards the open sea (Mihanović et al., 2013; Janeković et al., 2014). Next, major DW pathways from the open northern Adriatic shelf are placed along the western shore (due to Coriolis force, there is a lot of literature on that), being just hypothesized to occur along the eastern shore (Vilibić, 2003). In fact, our Arvor-C findings are the first proof of the DW coming from open Adriatic to the eastern shore, but this area (Kvarner Bay) is characterized by a gentle slope in its outer part, while the rest of connecting passages has a barrier which restrict eventual near-bottom DW inflow to the coastal region.

As coming from our model results, the most of the area was completely mixed in 2015 and therefore the stratification index presented in Fig. 13 is approaching zero in most of the domain, while the stratification is somehow being increased between bora events and DWF events due to relaxing horizontal advection that may establish a weak stratification at some of the NAdEx area. Vertical homogeneity over the whole water column can be also seen in Hovmoller plots of model results coming at two locations (old Fig. 11). So that, we anticipated an estimation of MLD as not straightforward to reach any conclusion about the DWF. However, we estimated and presented in old Figs. 16 and 17 temporal and spatial properties of the DWF fluxes and transports at connecting passages, which are mostly directed outward. In addition to that, we estimated average DW fluxes at the connecting passages and summarized them in the last row of Table 1 – all of them are directed outward, i.e. from the coastal region towards the open sea.

We believe that these estimates are a proof that the DWF occurred in the NAdEx area, and not advected from the outside (except at the very bottom in the Kvarner Bay). Yet, our arguments are obviously not properly presented in the manuscript, so that plan to add a separate section 6.3 in the manuscript, which is summarizing all the results and arguments about the DWF. Also, we will add new material in Section 1 and add a paragraph in Section 7, which discusses this question and summarizes our results with respect to previous DWF studies in the NAdEx area.

- 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;

- Ok, we will rephrase the statement, concentrating to the description of the lateral exchanges and not going into explanation of processes.

- 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?

- We added more on the model performance at the lateral boundaries, particularly commenting on A2 observations. The connecting channel Sedmovraće, off which the station A2 has been positioned toward the open sea, is very narrow, about 600 m in its deep section, and the model with horizontal resolution of 500 m is obviously not capable to reproduce the dynamics at these spatial scales. Nevertheless, this channel is connecting outer waters with inner in the channel area between A1 and A2, while the latter are connected with the rest of the NAdEx area through shallow passages only (< 40 m), which physically prohibit the inflow of near-bottom dense waters from the open sea. Therefore, it is not physically possible that dense waters come from the open sea to Kvarnerić and Velebit Channels through Sedmovraće (where A2 station has been moored).

- 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.

- As said, the computation of the mixed layer depth is straightforward in deep environments (like Southern Adriatic or Gulf of Lions), but not for a shallow area which is mixed to the bottom, what is the case for the northern Adriatic between November and March (including NAdEx area) (Franco et al., 1992; Artegiani et al., 1997; Viličić et al., 2009). We will put more estimates and arguments about DWF in separate section 6.3 of the manuscript.

Artegiani, A., Bregant, D., Paschini, E., Pinardi, N., Raicich, F., Russo, A., 1997. The Adriatic Sea general circulation Part I Air–sea interactions and water mass structure. Journal of Physical Oceanography, 14, 1492-1514.

Franco, P. and Michelato, A., 1992. Northern Adriatic Sea: Oceanography of the basin proper and of the western coastal zone, Science of the Total Environment, Suppl., 35-62.

Viličić, D., Kuzmić, M., Bosak, S., Šilović, T., Hrustić, E., Burić, Z., 2009. Distribution of phytoplankton along the thermohaline gradient in the north-eastern Adriatic channel; winter aspect. Oceanologia, 51, 100-118.

- 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.

- Ok, we simplified the computation of the residence time and present result in the form of box-whiskers diagram (Fig. 1). A simple estimate of the residence time within the studied domain was obtain using equation:

$$RT = \frac{\iiint_{x,y,z} \rho(x,y,z)\, dx\, dy\, dz}{\oiint_{\mathcal{C},z} \rho(x,y,z)\, \vec{u}(x,y,z).\vec{n}\ d\mathcal{C}\, dz}$$

where $\rho(x,y,z)$ is the density of the water at each point (x,y) and for each depth z of the domain, while $\vec{u}(x,y,z).\vec{n}$ is the normal velocity along the contour C of the domain. Such an approach assumes that (i) only the velocities at the border of the domain are used to calculate the residence time, (ii) only the outflow of water at the border are taking into account; the goal here is to only look at how long it would take for the water masses to leave the domain assuming that there is no income of water within the studied domain, and (iii) the residence time is calculating with a time step of 3 h over the period of the studied event, assuming a steady state of the dynamical conditions at each time step. The box-whiskers diagram (Fig. 1) is thus showing the statistical properties of the estimated residence time for each 3 h model results.

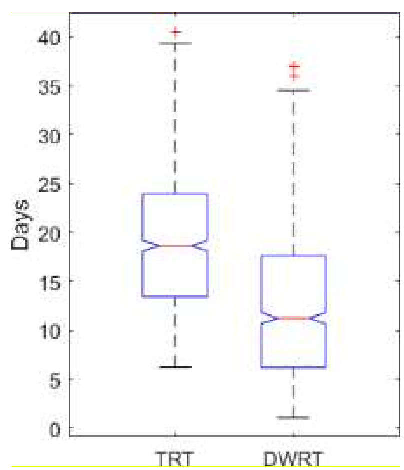

Fig. 1. Simple estimate of the residence time of both total water (TRT) mass and dense water (DWRT) within the studied domain.

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).

- We don't agree with this reviewer comment. Particularly, as Reviewer #2 asks for inclusion of a glider data figure, which we did as the figure is showing the existence of the thermohaline front. By doing that we provided new results to support the existence of the thermohaline front stretching from the Kvarner Bay towards the open sea, what is recognized as a drawback by the reviewer.

Regarding Figs. 9 and 10, they provide an information about the reliability of the model versus all measured data, which are essential for assessing the quality of the model discussed in Section 7. For example, Fig. 10 clearly show an underestimation of currents by the model, not provided by any other figure, which is quite important result highlighted in the abstract and being the base for discussion on ways to improve modelling in such a complex area. So that, we are in favour for keeping them in the manuscript. See also the response to the M7 comment.

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.

- Yes, we agree - that was claimed following official publications of the Meteorological and Hydrological Service of the Republic of Croatia (MHS, 2015, 2016). We do not claim such a statement from Fig. 2. Anyway, we computed some statistics and comparison between winters on heat fluxes from the model – the details are provided at the beginning of this document. We will insert these computations and analyses into the revised manuscript.

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 0.05 kg/m3 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.

- To repeat, the comment and all of the quoted references are dealing with the dense water formation occurring through deep ocean convection, for which the computation of mixed layer depth is essential. Here, the NAdEx area is shallow and mixed to the bottom in the winter of 2015. There is a substantial literature on that for the shallow northern Adriatic shelf (already presented in introduction, but we add a few more references), none of them using MLD in DWF analyses as not being relevant. However, we agree that the proof for the DWF in the NAdEx area and the associated DW dynamics in the connecting channels is not explained well, so we further developed the analysis, which will be presented in separate section 6.3 of the revised manuscript. See also previous comments on the DWF issues.

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.

- We agree and we will rewrite these statements in the revised manuscript.

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.

- Ok, we added more discussion on that, based on previous works by Janeković et al. (2014) and Vilibić et al. (2016). Both studies document the negative salinity bias observed in the whole Adriatic, being presumably a result of "lateral AREG model boundary conditions that use river discharges based on the Raicich climatology" (Vilibić et al., 2016). Some other studies apply a bias correction for their simulations of DWF in the Adriatic (e.g. Benetazzo et al., 2014), but we are not in favour of such an approach; we think that it is better to find a feasible explanation for such a model behaviour, which may then be corrected and lead to an improvement of the model.

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)?

- The Adriatic Sea, and in particular northern Adriatic Sea, is a shallow place and it does not need 100 sigma levels as for the Mediterranean. Some of previous studies reproduced reliably the DWF using 20-30 sigma layers (e.g. Janeković et al., 2014; Benetazzo et al., 2014). Also, our model run is a multi-annual run (2008-2015), not just focused on a single dynamics events but more to get a broader picture on the DWF and other processes in the northern Adriatic. Indeed, our study documents very fine details in DWF-related processes for the very first time, as some of NAdEx measurements are the very first of such kind in the Adriatic (e.g. Arvor-C and capturing of fine structures near the bottom of the Kvarner Bay). So that, our results are opening a direction to improve the DWF modelling, also by increasing number of sigma layers to better reproduce near-bottom dynamics. Yet, this is beyond the scope of this paper. Discussion on that will be added in Section 7, when commenting model drawbacks and ways how to resolve them in the future.

M7 Fig. 10 gives a too raw evaluation to be insightful because there is no spatiotemporal 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.

- We don't agree that Fig. 10 (and Fig. 9) in the manuscript are not providing substantial information about the model performance – the presented analysis is a statistical evaluation of model-to-observations performance, normally used in such studies and using standard presentations (Q-Q plots and box-whiskers diagrams). There is a large number of conclusions in the text derived from these figures, e.g. about an underestimation of currents in connecting passages, a good reproduction of temperature over a range of percentiles (what is quite important for assessment of heat losses and the DWF), and other. Yes, we agree that temporal evolution of the model performance cannot be achieved with such an approach, but such a detailed assessment is beyond the scope of this study. Also, some of raised questions (e.g. the last one) – on which we agree that it would be quite useful to discuss in the paper - is in fact extending the analysis compared to the presented one, so we plan to put extra material and the text in the revised manuscript.

  In particular, we plan to add modelled residual currents and ellipses of standard deviation at all A stations (Fig. 2). It can be seen that (i) the model reproduces satisfactorily the observations (bearing in mind that the bathymetry and the atmospheric forcing is quite complex in the area) except at A2 station, but this station is placed in quite narrow connecting passage (about 600 m) not properly resolved with the model (resolution of 500 m) – more comments on that are already placed above, (ii) the model reproduce fairly well vertical changes in currents (again except at A2), aside the fact that the direction may differ from observations due to differences between real and model shorelines and bathymetry (like at A7 and A9). Anyway, we think that the model results are usable for investigations of DWF and transports in the area, while their shortcomings are discussed in the discussion section.

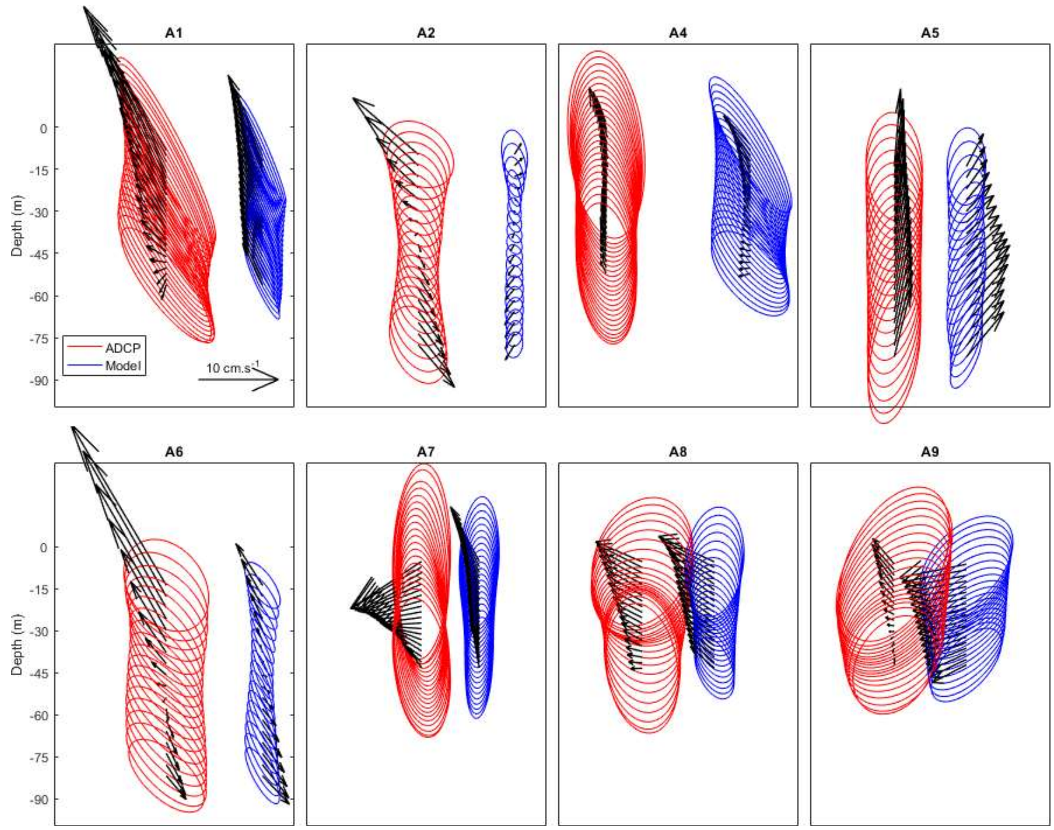

Figure 2. Mean residual currents and ellipses of standard deviations measured (in red) and modelled (in blue) at A1 to A9 stations between 1 February and 1 April 2015.

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/m3. 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.

- We replot the figure by using different colobar, which provides much more details to the cooling events and the associated DWF (Fig. 3). Again, we did not compute MLD as not relevant for the DWF in shallow (shelf) areas. It can be seen clearly that no dense waters are advected to the region after the bora events. Also, we added simple computations of bora-driven cooling to make the relationship more robust, using a simple box model (e.g. Gill, 1982):

$$\Delta T = \frac{1}{Hc\rho_0}\int_{t_1}^{t_2} Q\, dt$$

where ΔT is the temperature changes induced by surface heat loss Q in the time interval between t_1 and t_2, while H is the ocean depth. This model assumes no lateral exchange of energy between the box and the adjacent sea, what is a fair approximation for short and transient events like the bora. At station G1 this simplified formula gives ΔT = -0.96°C, -0.48°C and -0.03°C for three bora events (the third bora was very weak at G1), respectively, while the respective cooling rate as provided by the model is -1.04 °C, -0.51 °C and -0.07°C. Given the assumptions of this simple box model, one can say that the cooling in the area is dominantly driven by the bora wind.

More on that will be added to the manuscript.

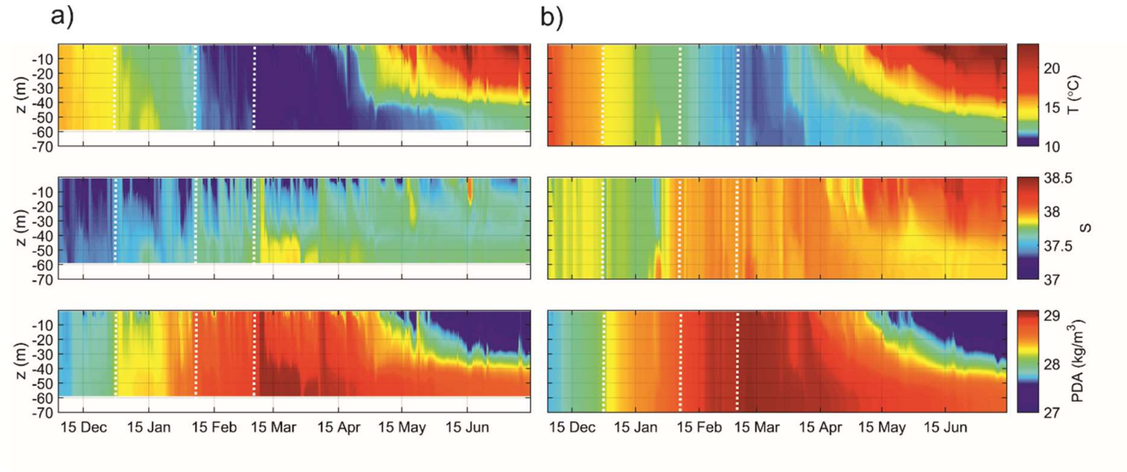

Figure 3. New Fig. 11 from the discussion paper, with colour bar adopted to show more details.

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 0.01 m2/s2), 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<0.01m2/s2, 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.

- The SI is computed for the whole water column – the info is added in the text - to present the level of stratification and to assess if stratification may prevent the DWF. Again, MLD is not the parameter considered relevant if DWF studies over a shelf, while the suggested literature is dealing with deep-convection processes occurring over a few thousand meter depths.

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.

- Ok, we changed the methodology of residence time estimates as suggested. Also, we switch the presentation from probability distribution to simple box-whiskers statistics. The details are provided in introductory part of this document.

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.

- Ok, corrected.

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?

- NWP ALADIN/HR model used in our study uses SST fields coming from the IFS (Integrated Forecast System) operational forecast run in ECMWF (European Centre for Medium Range Forecast). These SSTs have a positive bias towards in situ measurements during the winter and during bora episodes in the Kvarner Bay. When compared to the SST satellite observations at the open Adriatic, the bias is much lower. It has been found that these SST differences have an impact on the value and location of the precipitation maximum in the domain, but affect wind speed only slightly, as strong wind episodes (such as bora) driven by a strong synoptic forcing are primarily controlled by surrounding topography. There is paper on the subject has been submitted only recently but there are few reports available (ALADIN/HIRLAM Newsletter No8. http://www.umr-cnrm.fr/aladin/meshtml/NL8-final.pdf and several posters and Workshop presentations)

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?

- The ALADIN/HR output is every 3 hours, as being a standard of the operational product of the Meteorological and Hydrological Service. Although bora wind may have a substantial variability on periods from several minutes to a few hours, previous modelling studies that used 3-h ALADIN/HR forcing provided reliable results (e.g. Janeković et al., 2014). Unfortunately, we cannot give a response to the second part of the question, for which sensitivity studies should be carried out.

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

- The bathymetry smoothing has been performed using a linear programming technique (Dutour Sikirić et al., 2009) that suppresses horizontal pressure gradient errors. Such approach is required for complex bathymetry with steep slopes such as the Adriatic Sea, to get an unbiased result for longer multiyear integration. The explanations will be provided in the revised manuscript.

  Dutour Sikirić, M., Janeković, I., Kuzmić, M., 2009. A new approach to bathymetry smoothing in sigma-coordinate ocean models. Ocean Modelling, 29, 128-136.

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?

- Operational integration is mentioned just for ALADIH/HR which is numerical weather prediction model, not for ROMS, which has no assimilation imposed. The reference about the ALADIN/HR assimilation will be added in the manuscript (Stanešić, 2011).

  Stanešić, A., 2011. Assimilation system at DHMZ: Development and first verification results. Hrvatski meteorološki časopis, 44/45, 3-17.

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?

- It is the one-way nesting. For sure, the two-way nesting at lateral boundaries will change the transports there, yet we consider these effects not substantial for description of the DWF. What is more important, in our humble opinion, are feedback processes at the ocean surface (two-way coupling between ALADIN and ROMS), which is known to alter modelled wind and ocean currents and heat fluxes in the open northern Adriatic up to 10-20 % during bora events (Pullen et al., 2007, Ličer et al., 2016). Then, the changes in the wind field will for sure change water exchange in connecting passages. Some of the discussion regarding these issues is already in the manuscript, but we will extend the discussion in the revised manuscript and add new references.

  Pullen, J., Doyle, J., Haack, T., Dorman, C., Signell, R., Lee, C.M., 2007. Bora event variability and the role of air-sea feedback. Journal of Geophysical Research, 112, 33407, doi:10.1029/2006JC003726.

  Ličer, M., Smerkol, P., Fettich, A., Ravdas, M., Papapostolou, A., Mantziafou, A., Strajnar, B., Cedilnik, J., Jeromel, M., Jerman, J., Petan, S., Malačič, V., Sofianos, S., 2016. Modeling the ocean and atmosphere during an extreme bora event in northern Adriatic using one-way and two-way atmosphere–ocean coupling. Ocean Science, 12, 71-86.

3.

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

- Ok, added.

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.

- Yes, we are aware that bulk estimates may not be appropriate for some winds and ocean regions, also for the Velebit Channel which exhibit strong and gusty bora, with a lot of sea spray during extreme events. Yet, ALADIN products (winds, temperature, …) has been verified also in the NAdEx area during a number of severe bora episodes (e.g. Tudor et al., 2012, 2013). We will add some text and the reference on that issue in the discussion section.

  The wind gusts in ALADIN/HR are computed according to Boržkova et al. 2006. This particular modification was introduced as ALADIN was used in support of the MFSTEP oceanographic campaign. This is why we find it most suitable for this purpose. The problem is more complex. There are few measurements to calibrate (as pointed by the reviewer) and currently no measurements above the sea surface in this area. However, this area is composed of many small basins where waves have to build up as the wind blows from the coastline. Any relation derived for the open ocean would probably fail here. Coupling to a wave model seems more promising solution.

  Brožková, R., Derková, M., Belluš, M., Farda, A., 2006. Atmospheric forcing by ALADIN/MFSTEP and MFSTEP oriented tunings, Ocean Science, 2, 113-121.

  Tudor, M., Ivatek-Šahdan, S., Stanešić, A., 2012. February 2012 winter conditions in Croaria. 6th HymMeX Workshop, Primošten, poster available at https://www.researchgate.net/publication/283897781_February_2012_winter_conditions_in_Croatia.

  Tudor, M., Ivatek-Šahdan, S., Stanešić, A., Horvath, K., Bajić, A., Zhang, Y., and Ray, P., 2013. Forecasting weather in Croatia using ALADIN numerical weather prediction model. Climate Change and Regional/Local Responses. InTech, Rijeka, 59-88.

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?

- We estimated significance between temperature and salinity series at A7, A8 and A9 stations by applying t-test for mean and std values (Table 2). Both temperature and salinity series are significantly differing between all stations with $p<0.001$. In addition, to put the conclusion about thermohaline front more robust, we will add a figure with glider data to the manuscript, where the front can be clearly seen (Fig. 4). Of course, the front may not be a permanent feature during the whole wintertime period. These results and discussion are going to be added to the revised manuscript.

Table 2. Statistic of temperature and salinity data at stations A7 to A9 between 1 February and 31 March 2015.

Station	Mean temperature	Std temperature	Mean salinity	Std salinity
A7 (8497 data)	12.128	0.570	38.265	0.133
A8 (16993 data)	11.619	0.706	38.027	0.204
A9 (8497 data)	11.302	0.737	37.979	0.213

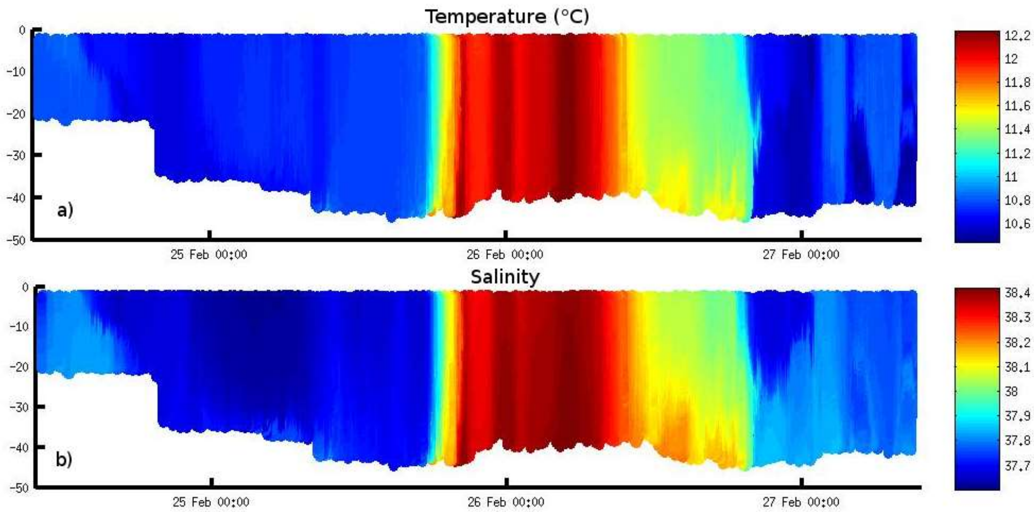

Figure 4. Temperature and salinity profiles measured by Slocum glider between 24 and 27 February 2015 in front of the Kvarner Bay. Glider trajectory is provided in Fig. 1 of the discussion paper.

5.
m10 Quantify the model bias decrease between the winter and spring cruises.

- The bias is depth-dependant, so we analysed vertical distribution of the bias (Fig. 5). It can be clearly seen that winter bias – both in temperature and salinity - is much larger during over the most of the water column, except sporadically in deeper layers. Some text on that will be added to the manuscript.

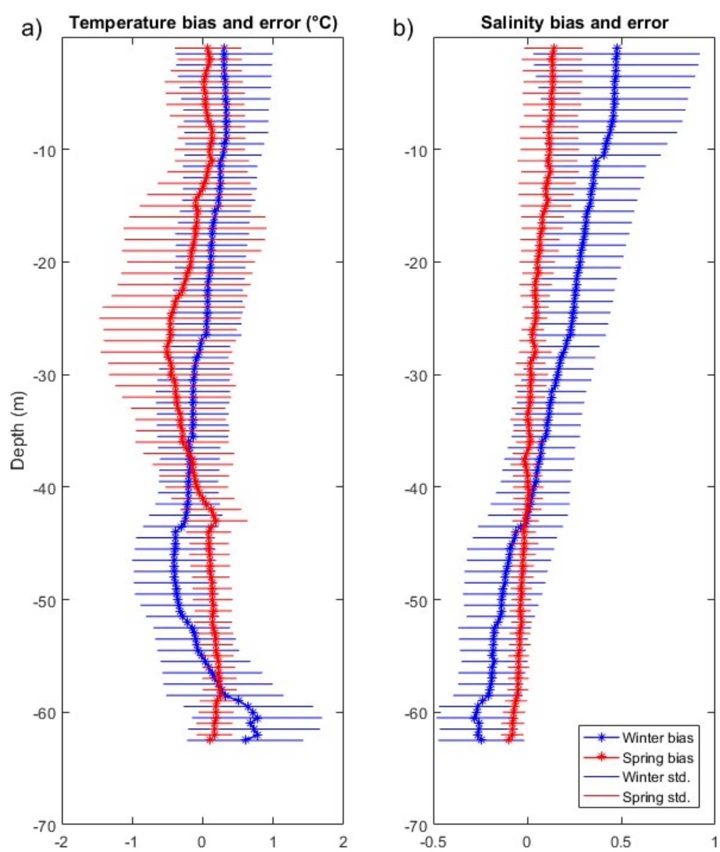

Figure 5. Vertical profiles of the bias and root-mean-square-error during spring (May 2015) and winter (December 2014) cruises.

m11 Comment the persistent low salinity bias around station 18.

- This minimum could be due to submarine springs which are discharging freshwater from neighbouring Vrana Lake to the sea and which are not introduced to the model. The area around station 18 is quite shallow, so such an effect may be seen on salinity data. A comment on that will be added to the text.

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.

- Ok, we agree, that would be done in revised manuscript.

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).

- We don't agree with the reviewer, as Fig. 9 does provide additional information about average temperature and salinity bias of the CTD data, not attainable from Figs. 7 and 8. Also, the figure present comparative statistics for datasets coming from different measuring platforms. So, we decided to keep the figure. We will rewrite the caption of Fig. 9 not mentioning ideal distribution any more.

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.

- Yes, we agree that bias is not describing the variability of the temperature and salinity, so that we computed rmse (root-mean-square-error) values as well. Their overall values are provided in Table 3. Yet, we decided not to present all of these computations in the manuscript, as the model verification part of the manuscript will be too large compared to the rest of the results, so we are planning to add a few results describing the rmse variability in the manuscript.

  Table 3. Model-to-observations bias and rmse values at A stations (the lowest layer has been taken for current speed estimates) for the whole measuring period.

	current speed (m s^{-1})		temperature (°C)		salinity	
station	bias	rmse	bias	rmse	bias	rmse
A1	-0.05	0.06	-0.54	0.35		
A2	-0.04	0.05	-0.30	0.38		
A3			0.29	0.85	-0.05	0.14
A4	-0.03	0.05	0.15	0.39	-0.02	0.09
A5	-0.02	0.04	-0.09	1.29		
A6	-0.02	0.05	0.32	0.67		
A7	-0.04	0.05	0.36	0.44	0.02	0.16
A8	-0.04	0.05	0.46	0.65	0.16	0.20
A9	-0.04	0.05	0.56	0.88	0.07	0.19

6.1
m15 Fig. 12 shows repetitive time series: instead, an area-averaged surface buoyancy flux time series (in m2/s3/day, and not m2/s3) 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.

- No, we are not presenting temporal changes of the buoyancy flux, but the flux itself, so the unit should be m^2/s^3. Thus we will change the caption to "daily values" instead of "daily changes". Also, we followed the reviewer's suggestion and computed the time series of averaged buoyancy fluxes over the whole NAdEx domain (Fig. 6), while spatial differences will be commented in the text only. The new figure will be placed in the revised manuscript.

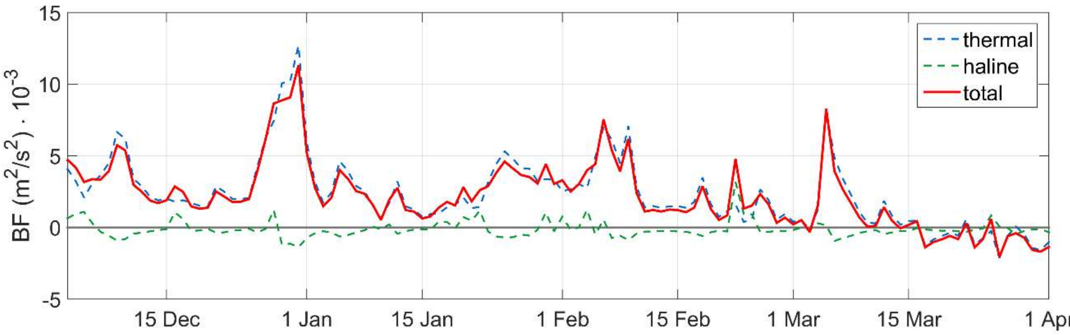

Fig. 6. Daily values of surface buoyancy fluxes and its components modelled over the whole nested model domain.

6.2.
m16 The so-called temperature flux should be named heat flux.

- Ok, changed.

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

- Ok, specified.

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.

- Ok, done, the figures are changed – see below (Figs. 7 and 8).

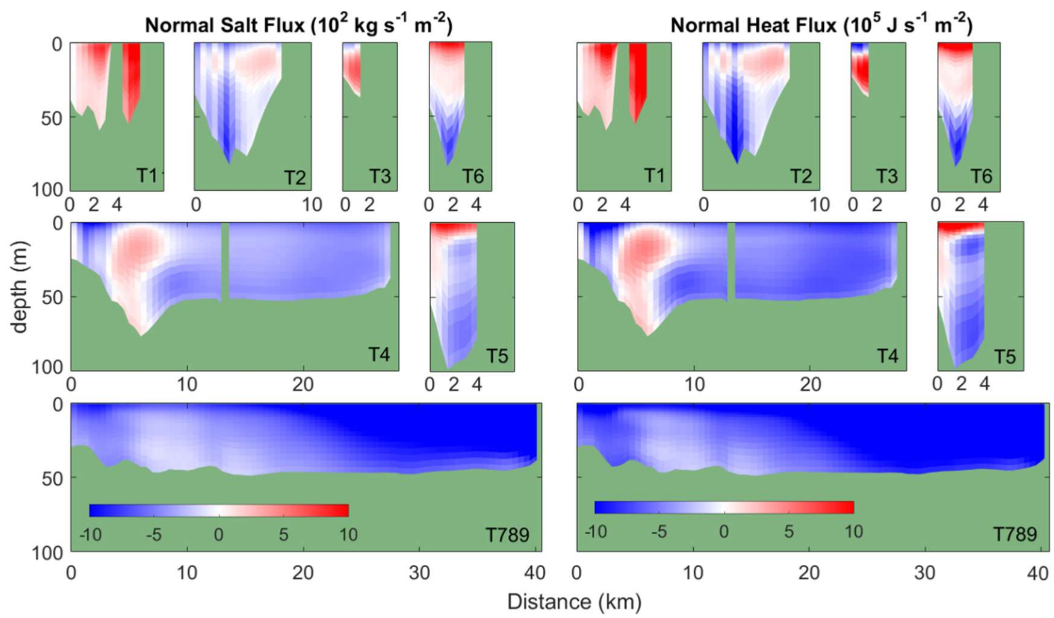

Figure 7. Replotted Fig. 14 from the discussion paper.

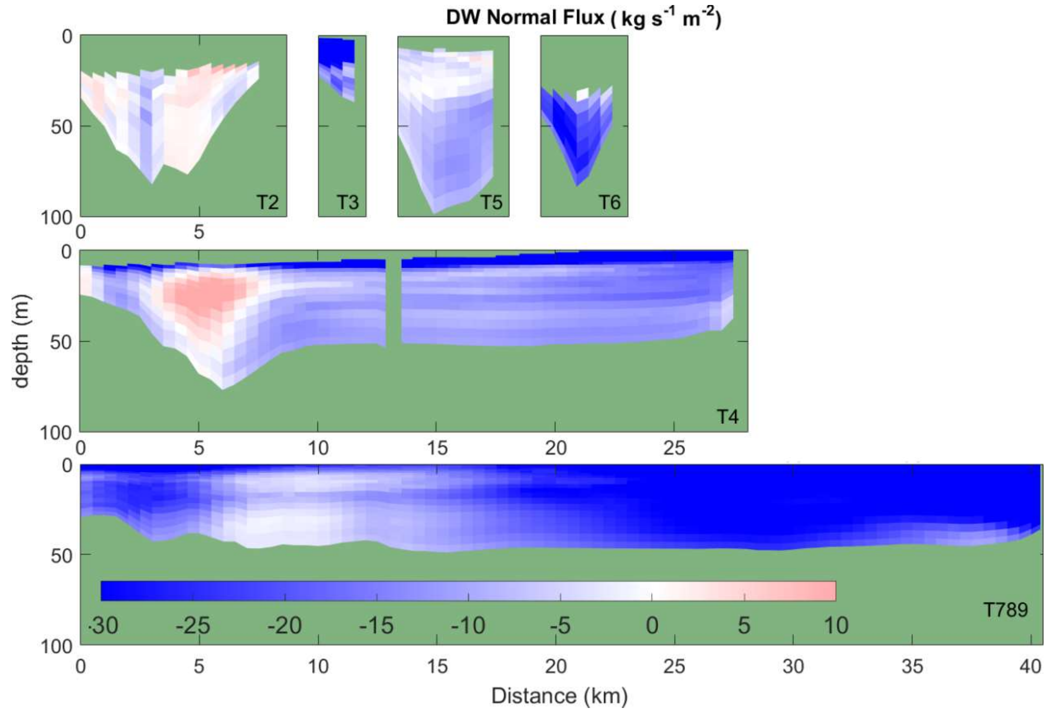

Figure 8. Replotted Fig. 16 from the discussion paper.

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)?

- That is due to wind-driven dynamics, that obviously dominates over the thermohaline circulation during strong bora events.

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 apparent discrepancy between the model and observations?

- We don't agree that Fig. 3 show an inflow of any water through the open boundary. It simply shows that the DWF occurred somewhere and that dense waters are present in bottom layers. We will now put more evidences that these waters are generated in the NAdEx area, while the open Adriatic dense waters came only at the very bottom of the Kvarner Bay (where CTD sampling has not been carried out).

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.

- We agree, however such a computation will direct the paper to new investigations, which in our opinion deserve a throughout analysis and may go into a separate paper. Thus, we will just discuss the idea in the text.

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.

- Yes, the velocity of dense waters are generally smaller than overall velocities, however, the volume of dense water is much smaller than of overall NAdEx volume, while its outflow is occurring not just at the bottom (if density threshold is used for definition of dense waters). For that reason, DW residence time is smaller than total volume residence time.

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- The most of references are relevant for the deep convection, which is here not the case. We will add a few of them to the revised manuscript as general references on the dense water formation.