Response to the referees' comments on the manuscript "Exceptional dense water formation on the Adriatic shelf in the winter of 2012" by H. Mihanović et al., submitted to Ocean Science (os-2012-115)

Referee #1:

General Comments:

The manuscript presents exceptional DWF events in the Adriatic Sea during the winter of 2012. Observational and modelling methods are combined to raise a discussion about excessiveness of shelf convection, thermohaline circulation pattern variability and its effects on regional internal processes.
Observational results suggest that the deep layers of the middle Adriatic (i.e. Jabuka Pit) are replenished under exceptional formation events in the eastern (i.e. inner Croatian waters) and northern Adriatic Sea (i.e. Gulf of Trieste). The comparison of the thermohaline properties with published/historical observations is limited to a few references. An interannual thermohaline timeseries, of all available observations in very coastal areas such the Gulf of Trieste and the inner Croatian waters, should address whether or not these 2012 winter values are exceptional.

- Thermohaline properties observed in the winter of 2012 will be compared with available long-term series in Croatian coastal waters, to show that 2012 winter values were indeed exceptional (see figure below). Also, we will add more references about long-term thermohaline properties, to fortify our hypothesis.

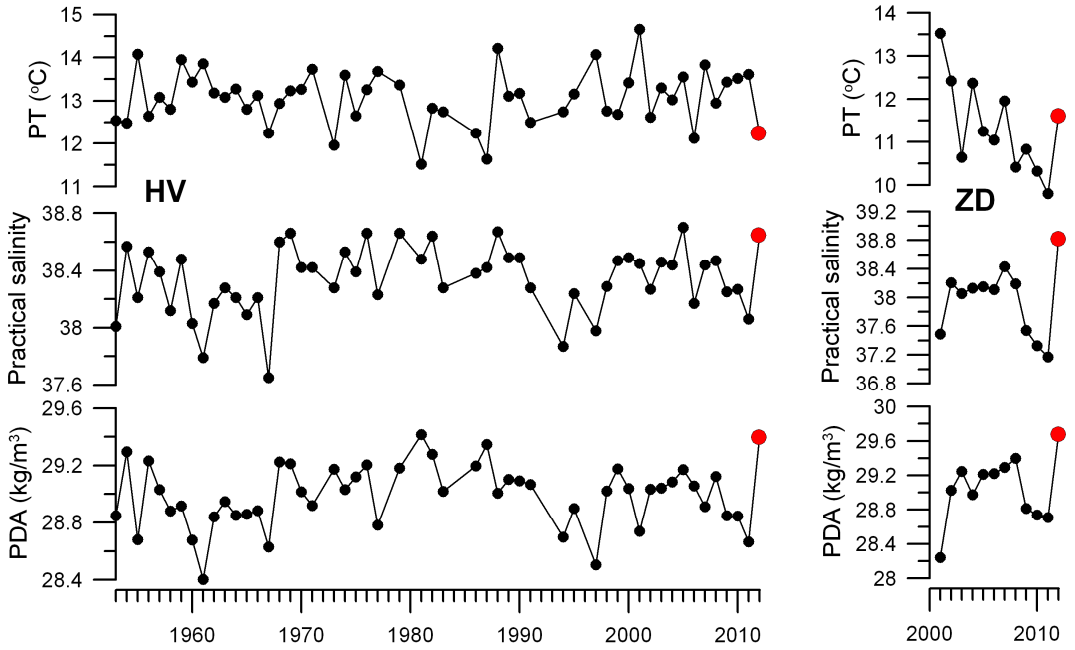

Figure 1. Long-term series of potential temperature (PT), practical salinity and potential density anomaly (PDA) collected at stations HV (30 km south from station ST) and ZD at the maximum density level between 1 February and 1 April. These dates are chosen as the maximum density is occurring during this interval in deep Adriatic Sea. Red circles denote year 2012.

Modelling methods are not analyzed in depth and are only used for box-model calculations. In addition, the authors discuss climate change and its impact on the deep aquatic system without this being adequately justified in the manuscript. The authors use a high-resolution ocean model. However, they do not compare the modelling results with the observational dataset, so as to strengthen the assumptions made for the deep current pathways.

- Climate change discussion will be removed from the abstract and just briefly discussed in the manuscript. The assumptions for the deep current pathways will be strengthened by taking into consideration freshly published paper by Vilibić and Mihanović (2013) and ocean modelling results. Moreover, observational data will be compared with the model, to verify a reliability of the operational ocean model.

  Vilibić, I., Mihanović, H., 2013. Observing the bottom density current over a shelf using an Argo profiling float. Geophysical Research Letters, doi:10.1002/grl.50215, in press.

In overall, a recent observational dataset is used to present the Adriatic's hydrography, while most sections of the manuscript have to be analyzed in more details to increase the article's scientific impact. Therefore, I recommend this manuscript for publication after a major revision.

- Scientific impact will be increased by relating modelling results with the data in more details, and by better description of datasets and campaigns presented in the study and their relevance to the winter 2012 (ad-hoc in-situ measurements determined by the severity of the winter and obtained during and immediately after the event, exceptionality in respect to previous long-term measurements).

Specific Comments:

1) In the Abstract is written that "…connection with climate change are discussed". The manuscript is based on 2012 winter observations and forecast ocean/atmosphere models. Climate change is not adequately justified in the manuscript and therefore should not be mentioned in the Abstract. If the authors want to present climate projections, they should present and justify the methodology adopted and to make extensive changes in the manuscript.

- Climate change will be removed from the abstract and more carefully discussed in the manuscript.

2) Section 2.1 begins with the phrase "A number of oceanographic field campaigns were carried out…", but no reference is given. If the observational dataset from these recent cruises is presented for the first time, then the data collection and analysis procedures should be presented more carefully.

- Most of these campaigns were organized to observe the dense water formation after a prolonged cold and windy 2012 period on the northern Adriatic shelf. The data collection and analysis procedures will be presented in more details in revised manuscript.

3) In section 2.2 the modelling methods are described briefly and in a confusing manner, without providing details to the reader about the models configuration and how these atmospheric and oceanic datasets are processed.

- Similar comment was given by Referee #2. The section 2.2 will be re-written providing more details about the model configurations and how these atmospheric and oceanic datasets were processed.

4) In Section 3, page 3708, the authors state that "To confirm this,… process oriented numerical modelling exercises". In the manuscript a high resolution ocean model is introduced, capable to resolve the inner Croatian elongated embayment, the Gulf of Trieste and many coastal areas discussed in the observations. Process oriented modelling experiments would have been the best option, to monitor Adriatic formation sites/rates and thermohaline circulation pattern variability. However, it is recommended for the authors to study the Adriatic's ocean state prior, during and after the extreme winter of 2012, by processing the data of the operational system and to make connections of the modelling results with the observational findings (e.g. circulation pattern variability, formation sites/rates, density current pathways, entrainment processes etc).

- We agree that the open Adriatic may be properly studied by the operational model, and we will introduce some fresh material on that. However, we should emphasize that the dynamics of inner Croatian channels and embayments may not be properly reproduced by the presented model as: (i) the model resolution is not sufficient to resolve the bathymetry features (e.g. Velebit Channel where heat/water losses were the highest is about 100 km long and just 2-7 km wide, while there are a dozens of channel connections and inlets, e.g. Telašćica (TL) Bay, which is just from a few hundreds meters to 2 kilometres wide), and (ii) freshwater fluxes are not introduced properly to the model, due to insufficient knowledge on numerous karstic submarine spring discharges in the area. Also, east Adriatic rivers have almost no regular and easily available discharge measurements and normally research and operational ocean systems use climatological values. However, that was far from the real situation in 2012 due to prolonged dry period (see the manuscript) and the model didn't work well in this area. That problem was also recognized as important in previous Adriatic modelling studies (there are a number of references on that). We intend to introduce the connection between the model and observations based on freshly published paper by Vilibić and Mihanović GRL (2013) paper, and by modelling of the pathways during and after Jan-Feb 2012 period.

5) I found most of the box-model calculations to be correct if only the atmospheric forcing is taken under account. Why there is not a lateral input in the box-model calculations? Is lateral preconditioning considered to be negligible over the integrated period? How the authors integrated and/or accumulated ocean/atmosphere quantities over time periods in an operational system? The calculations are made through several forecasting cycles or there is a single analysis run? A more detailed discussion is needed.

- We neglected lateral input in box model calculations as acting on longer timescales than the atmospheric forcing – we believe that it is a fair approximation for the manuscript topics. We did the calculation though a number of forecasting cycles, as the Aladin/HR model has been

executed daily and COSMO/ROMS model daily as well. We will expand this section in order to provide the requested information.

6) The estimated formation volume of NAdDW by about 4250 km3 is converted to an average transport by about 0.55 Sv over 3 months and compared to estimations given by Vilibić and Supić, [2005]. In the referenced paper I only found annual formation rates by about 0.05 Sv and not transport rates on shorter time scales. Furthermore, if volume converted in annual formation rate we get ~0.14 Sv, which is about 3 times larger than the typical formation rate and not an order of magnitude as stated by the authors. This part of the box-model volume calculations must be presented more clearly.

- We will revise and expand the calculations and discussion and add more relevant references to the manuscript. For example, Artegiani et al. (1997) documents 0.07 Sv yearly rate but for NAdDW with density > 29.2 kg/m3, while our calculations give 0.14 Sv for NAdDW with density > 29.5 kg/m3. If we lower the density threshold to 29.2 kg/m3 then the most of the south Adriatic will also be full of dense water (however, this is not NAdDW but AdDW). Therefore, the choice of the threshold is largely determining the volume of the water, and our choice is telling that the most of the northern Adriatic was filled by very dense water (therefore, it is not possible to have larger production values, at least estimated by the volume method).

7) The last two pages of the manuscript (pp. 3711 and 3712) present preconditioning factors and climate change impact not adequately justify. The discussion is based mainly on references and not on the datasets (observational and/or modelling) presented in this manuscript. I would suggest the authors to analyze more about the findings in the operational system introduced in the "Data and Methods" section, in order to increase the scientific impact of the manuscript.

- We will put more details on preconditioning, including the comparison of the observations with long term averages as some stations having long-term dataset, and by including some fresh references (Vilibić and Mihanović, GRL, 2013).

Technical Comments:

1) In legend Fig.1 is written: "transect used in Fig.2c". This should be "transect used in Fig.3".

- This will be corrected.

2) In legend Fig.4a and Fig.5 should be stated that positive/negative values denote gain/loss for the ocean. Also, in legend Fig.4b, 4c, 4d is not explicitly addressed that the data is observational from AA station and not modelling close to AA station. Finally, Fig.5a would be improved if the scale of the colorbar is changed to [-1000 0] MJ.m-2 instead of [-1000 1000] MJ.m-2.

- Legend will state that positive/negative values denote gain/loss for the ocean. Also Fig. 4 legend will be clarified. The colorbar in Fig. 5a will be readjusted.

Referee #2:

General comments:

The article presents observational evidence for deep water convection on the Adriatic shelf and the record-high potential densities reached during the winter period of 2012. It uses the observational data in combination with numerical models to interpret the temporal evolution of properties. In the abstract the authors make a connection between the excessive densities reached during the winter convection in 2012 and climate change, but the discussion in the paper only touches the subject in passing.

- Climate change mentioning will be removed from the abstract.

The relevance of the shelf convection variability for the thermohaline circulation of the Mediterranean should be analysed based on climate model projections and not just postulated.

- We agree, but that is far beyond the scope of this paper. Climate models are still not reproducing properly the processes at fine scales, and no assessment (to our knowledge, gained from personal communication to Med climate model community) of the Adriatic shelf processes in the Mediterranean thermohaline circulations has been done at climate scales. Therefore, we will rewrite some of the text and put as discussion and not as the result of our research.

Overall I am missing a clear scientific question behind the paper and a more in-depth analysis of observational data as well as the modelling results. I recommend this manuscript for publication after a major revision.

- We will emphasize more the connection between the observational data and the modelling results, particularly focusing on the comparison of observational data with available long-term time series (see also general comment given by the Referee #1).

Specific comments:

The text is full of abbreviations such as PT for potential temperature or DWF for deep water formation which make the text more difficult to read and do not save that much space. I would recommend skipping as many of these abbreviations as possible.

- We will try to skip using abbreviations as much as possible.

Introduction:

The authors mention that the contribution of shelf generated dense water is underrated, but it remains unclear what could be the importance of shelf convection for the thermohaline

circulation. The authors should present more comprehensive summary about the importance of shelf convection on the larger scale if they are going to make a connection to climate change.

- We will add more references and text about the importance of shelf generated dense water for thermohaline circulation (e.g., freshly published paper by Vilibić et al., CR, 2013).

  Vilibić, I., Šepić, J., Proust, N., 2013. Observational evidence of a weakening of thermohaline circulation in the Adriatic Sea. Climate Research, 55, 217–225.

Does the sentence 'Although extensively investigate' (line 21-24, page 3702) refer to modelling studies only or does it also cover observational studies?

- It refers to both modelling and observations studies on shelf dense water generation at climate scales. E.g., the Adriatic shelf dense water processes have not been analysed by climate models, and were only sporadically analysed by multidecadal observations (e.g., Vilibić et al., CR, 2013). However, dense water generation at the process scale (e.g., case study research on dense water generation) has been done more frequently and there are a number of references on that. We will clarify the text on that.

The introduction mentions a multidecadal monitoring effort but the time period remains unclear as well as the number of actual campaigns carried out. Being at sea during harsh winter conditions can be a challenge and probably there are big gaps in the long-term record. How confident are the authors that all major atmospheric cooling events are covered by measurement campaigns and that densities in excess of 30.3 are rare? What is the role of salinity in that process and how does LSW advection and its changing properties impact the shelf convection?

- The measurements of generated dense water at the Adriatic shelf are not necessary to be taken during the harsh winter outbreaks, as the dense water pools remain at shelf for some weeks, especially at deep layers (there are a number of references on that, e.g., Supić and Vilibić, ECSS, 2006). Precisely, the density excess of 30.59 kg/m3 in Gulf of Trieste was measured just after the cessation of winds and storm, while other campaigns were conducted up to 2 months after the event (Jabuka Pit density was the highest 2 months after the event, as the dense water needs some time to come to the pit). We put more references on multidecadal measurements, particularly on transects where dense water formation is observed few days/weeks after it occurred in the northern Adriatic. Moreover, Gulf of Trieste and available Croatian long-term time series will be given to confirm the importance of 2012 event.

  Supić, N., Vilibić, I., 2006. Dense water characteristics in the northern Adriatic in the 1967-2000 interval with respect to surface fluxes and Po River discharges. Estuarine Coastal and Shelf Science, 66, 580-593.

The introduction ends with the statement that the study will raise a discussion on different aspects of the observed extreme event. This is too vague to be helpful to the reader. Could the authors please specify which aspects they are going to look at.

- We will be more precise with the aspects mention at the end of Introduction.

Section 2.1

Please specify how many field campaigns were carried out between February and April 2012.

- Similar comment was given by the Referee #1. Most of these campaigns were *ad hoc* organized to observe the dense water formation after a prolonged cold and windy 2012 period on the northern Adriatic shelf. The data collection and analysis procedures will be presented in more details.

Sentence: 'Also, vertical …. (line 2-4 on page 3705) is complete.

- The sentence will be rewritten and clarified.

The section mentions an Argo float in the Jakuba Pit, I can't see where the data are used.

- The data from the Argo float are presented in the Figure 1 and corresponding table. The location where Argo measured the largest density is denoted with AR in Figure 1. The same notation will be used in the text, together with appropriate reference for this Argo float data analysis (Vilibić and Mihanović, GRL, 2013)

Section 2.2

I find the presentation of the numerical models confusing. There seems to be a lot of detail information about the models that I am not sure is needed or out of context. But on the other hand I have no idea of the period of the model runs which are analysed, which model parameters are provided as output. What is the reason for using two different models? I don't understand the 'one-way coupling of the COSMO/ROMS system. Which component is coupled and which is not? Is the second COSMO model the same as COSMO-17? I would suggest to restructure the entire paragraph.

- We will rewrite the presentation of numerical models, with the requested information. We used two operational models as the Aladin/HR model is adopted to highly changeable orography along the eastern Adriatic shoreline, and that is the place where highest heat/water losses occurring during bora outbreaks (also true for 2012), while COSMO/ROMS modelling system was not working well there but is better reproducing the physics at the open Adriatic and western coastal waters. As we had dense water generation in both areas, we thought that it might be better to analyse the process by both operational models.

Section 3

The authors postulate that dense water has to flow from the inner Croatian sea to Station BL. But they refer to process oriented modelling-studies to confirm this. Wouldn't the data from the 2km ROMS provide exactly the data base to do this and why couldn't these data be analysed?

- No, it would not be sufficient, as the bathymetry of Croatian channels is too complex (the channels may be just a few hundreds meters wide), demands higher resolution than 2 km and

is not well reproduced by the model. Also, the freshwater income through east Adriatic rivers (which do not have regular and easily available discharge measurements but they are introduced with climatological discharges into models) and numerous submarine karstic springs (which are not possible to properly model due to insufficient knowledge on their locations and discharge rates) are not properly introduced in the model (in fact, no research study has been able to introduce them properly).

The authors also mention the role of salinity in the preconditioning of the deep water convection on the shelf. They cite temperature and salinity trends from climate projections and the potential of warmer and more salty deep waters in the thermohaline cell. Is there a scientific analysis behind the next sentence which claims that the changes in the coastal areas are more rapid and could weaken the thermohaline circulation? Is that just a postulation? What should the process that weakens the thermohaline circulation? The next paragraph presents a different aspect, namely that increased blocking could strengthen the thermohaline circulation.

- Yes, Vilibić et al. (CR, 2013) found that the Adriatic thermohaline cell is weaker today than before 60 years (from observational data), which is assigned to decreasing trend in river discharges observed in the last decades. We will add and clarify the text in this paragraph.

Final comments:

What I missing is a clear connection of all this to the data presented in the paper and making it less speculative or vague.

- We'll restructure the paper to make it more specific and clear, making stronger connection between observations in 2012 and historical records, and between observations and models.

Figure 1 is too small and overloaded with information: I would strongly recommend to remove the table from the figure and move it into the text, otherwise the numbers are much too small to be read.

- Table will be removed from the figure and incorporated independently into the text.

The green line in the subset of figure 1 is barely visible. The figure also contains a schematic circulation scheme for the dense waters generated on the shelf. The source of information for this scheme should be discussed in the text.

- Green line will be enhanced in the figure. The scheme presented in the figure will be discussed in the text, providing sources for this information (Vilibić et al., DSR, 2004, Vilibić and Mihanović, GRL, 2013).

Figure 5: Any reason why this is only calculated for the ALADIN model?

- It is calculated also for COSMO/ROMS model, but we choose the Aladin/HR fields to be presented in the manuscript as Aladin/HR better reproduced the physics along the eastern

shoreline, where the maximum of bora wind, heat and water losses normally occurs (and occurred also in 2012). The text will be clarified accordingly.