

Interactive comment on “Exceptional dense water formation on the Adriatic shelf in the winter of 2012” by H. Mihanović et al.

Anonymous Referee #2

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Reviewer comments Research article os-2012-115 entitled: Exceptional dense water formation on the Adriatic shelf in the winter of 2012 by H: Mihanovic et al.

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. 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. Overall I am missing a clear scientific question behind the paper and a

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more in-depth analysis of observational data as well as the modelling results. I recommend this manuscript for publication after a major revision.

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. 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. Does the sentence ‘Although extensively investigate...’ (line 21-24, page 3702) refer to modelling studies only or does it also cover observational studies? The introduction mentions a multi-decadal 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 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. Section 2.1 Please specify how many field campaigns were carried out between February and April 2012. Sentence: ‘Also, vertical ...’ (line 2-4 on page 3705) is complete. The section mentions an Argo float in the Jakuba Pit, I can’t see where the data are used. 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

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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. 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? 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.

What I missing is a clear connection of all this to the data presented in the paper and making it less speculative or vague. 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. 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. Figure 5: Any reason why this is only calculated for the ALADIN model?

Interactive comment on Ocean Sci. Discuss., 9, 3701, 2012.