Ocean Sci. Discuss., 9, C1608–C1611, 2013
www.ocean-sci-discuss.net/9/C1608/2013/

Ocean Science Discussions

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

Anonymous Referee #1

Received and published: 13 February 2013

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

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

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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.

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.

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.

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.

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.

3) In section 2.2 the modelling methods are described briefly and in a confusing man-

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ner, without providing details to the reader about the models configuration and how these atmospheric and oceanic datasets are 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).

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.

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 $\sim$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.

7) The last two pages of the manuscript (pp. 3711 and 3712) present precondition-

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ing 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.

Technical Comments:

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

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.

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