Anonymous Referee #1

General comments

This paper presents an important aspect of the oceanography of the Adriatic Sea. The relevant scientific questions addressed are within the scope of OS. The title clearly reflects the contents of the paper. The model set up is appropriate with suitable horizontal resolution. Nevertheless, in the text there are some vague conclusions, which are not supported by convenient analysis, or figures, they are not based on valid assumptions and they are not clearly outlined. In some cases the results are not sufficient to support the interpretations and conclusions and some statements regarding data shown in figures are not obvious and concise for the reader. Common terminology is not followed. For example the term "event" is not always preceded by the same characterization (it is either cooling/warming event, or seasonally defined (e.g. summer2003), or yearly defined (2001 event) or just referred to as event). General statements about the model performance without proper validation of the model with the observations should be avoided. In some cases it is not clear if the authors discuss model results or observational data. The conclusions paragraph is too long, with too many details on results already discussed inside the manuscript.

We thanks the Anonymous Referee#1 for his/her important and detailed comments on our manuscript. We have prepared a new version of the manuscript trying to improve the description of the results and avoiding vague conclusions, especially when are not supported by convenient analysis. In addition a new manuscript has been considered in support to our finding (Cardin et al., Continental Shelf Research 2011, "Variability of water mass proprieties in the last two decades in the South Adriatic Sea with emphasis on the period 2006-2009"). In the new version of the manuscript also the terminology has been improved and the structure of the manuscript changed according the Referee's suggestions. We believe the new version of the manuscript is much more readable and the presented scientific conclusions are more robust.

Specific comments

Abstract. A summary of the model deficiencies should be included.

In the new version of the manuscript this detail will be included.

1. Introduction. In the introduction, there is no reference on the evaporation and precipitation rates of the basin, the Otranto Strait regime, the definition of MLIW, although all these parameters and features are thoroughly examined in the paragraph where the model results are discussed.

We thanks the Reviewer for this comment. Reference to evaporation-precipitation rates of the basin and Otranto transport will be introduced in the revised version of the manuscript. For what concerns the MLIW, it is already mentioned in the introduction pg 569 lns 10 and 23.

2. Model description

3. Data and methods !It would be more convenient to use this name instead of results and discussion for the first 30 lines of section 3.

We thanks the Reviewer, in the new version of the manuscript the proposes structure will be used.

4. Results and discussion
4.1 Heat and water fluxes It would be helpful to estimate the mean annual heat budget at least for the Adriatic basin and include it in figure 2.
A new version of Fig.02 has been prepared and it will be inserted in the manuscript.

Explain what kind of time series are presented in figure 2 : monthly means?
This info has been included in the caption of all the figures. Thanks the Reviewer for pointing out this important issue.

p.574,lines 16-17 :"The autumn and winter cooling exhibits a strong inter-annually variability mainly due to the latent heat flux component of the surface heat balance" This conclusion is not supported by a figure. At least you should mention that, if a figure of the heat budget components is not possible to be introduced.
The Referee is right, Since the manuscript has already a large number of figure and in addition we think this aspect is not crucial for the discussion we preferred to modify the sentence clarifying that Latent heat flux time series is not shown.

p.574, lines 17-22 : "Thus: : :..heat fluxes". This conclusion is somewhat arbitrary and definitely not clearly stated and not supported by the analysis of the results. Moreover what is written about summer 2003, is not obvious in figure 2. At least you could refer to the actual value of the "significant larger than climatology" summer heat flux of the year 2003.
We thanks the referee for this important comment. In the new version of the manuscript the first sentence has been removed from the text, while the comments on the anomalies have been rewritten.

p.574, lines 23-26 :"The mean annual heat budget ..basin" Are you referring to the surface heat budget? If yes again the conclusion about the overall heat budget of the basin is arbitrary, vague and it is not supported by any analysis of the model results.
Thanks the Referee for this comment. The statement will be modified specifying that the heat budget is referred to the surface and the sentence on the Otranto contribution removed, since we agree with the referee it was a speculation.

p.575, line 14-15 : How do you define seasons of the year? Autumn ! November ,December??? Please clarify.
Thanks again to the Referee, seasons definition will be included in the text.

p.575,lines 27-28 and p.576 line 1: "With the sole: : :.heat losses" This is a very confusing sentence. Are you referring to the annual heat flux at the strait?? Are you referring to all the year or only to winters? Are you referring to the all the years 2000-2008? For example 2007 net heat flux in Otranto strait seems to be negative.
We partially agree with the Reviewer (the period is clearly stated winters 2003-2004-2005-2006), however the sentence will be rephrased in order to make it clearer.

p.576, line 5: "large heat anomalies , not as large as in other periods" ???? ! how large, which are the other periods! please be accurate and concise.
We thanks the referee for this comment, the sentence will be rewritten.

p.576,lines 5-6: "the extended length of this episode has largely affected the thermohaline characteristics of the basin" How?
The detailed description of the thermohaline characteristics is provided in a dedicated paragraph. However we agree with Referee suggestion and a sentence anticipating the detailed description will be added in the new manuscript.

From page 575 line 14 to page 576 line 17, this part of heat and water fluxes paragraph is very confusing! I think it should be rewritten using a common structure. For example, describe the major cool or warm events first during autumn-winters and then during summers, and in a separate paragraph the advective heat flux in the Otranto strait. Do not go back and forth from cooling seasons to warming seasons. Keep the same methodology for all the sections
In the new version of the manuscript this part of the paragraph will be re organized following the Reviewer's suggestion

p.576, line 23-24 : " Starting : : : seasons" It is not clear if you refer to the evaporation rate or the heat flux. Moreover your statement is not accurate. From the figure 4 it seems that during autumn-winter 2006-2007 the evaporation rate anomaly is negative.
The sentence will be rephrased.

p.577, line 1-5: Please use references to support your statements
Reference will be included.

Figure 5: Please clarify that this diagram is derived using model results.
A better explanation of the diagram will be included.

4.2 Temperature and salinity characteristics
p.578 line 14-16: "However ..a longer period" How do you justify this statement?
The statement will be rephrased.

p.580, line 29 "2007 event"! define the event, define the season, use a common way to describe the "events" in this and previous paragraph.
In the new version of the manuscript the usage of the word "event" will be modified following the Reviewer's suggestion.

p.581,lines 11-12: The difference between : : :.mixing" if you refer to the model results, please write it.
This will be stated clearer in the new version of the manuscript.

p.581,lines 3-8: " The successive : : :..2004" How do you know/justify that statement? What are the "normal values"?

Reference to the figures will be introduced and "normal" specified.

p.583, line 17: "This missing : : :. mixing" or it could happen the opposite! This is not a justification, but a possibility about the positive model bias.
The referee is right, the sentence was just a possible explanation and surely not crucial for the discussion, in the new version of the manuscript it has been removed.

p.583, line 25-26: "However: : :simulations" This is not true, observations of the years 2002 and 2003 have very different values from those the model results as you mention later in the text. Please avoid such general statements.
The Reviewer is right, in the new version of the manuscript this sentence will be removed.

p.584, line 9-11: "Thus : : : following years" This is not a valid assumption and it can not be an accepted validation for the model.
Referee is right, sentence will be removed.

p. 584, line 12-13: The increase…….: August 2006 cruise" You have only 5 points in 9 years to compare with the model results, out of which at least 3 (2002,2003, Jan 2006 1st) have very different value, so you can not argue that the sole exception is 2006.
The Reviewer is right, in the new version of the manuscript the sentence has been modified and additional reference to Cardin et al 2011 included.

4.3 Dense waters
The calculated formation/dissipation rate on a daily basis is very interesting and gives important information. But how do the authors use this indicator and how do they estimate the formation rate of dense waters from this indicator?
The formation rate is computed from the volume of dense water, so we first computed the daily volume of dense water in each sub-basin (blu line in Fig.11):

$$V_{dense} = \sum_{i,j,k} dx(i,j,k)dy(i,j,k)dz(i,j,k)$$

Where dx, dy and dz (all in km) are the horizontal and vertical model grid scale factors and are different from zero only in the model grid points having sigma-t larger than 29.2.
The formation rate (black line in Fig.11) is then computed according:

$$DW_{rate} = \frac{\partial V_{dense}}{\partial t} \text{ or in discrete form } DW_{rate}(t) = \frac{V_{dense}(t+1) - V_{dense}(t)}{\Delta t}$$

The idea od using this indicator has the purpose of enhancing the interannual variability, which from a simple plot of the volume could be underestimated.
However we did not find in literature similar computations, this is why, when referring to previous studies we also calculated (even if it is not shown in the manuscript) the "seasonal" rate. If the Reviewer thinks that the explanation using the formulae can help the reader in understanding the discussion we can include it in the new version of the manuscript.
They only mention and compare with other calculations the "seasonal" formation rate which is not shown.

We partially agree with the Reviewer, because the formation rate is already commented in the text at Pg 586 ln 2-3 and 19, Pg 588 ln 20-21. However further details about this diagnostic will be introduced in the new version of the manuscript.

You mention the term " formation velocity". What does it mean? Do you refer to the duration of the process?
We thanks the Reviewer for this comments, the word "velocity" used in the text was misleading, and was used to indicate the formation rate. In the new version of the manuscript the word will be substituted using an appropriate terminology.

Mantziafou and Lascaratos 2008 has shown that the amplitude of the interannual variability of the deep water formation in the Adriatic basin is not proportional to the mean winter buoyancy loss but it is highly dependent on the high frequency variability of the atmospheric forcing over the area under convection. Also Mertens and Schott (1998) have shown for the DWF processes in the Northwestern Mediterranean, that the longer the time period over which the buoyancy loss is distributed, namely when the buoyancy loss events are not frequent, the more important is the effect of lateral buoyancy fluxes. These fluxes disturb the local balance by bringing more stratified water at the surface and thus preventing convection, or reducing the rate of the mixed-layer-depth development. You can use such information to justify why "the formation velocity" and the relative DW volume formed is different from year to year. Mertens, C., Schott, F., 1998. Interannual variability of deep convection in the north-western Mediterranean. Journal of Physical Oceanography,28, 1410–1424
We thanks the Reviewer for this comment, explanation and reference will be included in the new version of the manuscript.

p. 587 line 14 : " and consequently : : :. values" The way it is written it gives the impression that the increasing salinity , increases the temperature. Please rephrase.
The sentence will be rephrased

p.588, line 18 : You can refer to Mantziafou and Lascaratos 2004 who have shown the existence of "memory" in the Adriatic basin and have concluded that the DWF rates in the basin depend not only on the heat forcing of the present year but on the time history of the heat forcing as well.
The reference will be included in the new version of the manuscript.

The paper of " Manca B.B., Civitarese G., Klein B., and Ribera d'Alcalà M. (2004), Dense water formation in the Southern Adriatic Sea Associated with variations of the thermohaline circulation in the Ionian Sea during 2001-2002, Rapp. Comm. int. Mer Médit., 37, 122.) could be included in the references. The authors could possibly find important information on deep water formation process in Adriatic basin for 2001-2002 to refer to.
We thanks the Reviewer for this suggestion, however after reading the suggested manuscript we did not find relevant information justifying the inclusion of the manuscript in the reference list.

Climatologically, dense water locally formed in Southern Adriatic has sigma-t lower than 29.2 That is another reason for the low formation rates calculated in most of the years of investigated period, which I think it should be mentioned.
The Reviewer is right, a sentence clarifying this point will be included in the dense water section of the new manuscript.

Details on DW formation in 2006-2008 should be transferred from the last section (summary and conclusions) here.
We thanks the Reviewer for this comment, in the new version of the manuscript the discussion on DW formation during 2006-2008 will be moved to this section.

5. Summary and conclusions.
This section should be shortened and rewritten. There is no need for so many details already mentioned in other sections of the manuscript. Write only substantial conclusions.
We partially agree with the Reviewer, in preceding sections hydrographic characteristic, heat fluxes and DW formation have been discussed separately, while in the "summary and conclusion" section we try to concatenate fluxes-thermohaline proprieties and DW production in order to have a complete picture of the system. However we have prepared a new version of the section, removing, as suggested by the Reviewer, the discussion on DW formation during 2006-2008 and some details already discussed in the manuscript.

The deficiency of the simulation of MLIW intrusion is not mentioned.
We do not agree with the Reviewer, model deficiency in simulating MLIW intrusion is mentioned at Pag.591 ln5-6-7. However last section has been modified following the suggestions, and we hope it is clearer in the new version of the manuscript.

Technical corrections
1. In my view, it would be more convenient to use the following paragraph structure 1.Introduction 2. Model Description 3. Data and methods (instead of results and discussion) 4. Results and discussion 4.1 Heat and water fluxes 4.2 Temperature and salinity characteristics 4.3 Dense water mass formation 5. Summary and conclusions
The new version of the manuscript will follows the structure suggested by the Reviewer.

2.In all figures with time series write what kind of time series you present?. Mean monthly values? Mean daily?
Figure captions will be modified accordingly.

3. Use present and not past tense
We prefer to maintain the use of the past tense.

4.When you refer to data, it would be helpful to mention the color used for each cruise in a parenthesis so as to help the reader.
The text of the manuscript will be modified accordingly.

5.Use values instead of adjectives like : significant larger than , : : :.to describe the magnitude of a flux, parameter etc.
The text has been modified

6. In Figure 1 a) in T-S diagrams use colors for different cruises data as in the Adriatic map b). Is the model grid rectangular? Use a similar to fig2. From Oddo et al. 2005 figure to show the two different grids.
A new version of Fig.1 has been prepared following the Reviewer suggestions and will be included in the new manuscript

7. Please refer to the color of the CTD data cruise everytime (e.g. August 2006 (cyan)). It would be very helpful to the reader.
See answer to comment-4.

Typing errors
Abstract:
investigated period or period under investigation.
Introduction:
Page 567 line 5 : The river runoff:.. -! start a new paragraph
page 570 line 18 ! this is later
Model description:
page 573 line 2 : a horizontally averaged density is subtracted before computing the baroclinic integrals.
Heat and water fluxes :
page 575, line 5 : who instead of which
Page 576, line 20 : evaporation rate
Page 577, line 13: MLIW is absent
Temperature and salinity characteristics
Page 578, line 14: almost 3 instead of 2/3
Page 578, line 15: short intervals of strong
Page 581, line 27: it is interesting to note that this anomalous.
Page 582, line 21: excessive vertical mixing
We thanks the Reviewer for this patient work, all the suggestions will be applied in the revised version of the manuscript.