Review of manuscript "Qualified temperature, salinity and dissolved oxygen climatologies in a changing Adriatic Sea" by Lipizer et al., submitted to Ocean Science (osd-11-331-2014)

The manuscript provides new estimates of climatological temperature, salinity and dissolved oxygen seasonal fields in the Adriatic Sea at a number of vertical levels and using centurial 1911-2009 dataset and a new algorithm (DIVA). The latter two issues are a new value of the manuscript compared to previously published Adriatic climatologies, and therefore the material is worth of publishing. However, there are very large number of problems and defectives in the paper which should be solved before eventual acceptance. These are:

1. The discussion is embedded into results, what makes following of the text pretty hard. The text should be written more concisely and without too many assumptions that are not properly proved in the present analysis or by existing literature. For example:
"Dissolved oxygen concentration in the surface layer presents a clear seasonal cycle, influenced firstly by water temperature and salinity, gas solubility in seawater being inversely correlated with temperature and salinity, and secondly by biological processes of plankton production and respiration." (p. 345, l. 21-24). How do you know what is the primary and what is the secondary process which influence DO in climatological fields? I think that this is true everywhere except the area close to the Po River delta and upper parts of the WAC. However, you cannot explicitly say that without an appropriate indepth analysis of these two processes. I suggest omitting such generic and unproved sentences from the manuscript.

> **Authors:** Part of the result section has been accordingly modified, removing generic assumptions. The presentation of the results is now more concise and focuses on the new outcomes of this study in comparison with previous climatologies. Generic assumptions have been omitted or have been supported by appropriate references.

2. Why you did not use AOU in climatological maps rather than DO? Then you will eliminate the effects of temperature and salinity on which you are referring throughout the text. You used AOU in section 3.4 only, why not to use it everywhere?

> **Authors:** We didn't calculate climatology for AOU for the following reasons:
>
> 1. AOU is a derived variable, which is calculated from temperature, salinity and dissolved oxygen. As such, its value may be influenced by the cumulative sum of errors in the measured variables. In this analysis, we have dedicated special attention to quantify the "uncertainty" of climatological maps, which may derive from the sum of several sources of errors (e.g. synopticity and representativeness errors) as well as from the real variability of the oceanographic structures. The results of Section 3.3 (Climatology-observation misfits, Fig. 6 and 7, Table 1) show that the "uncertainty" associated to temperature, salinity and dissolved oxygen (mostly in the surface layer, Table 1) has a different spatial and temporal pattern. Therefore, the distribution of the approximated values of the derived variable AOU would be influenced by the combination of the different patterns. In this case, the combined uncertainty (derived from uncertainty in temperature + salinity + dissolved oxygen) would be difficult to interpret. Conversely, in section 3.4, after having calculated the uncertainty associated to each variable (Table 1) and relying

on the results that uncertainty, as a general pattern, decreases at increasing depth, we used AOU to have a qualitative indication of the relative age of the deep waters (the last ventilation episodes).

2. Climatological layers are particularly useful for modeling experiments which mostly use dissolved oxygen (e.g. biogeochemical model initial conditions, validation protocols).

3. Do you think that computations of the Adriatic climatology make sense at 100 years timescale, while the trends (heating, salinisation, deoxygenisation) and variations are occurring at decadal scales? Why not to have climatologies at 30-year intervals, like in atmospheric climate sciences? This should be commented properly in the discussion; both strengths and weakness of such an approach

Authors: We are aware of the fact that recent protocols indicate 30-year time-scale as appropriate for atmospheric climatologies (IPCC, 2001), but in the ocean multidecadal (60 to 90 years) temperature variability has been found, at least in the North Atlantic (Schlesinger and Ramankutty,1994). In the case of the Mediterranean, the Eastern Mediterranean Transient and the Bimodal Oscillating System have been recognised to have decadal time-scale, but their impact and their modulation in time may have longer time-scales. It is therefore difficult to demonstrate that 30 years is appropriate for the Adriatic basin.

Schlesinger, M. E. (1994). "An oscillation in the global climate system of period 65-70 years". Nature 367 (6465): 723–726.

We share the concerns of the referee on the climatology based on a long time span; this is the major motivation for the analysis on the long term variability focused on the deepest parts of the basin, which are believed as the most “climatically sensitive” areas.

The revised discussion now includes an integration on this important issue.

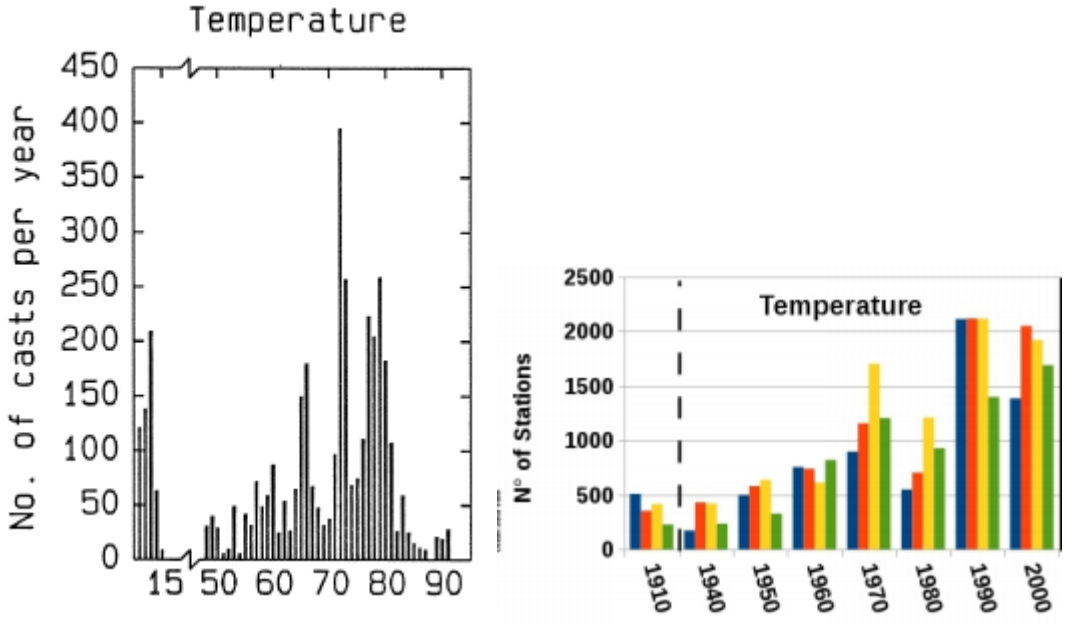
As a side comment, this paper aims at providing the overall state of the Adriatic, focusing on several depth layers which were not studied in earlier climatological studies (e.g. Artegiani et al.,1997b; Zavatarelli et al., 1998; Russo et al., 2012).

4. How do you comment on the fact that “our climatology, which includes almost two more decades (1991–2009), provides higher temperatures (up to +2 C) over the whole basin in spring, summer and, in the case of the northern and the southern part of the Adriatic, also in autumn” (p. 348, l. 19-22)? The time interval in previous climatologies is 80 years, the time interval in your climatology is 100 years, and the difference in sea surface temperature is up to 2 deg C. Conclusively, the average temperatures in the last two decades (between 1992 and 2009) should be at least 7-8 deg C higher than before? Or the quoted difference is a result of the data availability, which was much more larger in the last two decades than before? Conclusively, the method you applied is not appropriate or previous climatologies were computed badly. Or …? There is no discussion on that in the manuscript - please discuss!

Authors: The temperature difference, which in the most extreme case reaches up to 2°C, derives from visual inspection of the temperature fields (this study) and comparison with data average at sub-basin scale (Zavatarelli et al., 1998). As we agree that this

evaluation should be based on comparable analysis, which is outside the scope of this manuscript, we decided to avoid any quantitative estimate.

However, the difference we see (even if only qualitative) can be ascribed to the influence of the recent decades due to the much larger data abundance. Anyway, also in the analysis of Zavatarelli et al., (1998) the data distribution histogram has a very strong skewness, with larger number of casts in the 1970s-early 1980s.
See comparison of the datasets used (left: Zavatarelli et al., 1998, right: this study: number of stations per decade, per season in different color bars).



The revised version of the manuscript now contains more detailed information on the method applied, the discussion has been integrated with a more focused comparison with previous climatologies.

Some minor to moderate issues:
5. Title. Why qualified? Did you pass any qualifications, who were the opponents? It is better to say "New temperature, salinity …"

Authors: The term "qualified" is motivated by the approach adopted which addresses the issues of validating the approximated climatological fields through several approaches (TS diagram comparisons, Ordinary Cross Validation, misfit quantification and qualification of climatological maps). Details on qualification procedures are reported in Sections 2.4 e 2.5.
As a possible alternative, we propose the term "Validated climatologies".

6. Page 332, line 4. DIVA acronym should be written in full when introducing to the manuscript.

Authors: It has been added.

7. Page 339, lines 22-26. Did you compute the misfit for dissolved oxygen data (measurements vs. climatology)? Did you perform the verification for other depths than 100 and 200 m? I want to see the comparison for all depths and all parameters between approximation and measurements - it may be plotted as a contour graph (x depth – y subset), one for each parameter. Such a plot may substitute present Fig. 4.

Authors: We acknowledge that some potential confusion between Ordinary Cross Validation (Fig. 4) and assessment of uncertainty (later Fig. 7-8) can arise from the manuscript in its present form. The text has been accordingly revised and the basic concepts clarified.

Figure 3 and Figure 4 show two different approaches to validate the approximated fields. The approach described at page 339, lines 22-26 (Fig. 3) is suitable for the comparison between combined TS properties (approximated vs in situ data) which, together, identify a water mass. This comparison aims at showing that the approximation process, done separately for temperature and salinity, maintains the core properties of the water masses. For all depth layers, for the four seasons and for each sub-basin (North, Central and South), TS approximated fields have been compared with the climatological TS diagrams reported in the literature (Artegiani et al., 1997a) for the different water masses known for the Adriatic (A. Rabitti 2008 Tecniche variazionali e analisi statistiche applicate alla caratterizzazione spazio-temporale dei parametri fisici e biochimici del Mare Adriatico", Master Thesis). The comparison has shown that approximated properties lie within the literature ranges of the different water masses. The same kind of approach is not suitable for dissolved oxygen as it is not a conservative property.

The approach illustrated by Fig. 4 is based on Ordinary Cross Validation which aims at reconstructing the fields in correspondence of positions where the data have been temporarily removed (i.e. do not contribute to the approximation). This exercise has been specifically performed in order to test the robustness of the parameters (correlation length, L, and S/N) used and to assess the predictive capability of the method in absence of information. These results, therefore, are of little use when approximation versus observations misfit has to be estimated. To achieve a better statistical confidence, the approach has been applied to the largest datasets (therefore temperature and salinity).

Conversely, the analysis of the misfit between in situ data and approximated fields has been carried out for all the layers discussed in the paper, for all parameters and for all seasons. The approach is detailed in section 2.5 and its results commented in section 3.3.

8. Page 344, lines 13-17. Distinct pattern may be noticed at 200 m, not at 100 m. Therefore, water masses that are doing the separation at 200 m are NAdDW (at the bottom of the middle Adriatic pits) and MLIW+AdDW (in the intermediate layer of the South Adriatic). Btw, Adriatic Deep Water (AdDW) has not been mentioned in the introduction sections (Section 2.1), it should be quoted properly and deep-convection processes should be described with a few sentences and references.

Authors: The naming conventions of water masses can create often some confusion. Since we continuously refer to Artegiani et al., 1997 (as also requested by the referee), we tend to stick to its water masses definitions. The water mass properties observed in Winter at 200 m (Fig. 3c, d) correspond to those of the MAdW and to MLIW, the NAdW

being fresher. However, as reported by several authors (e.g. Artegiani et al., 1997a, Vilibic et al., 2004, Marini et al., 2006) the middle Adriatic pits are periodically renewed by NAdDW, and the MAdDW originates from the NAdDW moving southward. The core properties of the warmer and saltier water in Fig. 3d well correspond to the MLIW in the southern Adriatic as defined by Artegiani et al., 1997a.
The description of the important processes of deep convection and the definition of the AdDW, which were missing in the submitted manuscript, have now been added in the introduction.

9. Pages 344-345, lines 23-6. Show it, this is important for such type of manuscript (see suggestion for new Fig. 4).

Authors: This part is addressed in Section 3.3 where the spatial (i.e. horizontal and vertical) and temporal (i.e. seasonal) uncertainty (i.e. climatology-observations misfit) in the climatological fields is presented.

10. Page 346, lines 14-17. From plots I can see that low salinity waters spread towards the eastern coast during spring and summer. Also, your statement is somehow in contrast with multi-decadal in situ measurements carried out in the northern Adriatic (see Fig. 3 in Supic and Vilibic, Estuarine Coastal and Shelf Science, 2006; see also Supic et al., Annales Geophysicae, 2004). The spreading of the Po River waters towards the eastern Adriatic starts in March, has a maximum in June-July, and lasts up to November, while no halocline is present there during December-February period in the most of the northern Adriatic. So, you have a problem with the dataset or your method is filtering out mesoscale features in the northern Adriatic or you should do the reinterpretation of your results.

Authors: The comment is appropriate: a weak signal of spreading of the Po River plume is present in the surface salinity field in winter (January – March). This is consistent with the March onset of well detectable Po plume. The spring (April – June) and summer (July – September) patterns are consistent with the comments. The text has been accordingly modified.

11. Page 346, lines 22-24. In which season? Winter?

Authors: Yes, it has been specified.

12. Page 346, line 24-27. I don't understand this sentence. There is no estuary, just a river delta, and the sea in front of Po River delta is two-dimensional. How you can compare it with one-dimensional estuary?

Authors: This sentence has been deleted from the paragraph.

13. Page 347, lines 7-8. From Fig. 2 I can see that you have just a very few measurements in the Quarner area, and generally in the coastal northern eastern Adriatic. No comments and conclusions can be drawn from such a poor dataset.

Authors: We acknowledge the comment and have accordingly removed conclusions regarding the eastern Adriatic coastlines, where data are sparse.

14. Page 347, lines 8-10. What about comparison to previous climatologies based on in situ data, such as Artegiani et al (1997a, b)? This climatology should be more extensively used in comparison throughout the manuscript.

Authors: In the revised manuscript, earlier climatologies are more extensively considered and compared with this study and only additional relevant patterns recognized in this study have been emphasized.

15. Page 347, line 10-12. Where? I can see a warm eye very close to the eastern coast, which is probably a part of the EAC and not a separate feature as you quoted.

Authors: This sentence has been eliminated and only the major patterns have been discussed.

16. Page 347, line 14. "As in winter, …". Maybe in contrast to? That is opposite to longterm studies (Supic and Vilibic, 2006; Supic et al., 2004) - see previous comments.

Authors: Yes, this sentence has been corrected according to the comment and the suggested citations have been added.

17. Page 347, lines 25-27. I cannot see this, at least not relevant and significant. I can see colder water close to the Albanian coastline, which are probably a result of strong rivers there. And again Quarner area with very few data is commented.

Authors: This part has been removed.

18. Page 348, lines 4-6 and through the text. Are there any connections between DO seasonal changes and primary production in the northern Adriatic? I believe that there are a number of references on that and this issue has not been appropriately discussed in the text.

Authors: This part has been improved with reference to the biological effect on seasonal DO evolution. Throughout the manuscript, this aspect has been better underlined.

19. Page 349, lines 9-11. What may be the reason to have lowest 50-m salinity (accompanied with lower temperatures) pretty far from the Po River delta? It looks like a problem in the data, which has large spatial gap southeast from Gargano Peninsula. Please comment!

Authors: The lowest salinity at 50 m in the Otranto Strait is due to the progressive sinking of the WAC (retaining the signal of the Po River waters) along the Adriatic (Crise and Manca, 1992). In the Otranto strait area data spatial coverage is very high (see Fig. 2a) since it has been target of several research projects (Table A), therefore, the low salinity pattern observed in the Otranto Strait is robust as error field is low (Fig. 6e) and the median uncertainty at this depth level is 0.3% (see Table 1).

Crise A. and Manca A. 1992 Digital thematic maps from CTD measurements. A case study in the Adriatic Sea. Boll. Oc. Teor. Appl., 10 (1), 15-40.

This part has been introduced in the revised manuscript.

20. Page 349, lines 19-21. Please add a reference about "winter-intensified cyclonic circulation" in the South Adriatic or omit the sentence.

Authors: The following reference has been added: Poulain, 1999
Poulain, P.-M. 1999 Drifter observations of surface circulation in the Adriatic Sea between December 1994 and March 1996. Journal of Marine Systems, 20 (1-4), pp. 231-253

21. Page 350, lines 14-16, Fig. 5.21. It is quite interesting that strong horizontal gradients are present at 50-m level in the northern Adriatic. Can you comment on that?

Authors: The oxygen minima in the bottom layer of the northern Adriatic are due to the enhanced biological respiration in correspondence of the Po River influence (Justic et al., 1993).
Justić D., Nancy N. Rabalais, R. Eugene Turner, William J. Wiseman Jr., Seasonal coupling between riverborne nutrients, net productivity and hypoxia, Marine Pollution Bulletin, Volume 26, Issue 4, April 1993, Pages 184-189
The strong horizontal gradients reflect this spatial asymmetry.
This comment has been added.

22. Page 350, lines 22-25. DO concentrations in autumn are much lower than in summer. Why? Effects of primary production? Vertical mixing? Please comment.

Authors: Thermohaline stratification lasts until late autumn. This feature prevents any vigorous ventilation from the surface, therefore DO reduction can be ascribed to biological respiration processes, due to the decline of primary production consequent to nutrient consumption and reduced daily quantum yield.
An explanatory statement has been added to the text.

23. Page 351, lines 4-5. Are you sure that microbial mineralization is in progress at 100 m, pretty far from the bottom? Please put a reference of delete the statement.

Authors: Microbial mineralization is ubiquitous in the central Mediterranean Sea. The sentence has been generalized to include also other bacterial processes (total bacterial production) that affect oxygen consumption. The following references have been added: Solic et al., 2008 and La Ferla et al., 2005
Šolić, M., Krstulović, N., Vilibić, I., Kušpilić, G., Šestanović, S., Šantić, D., & Ordulj, M. (2008). The role of water mass dynamics in controlling bacterial abundance and production in the middle Adriatic Sea. Marine environmental research, 65(5), 388-404.
La Ferla, R., Azzaro, F., Azzaro, M., Caruso, G., Decembrini, F., Leonardi, M., ... & Ribera d'Alcalà, M. (2005). Microbial contribution to carbon biogeochemistry in the Central Mediterranean Sea: Variability of activities and biomass. Journal of Marine Systems, 57(1), 146-166.

24. Page 351, lines 8-11. At 100 m depth bathymetry do not stop the interchange between south and middle Adriatic (that is true for 200 m depth), as the Palagruza Sill is 170 m deep.

In fact, maximum MLIW entrance across the Palagruza Sill towards the middle Adriatic is happening largely at 100 m level (e.g., Vilibic et al., 2013).

Authors: The circulation presented by Artegiani et al.,(1997a) and confirmed by further experimental and numerical simulations shows a clear permanent cyclonic structure, consistent with the geostrophic hypothesis. The advective process through the Palagruza Sill is, therefore, limited by the topography. This in turn somehow decouples the two neighbor pits. Anyway, we smoothened the statement by changing "barrier" with "obstacle".

25. Pages 351-352, lines 24-1. This in contrast with climatology of the deepest middle Adriatic pit documented by Vilibic (2003). In his Fig. 3 the DO maximum is clearly positioned in spring, due to the incoming NAdDW. Also, Artegiani et al. (1997a) gives the DO maximum in winter and spring (see their Fig. 9). So, why your climatology is opposing to this?

Authors: It appears that the referee misunderstood the meaning of the text, because probably the text is not clear enough. The issue is that at 200 m depth seasonal variability is hardly recognizable if compared with the variability between the two pits (only one seasonal map shown). The attention is, therefore, focused mainly on the comparison of the properties in the two areas.

26. Page 352, line 2-4. Spill-over of NAdDW, not MAdDW!

Authors: Yes, thanks for the remark, it has been corrected.

27. Page 355. Change subtitle, as the most of the text is dealing with decadal and multidecadal variability. Something like "Multi-decadal variability and trends in deep waters"

Authors: It has been modified accordingly.

28. Page 360, line 16. Delete "qualified", there were no qualifications.

Authors: The term "qualified" is justified by the procedures adopted prior and after the approximation process as extensively discussed in Sections 2.4. So we are reluctant to drop this term.

29. Page 361, lines 17-19. You should explain why surface temperatures are much higher than in previous climatologies.

Authors: The estimate has been performed by visual analysis and extreme values can be caused by subjective judgments and by the different procedures implemented to obtain the results. See detailed comment in answer to comment n. 4.

30. Page 361, lines 22-24. You cannot explicitly quote that "summer DO bottom minimum is driven by biotic respiration processes" without a proof, so you should put probably or presumably or likely (or similar) in the sentence.

Authors: This has been added.

31. Pages 361-362, lines 26-3. I am not convinced about that, as this is in contrast with well known double circulation structure of the Adriatic thermohaline circulation. Such a strong statement should be proved by a circulation model or current measurements. Please delete these sentences.

Authors: The sentence has been deleted.