Review of manuscript "Qualified temperature, salinity and dissolved oxygen climatologies in a changing Adriatic Sea" by Lipitzer 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 in-depth analysis of these two processes. I suggest omitting such generic and unproved sentences from the manuscript.

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?

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.

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!

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 …"

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

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.

8. Page 244, 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.

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

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.

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

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?

13. Page 346, 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.

14. Page 346, 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.

15. Page 346, 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.

16. Page 346, line 14. “As in winter, …”. Maybe in contrast to? That is opposite to long-term studies (Supic and Vilibic, 2006; Supic et al., 2004) - see previous comments.

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.

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.

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!

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

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?

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.

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.

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).

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?

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

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"

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

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

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.

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.