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Interactive comment on "Qualified temperature, salinity and dissolved oxygen climatologies in a changing Adriatic Sea" *by* M. Lipizer et al.

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1 General comments

The paper presents a new regional climatology of the Adriatic Sea for T, S and dissolved oxygen for the 1911-2009 period, using an inverse method. Overall, the overall presentation is well-structured and the language fluent and precise, but some parts of the paper should be clarified (see next Section).

While the introduction is clear and relevant objectives are properly defined, the result description is difficult to follow for readers not familiar with the regional oceanography, and relies on figures with an insufficient quality.

As a new climatology, it should be compared more thoroughly with other climatologies (following p 348, l 14), either global (even if their resolution would probably be insufficient) or regional (MedSea or Adriatic) products. In particular, it would be relevant to identify the differences/improvements with other climatologies due to:

- the use of a extended data set (longer period, maybe more historical data sources than previous attempts to create a climatology),
- the use of a particular method of interpolation.

More details about the interpolation method (about one paragraph) would be useful. With the description in Section 2.3, it is not clear to me how the method works.

Section 3.2 provides a description of the gridded maps obtained for the different variables, at different depths and for different seasons. Though I see this description useful, my concern is that for readers not familiar with the Adriatic Sea, the interest is not obvious, as it is not clear whether there are modifications or improvement with respect to previous climatologies.

Finally, the core of the work (a set of new climatological fields) deserves more diffusion, so I believe the authors should provide not only figures (also see comments in the next Section), but also the gridded fields themselves (NetCDF format seems adapted for this).

2 Specific comments

2.1 Text

p 332, l 25: A more accurate definition of "*climatology*" could be given.

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p 333, l 1: do numerical models always rely on climatologies for initial conditions? Not on another numerical model with a larger spatial coverage?

p 333, l 15: "which are regarded as providing the most suitable records of possible long-term changes" → can you justify it? Also (and maybe it comes later in the text, but the data scarcity in the deepest layer con constitute an obstacle for studying the long-term changes.

p 333, l 26: they are some references to previous work on climatologies, and it would be relevant to know which interpolation/gridding methods were used for these. Also, what about the global climatologies, such as the series of World Ocean Atlas (http://www.nodc.noaa.gov/OC5/woa13/) or CORA (http://www.coriolis.eu.org/All-news/News/CORA3.4-gridded-fields)?

p 334, l 20: additional plots are available on OGS-NODC web site. → it would be nice to have an url to access the fields as well.

p 337, l 14-15: Quality Control (QC) procedures (Giorgetti et al., 2005; Holdsworth, 2010; SeaDataNet, 2010) which guarantee the consistency of the merged data → I understand that QC improves the quality of the whole dataset, but it is not clear to me that QC can guarantee the consistency.

p 338, l 3-5: concerning the data selection according to depth: how to you deal with possible different vertical resolution of the profiles? Since you select the data in a layer around the depth of interest, you might get more data at locations where the profiles have a fine resolution. Secondly, imagine you have a profile with observations at 340 and 360 m, but no observation between these depths. Does this means that for this profile, for the 350 m layer, you don't have any data?

p 338, l 18: [Bretherton et al., 1976] did not use finite-element method.

p 339, l 2: Finally, OI implementation must consider all the data at the same time → Several OI implementations do not consider all the data points, but rather the data

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located within a circle (with a determined radius) around the point of interest. See for example Menemenlis et al. (1997),
http://onlinelibrary.wiley.com/doi/10.1029/97JC00697/abstract

p 339, l 4: can you define what are the "*nodes*"?

p 339, l 8: the way the error field is computed is merely addressed in the paper. See also Beckers et al. (2014)
http://journals.ametsoc.org/doi/abs/10.1175/JTECH-D-13-00130.1

p 339, l 28: Ordinary Cross Validation: the referenced paper is not the most adequate.

p 340, l 14: can you quantify the number of data points that are repeated on common locations?

p 341, l 5 : Climatology-Observations Misfit: again, it would be useful to know how many times you have various observations at the same location. Do you allow for a deviation from the actual position?

Then what happens in cases (most frequent) where you have only one observation at the same location? The lowest value for COM would be obtained when $d_i = y_i$, i.e., when the observation is equal to the approximated field. In this case, you are with a strict interpolation. So before discussing the COM, the reader would need to know which analysis parameters are selected prior the analysis (what is explained in the appendix).

p 341: The noise is globally retained in the approximation process by selecting . . . $\rightarrow$ this paragraph may better fit in Section 2.3, where the method is described. Also, the whole paragraph is not clear to me. For instance, "a weight by DIVA solver for the minimization of the cost function" does really make sense without further context.

p 342, l 9: depends on the data coverage only . . . $\rightarrow$ and what about the noise associated with observations, as stated previously in the text.

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p 344, l 3: section 3.1: I would expect to have the values of the analysis parameters (see p 339, l 9-11) used for the interpolation prior to the results, even if the procedure to estimate them is provided in the appendix.

p 346, l 24: which season?

p 360, 1st conclusion: it's clear that a new climatology has been created, but the improvements and differences with respect to previous work are not discussed at all. 2nd conclusion: on what is based this affirmation?

p 363; eq A1: what stand d_i, N_d, L, ... for?

p 364, l 3: The correlation length (L) is the parameter that defines the dimensions of the grid → which grid?

p 364, l 4: every grid element is the combination of three sub-elements → could you be more explicit?

p 364, l 4: To avoid sub-sampling, it is required that L/3 is smaller than the scale of the dominant processes analysed. → what is meant by sub-sampling? And why L/3 (why not L?) has to be smaller than the characteristic scale?

p 364, l 15: (L = 0.8 for physical parameters and 1.5 for DO) → this information should be present in the core of the text, Section 2.3 for example.

p 364, l 36: The correlation length and the signal-to-noise ratio are, therefore, directly comparable to the corresponding parameters in OI (Troupin et al., 2012). → what is the relation with the rest of the paragraph?

2.2 Figures

Figure 2: hard to see without zooming in the pdf. It is obvious that more measurements are available, for instance, in the 1990's than in the 1960's. Is that unequal distribution

taken into account when you compute the climatological gridded fields?

Figure 3: looking at panels c) and d) gives the impression that the density anomaly was increased (more dots above 29 kg/m^3 in d). Can you explain that? Also, the diagrams would be more readable if the axis limits were better chosen (for example, the y axis goes until 18°C, while the max. temperature seems closer to 15°C. Another suggestion to make the comparison easier would be to create only 2 panels, one for summer 100 m and one for winter 200 m, using different colors for in situ and reconstructed.

Figure 5: the overall quality of these figures is below the rest (aliasing, resolution, . . .). The labels attached to the isolines are hardly readable, and for some of the sub-figures, it is hard to relate a given color to the colorbar. In addition, it would be more relevant to have the sub-figures corresponding to a common variable (T, S or Oxy) together, in order to easily identify the seasonal cycle mentioned in the text.

As this figure 5 seems to represent a central result of the paper, I would recommend the authors to re-prepare it with a better quality.

Figure 6: the labels (x and y axis, colorbar) are hardly visible

3 Minor comments

p 339, l 9: The methods only requires → method

p 347, l 9: by inversion → by an inversion

p 363: finite element → finite-element

p 364: SNR ratio → remove "ratio" (redundant)

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