

Interactive comment on “Seasonal and inter-annual temperature variability in the bottom waters over the Black Sea shelf” by G. I. Shapiro et al.

Anonymous Referee #1

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The manuscript analyses the interannual variability of the temperature on the bottom shelf water. The subject is potentially interesting in connection with the benthic ecosystem that can be very sensitive to change in water temperature. However, I found the manuscript not really convincing, the scientific approach can be sometimes criticized, some sections have to be rewritten (abstract, discussion and conclusions which are not supported by the materials see my comment below). As I understood, the authors studied the temperature in the bottom layer which is isolated from the surface in summer (~ only 45 % of the shelf according to Figure 3 provided by the authors) and not the whole benthic layer. Also, the title does not really reflect the content of the manuscript. In the introduction, the authors stressed the importance of their study referring to ben-

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thic ecosystem. However, we missed this connection. First, the part of the shelf with a depth < 50 m is not concerned. Then, no connexion with the oxygen dynamics is one neither with the dynamics of the benthos and the temperature change. The authors pretend to investigate lateral exchanges at the shelf break but this type of exchanges involves a lot of high frequency processes that are not treated at all in the manuscript.

This manuscript needs a very substantial revision.

Here below, my detailed comment:

Abstract The abstract has to be rewritten because it is much too long. In the abstract, we expect to find a summary of the objectives, methodology and main results. Here a lot of details are provided. Please clarify why the action of the biological pump is removed in summer when a thermocline is present. Sinking can occur through a thermocline. Page 2, line 15: the authors have to clarify what they mean by energy considerations

The section on the data treatment is very difficult to understand, we recommend a substantial rewriting as mentioned below. Page 5, lines 24-25: A reference has to be provided Page 6, line 17: What is the “first guess”? How do you estimate standard deviation? (temporal variability within a month, spatial variability within a cell?) Page 6, line 21: a reference has to be provided Page 7, eq. 1, it is very difficult to understand how the function $C(r)$ defined in eq. 1 is used in the following. The authors refer to a past paper; I would suggest giving more details here.

Pages 7, lines 9-10: The authors mention that the parameters r_d and r_s are chosen in order to obtain “an optimum balance between horizontal resolution and statistical accuracy of the calculated mean values”. How do they assess that the solution is optimal? What is the reference solution?

Page 7, line 13: how is the density computed? Do you have salinity/density data in all the temperature points? How is computed the “climatic” density?

Page 7, lines 25-26: This sentence is not clear : “In order to estimate uncertainty

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related to combining data collected in different months of the warm season ..” the authors have to clarify the type of uncertainty they are referring to.

We are told that “the intra-annual variability is assessed using temporally and spatially averaged data for each calendar month and then compared to interannual variability of the same parameter.” Where is it done?

Page 8, line 13: the authors have to specify how the lower bound of the CIW is defined in terms of density.

Page 8, lines 15,16: The authors employed energy considerations to identify the boundaries between the water masses. Which water masses? The bottom shelf waters and the upper layer? Page 9, eq. 5: $\bar{\rho}$ after does not depend of the vertical coordinate since it is the vertically – averaged density

Page 9, eq. 7: this is $g/2$ and not g

Page 10, it is not clear why the authors want to compute the vertical penetration of convective mixing. I am wondering why they are not computed the depth of the mixing layer from the density profiles they have. Instead, they use a procedure that require to estimate the amount of energy provided to the system. How is W estimated? Energy driven by the wind? Lines 8-12 are very theoretical examples. How will the author proceed with the real case? Where is it use after?

Page 10, line 6: the authors have to explain the following sentence: “Figure 2b proves the robust link between density levels and mixing energy values.” See also my comments on Figure 2. “This fact provides a physical justification to the preferential use of density, rather than temperature and salinity, levels to define the boundaries of the BSW”. I do not understand. If I am right, BSW is like the mixing layer from the bottom and not from the surface. What do the authors mean when they say that they will use a density value to limit the vertical extension of the BSW? Will they use a defined value? If yes, they have to prove that this value is really the limit of the benthic layer by an

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alyzing vertical profiles. They choose a value of 14.2 for delimiting the homogeneous bottom water, referring to Ivanov. However, Ivanov was studying the CIL and it is well know that the upper limit of the CIL is defined by 14.2. However, it is not clear that the bottom waters on the shelf extends until 14.2.

Page 10, lines 8-17: this paragraph is very confusing. The authors are estimating mixing layer depth from the surface using approach similar to that of Ivanov et al 2000. However, the aim of the authors is to estimate the depth of the homogeneous layer above the bottom and not from the surface.

Page 10, line 9, what is 3.1b?

Page 10, line 20: The authors say that the water column is homogeneous from October until May. What about the Danube plume? Where you can have a strong haline stratification?

Page 11, line 7: the authors say that 45% of the shelf area is occupied by the BSW. What about the remaining 55%? They are not locked because they do not have a density as high as 14.2. Figure 3 shows that in fact most of the shelf waters are not in the BSW because they have a depth lower than 45m.

Page 14, line 15 the authors say that “The near-bottom water body experiences only indirect influence from the atmosphere, and hence has greater inertia, so that time scales for local atmospheric forcing and lateral exchanges due to ocean dynamics become comparable “ I do not agree because due to its small depth, the shelf waters has a small inertia and are affected by atmospheric forcing.

Page 15, lines 1-2: the authors says that “Our calculations show that the near-bottom waters on the western Black Sea shelf below this density level remain largely “locked” i.e. isolated from the effects of surface processes from May to November. I suggest that the authors give strong arguments showing that 1) upper density of the BSW is 14.2, 2) below that density, waters are isolated from the atmospheric forcing.

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Page 15, line 17: please clarify: “However, our isopycnic analysis shows (see Fig. 3, left panel) that surfacing of bottom shelf waters due to such mechanism can only happen in early spring (March– April).” You can also have lateral exchanges at depth. You have ventilation until 150 m. Lateral exchanges at the shelf break involves processes varying rapidly (mesocale). I do not think that using climatic averages density surfaces derived from some averaged data is a reliable tool to investigate this type of processes. This section is not relevant.

Page 16: The authors compute correlation coefficients between the climatic-shelf averaged BSW temperature and SST at the surface. I am also reluctant to this procedure since the large averaging that is performed (over the whole shelf and month) may influence the computation of the correlation coefficients. For instance, when the distribution of data used for performing the average is not the same for the surface and bottom. Page 16, lines 22-25: The authors suggest that lateral export should be important because they can not find correlation between the surface and the bottom. First, this is contrary to what they mention at the previous page analyzing Fig 3, 2) see my previous points about the lack of correlation. After, we have one page of general description of lateral exchanges in the Black Sea which is not a discussion of the results.

Page 18, lines 3-5: The authors mention that their findings are different from those of Ivanov. They should be more specific (what is different and why).

Conclusions: Line 16-20: the authors mention: The Bottom Shelf Water (BSW) is defined as located between the density surface $\sigma = 14.2 \text{ kgm}^{-3}$ and the seabed on the western Black Sea shelf where the bathymetric depth less than 150 m. After reading the manuscript, I am not convinced of that. I would suggest that analyzing and showing vertical profiles the authors justify their findings. Besides, over most of the shelf bottom waters have a density lower than 14.2 see Fig 3

“During the warm season from May to November it is isolated from exchanges with the surface layer and hence has a limited supply of oxygen due to a lack in diapycnic

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mixing.” Once again, there is no proof of that in this manuscript. The vertical exchanges is never analyzed. The authors start from the hypothesis that below above a density of 14.2 waters are isolated and not ventilated by vertical processes. It still has to be proven.

“The use of anomalies rather than absolute temperature values allows aggregating the data spatially and reducing statistical uncertainties.” It needs a reference.

“The novelty of our result is that we now are able to show inter-annual/inter-decadal variability of bottom water temperatures and quantify the relative importance of horizontal (isopycnal) communications as compared to vertical mixing based on a very large data base spanning over more than 50 years.” This is not true. The authors were not able to quantify the importance of horizontal processes on the BSW properties. Instead, they compute some rough correlation between bottom temperature and SST. Since there is no significant correlation, they deduce that lateral transport must be important. However, one page before, they can not find proof of this transport (although the technique used is not appropriate to investigate a high variability processes)

Figures: Figure2: this figure is not clear. Figure a: first we are told that the profiles are for the shelf break and after it is mentioned “mean density profile for the outer shelf is shown in full circles”. So, these curves are for the shelf break or outer shelf? Figure b: what is the mixing energy penetration? The mixing layer? Why is “this mixing energy penetration” in ordinate (instead of depth)? Please clarify “the mixing energy penetration is also shown for density profiles minus/plus 1 standard deviation”

Figure 3: the legend has to be clarified. I would suggest: Climatic averaged depth of the isopycnal layer $\sigma = 14.2 \text{ kgm}^{-3}$ How is it reconstructed? Where are the isobaths? The numbers written on the figure does not correspond to isobaths.

Figure 4: It would be useful to use different color for depicting the limit of the “locked” water body during different months. Once again the term locked has to be justified more appropriately. Figure 5: Please clarify if “the average depth of the BSW boundary

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at the isopycnal $\bar{\sigma}_\theta = 14.2$ means the averaged depth of the layer $\bar{\sigma}_\theta = 14.2 \text{ kg m}^{-3}$ because it is not clear

Figure 7: The change of sign of anomaly occurs when the type of data change and when the authors used MHI data instead of Romanian data. Also, it is really questionable whether this “shift” is not in reality the results of a different spatial coverage of the observations. MHI data most representative of what occurs around Sebastopol Bay and Romanian data along the Romanian coast. Please clarify what represent the standard deviation.

Interactive comment on Ocean Sci. Discuss., 8, 321, 2011.