Authors response to the comments by anonymous referee #1 to the manuscript 'Seasonal and inter-annual temperature variability in the bottom waters over the Black Sea shelf'.

We thank the reviewer for reading the manuscript and providing his/her comments, below we give point by point responses to the comments.

General comments.

Reviewer: 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.

Authors: This is correct. We study the Bottom Shelf Water (BSW) on the productive Western shelf of the Black Sea which is defined as a shelf water mass located between the seabed and the upper mixed layer (page 4, line 11-13 of the original manuscript).

R: 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 benthic ecosystem. However, we missed this connection.

A: The connection to the outer shelf ecosystem is clarified on p4, lines 4-9 of the original MS): 'Habitats with normal levels of oxygen occur ...and in the deeper shelf zone from 40m to the onset of permanently anoxic conditions at around 130m (Zaitsev, 2006).' … 'This water body (i.e. BSW) has an arguably stronger influence on the deeper benthic communities of the outer shelf (depth range 40–130m) than parameters at the sea surface. As we show later in Sect. 3, the areas occupied by the deeper shelf ecosystem coincide well with the spread of the BSW and hence knowledge of temporal variations in the physical state of BSW could help understand the decline and recovery cycles of the benthic ecosystem.'

R: First, the part of the shelf with a depth < 50 m is not concerned.

A: Yes, we are concerned with water masses located below the base of the surface mixed layer. Very shallow coastal waters do not satisfy this criterion, see Fig.4, of the original MS.

R: 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.

A: We use the statistical technique which shows the relative strength of horizontal exchanges independently whether they are generated by high-frequency or low-frequency processes.

Detailed comments.

Abtract. 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.

The Abstract is shortened by 30% as advised.

Please clarify why the action of the biological pump is removed in summer when a thermocline is present. Sinking can occur through a thermocline.

Reference to the reduction of the biological pump due to pycnocline has been removed as this topic is discussed in details elsewhere (e.g. Dave Karl et al. www.msrc.sunysb.edu/octet/biological_pump.html).

Page2, line15: 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.
We agree. The energy based methodology for calculating density of the base of the mixed layer is a specialist technique which is uncommon for a wider oceanography readership and probably needs a separate paper. In the revised MS we omit completely Section 2.2 'Mixing Depth', which shows how the common value of sigma-theta=14.2 can be obtained from energy considerations. Instead we resort to the literature sources where the same level was obtained empirically based on numerous observations (e.g. Blatov et al., 1984; Ivanov et al., 2000, 2001).

Page 5, lines 24-25: A reference has to be provided
The phrase has been re-worded and the references (Vinogradov and Nalbandov, 1990; Yakushev et al., 2005) provided (page 4, line 14 in the revised MS).

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?)
'First guess' climatology is the one calculated before removal of the outliers. Standard deviation includes both temporal and spatial variability within the cell. Clarification is given in the revised MS (page 5 lines 8-9 and 11).

Page 6, line 21: a reference has to be provided
References provided as advised (page 5, lines 14-15 in the revised MS).

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.
The paper (Shapiro et al, 2010a) is freely available from the Ocean Science website where the reader could find details about parameters Rs and Rd. The definition and general properties of C(r) can be found in the original paper by Willis et al. (2004), for which a reference is added (page 6, line 6 in the revised MS).

Pages 7, lines 9-10: The authors mention that the parameters rd and rs 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?
The details are given in (Shapiro et al, 2010a). Basically the increase of Rs and Rd improves statistical accuracy (the error of the mean) as more points are used to calculate each value, but as always this causes degradation of resolution. The optimum combination gives the uncertainty of about 0.2-0.3 °C across most of the areas which is significantly smaller than the inter-annual variability, while preserving horizontal resolution of about 25 km, sufficient enough to confidently separate areas shown in the charts provided.

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?
Density is computed using UNESCO-83 (IES-80) equation of state. It is computed only for those points where both temperature and salinity are

present. Climatic density is computed from climatic temperature and salinity data.

Page 7, lines 25-26: This sentence is not clear : 'In order to estimate uncertainty 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 referring to the statistical uncertainty related to non-identical temporal distribution of samples over the warm season in different years. Clarification is given as advised (page 6 lines 26-27 of revised MS).

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?
In Section 3.1 and Figs.6,7 (original MS).

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: Ro-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, the 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 the 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 analyzing 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.
To avoid confusion, derivation of the mixing depth in Section 2.2 is now completely removed and energy analysis is replaced by literature references, see our response to the comment to Page2, line15 above. The subsections are re-numbered accordingly.

Page 10, line 9, what is 3.1b?
This section is now removed, see above.

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?
The paper actually reads: 'well mixed on most parts of the shelf'. The small area of the Danube plume is not included because the BSW (bounded by 14.2 isopycnal) never comes close enough to the surface to reach the Danube plume.

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.
This is correct understanding of Fig.3.

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.
The BSW are located below a strong pycnocline and are separated from the direct influence of the atmosphere. We include a new Figure 10 (in the revised MS) showing the decoupling between the sea surface layer and the BSW during summer. The waters which are affected by the atmospheric forces are located above the level of sigma-theta 14.2 as it is stated in the manuscript and illustrated by the new Figure 2.

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.
1) Upper density of the BSW is 14.2 by definition. We provide literature references and include a new Figure 2, which clarifies this point.
2) We add a new Fig 10 which shows that there is only a weak correlation between the temperature in the surface layer and the BSW (i.e. below the density level of 14.2).

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).'
Clarification is given as advised on p.16 lines 24-27 of revised MS.

You can also have lateral exchanges at depth. You have ventilation until 150 m.
Yes, we discuss these processes in the paper.
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.
In this paper we use the statistical technique which shows the relative strength of horizontal (isopycnal, to be exact) exchanges independently of whether they are generated by high-frequency or low-frequency processes. The role of mesoscale eddies, filaments, jets etc in cross-shelf exchanges is well known from literature and we refer to such processes on P17, lines 7-10 original MS. The anatomy of exchanges by a

mesoscale eddy is discussed in detail in (Shapiro, Stanichny and Stanychna, 2010, Anatomy of shelf-deep sea exchanges by a mesoscale eddy in the North West Black Sea as derived from remotely sensed data. Remote Sensing of Environment, 114 ,867-875).

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.

We fully agree with this argument. Exactly for this reason we do NOT compute 'correlation coefficients between the climatic-shelf averaged BSW temperature and SST at the surface'. We use anomalies instead of absolute values, the method which dramatically improves the results, see page 5, paragraph 2. The method of anomalies (see Hansen and Lebedeff, 1987) allows to compensate for the uneven distribution of data in the surface and bottom layers and it is the key to the methodology used.

Page 16, lines 22-25: The authors suggest that lateral export should be important because they cannot 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.

To the contrary, we did find the correlation coefficient between the surface (winter) and the bottom (summer), see Fig.10 -original MS, and Discussion. It is R=0.26 on the shelf and R=0.46 in the deep sea. To enhance our argument we include an additional Figure 10 and explanatory text (page 13, paragraph 2 in the revised MS).

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).

The answer to this question is given on page 18 lines 3-6: our analysis shows that 'the findings by Ivanov et al. (2000) ...are only valid for the deep sea but not on the shelf'. In the revised version we correct the grammar mistake ('is' replaced by 'are') which probably caused confusion (page 17, lines 16-18 in the revised MS).

Conclusions: Line 16-20: the authors mention: The Bottom Shelf Water (BSW) is defined as located between the density surface s =14.2 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.

This how we define this water mass. It is now clarified by the new Figure 2 (a vertical profile of density, temperature and salinity).

Besides, over most of the shelf bottom waters have a density lower than 14.2 see Fig 3.

This is probably where the cause of misunderstanding lies. Bottom Shelf Water is the name of the specific water mass, similar to e.g. Antarctic Bottom Water, and NOT a synonym to any waters located near bottom anywhere in the sea. Same water samples taken near the bottom may NOT

contain such water mass, for example AABW does not cover the whole of the Antarctic shelf, and the BSW does not cover the very shallow parts of the Black Sea.

'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 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.
We clarify the use of the 14.2 isopycnal by including a new Figure 2 (vertical profile) and provide evidence for the decoupling between surface and bottom layers by including the new Figure 10 (correlation graph). Literature references that discuss the coinciding of pycnoline, oxycline and halocline are provided (Özsoy and Ünlüata, 1997; Vinogradov and Nalbandov, 1990 etc.) (page 2, lines 23-27 in the revised MS)

'The use of anomalies rather than absolute temperature values allows aggregating the data spatially and reducing statistical uncertainties.' It needs a reference.
Reference to Hansen and Lebedeff (1987) is given in Section 3 of the original manuscript.

'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.
This comment is very confusing. What is 'some rough correlation'? In contrast to what this comment states we do compute Pearson correlation coefficients for both vertical and horizontal (isopycnal) communications and some other statistical links -see Fig.10 of the original MS. The horizontal correlations are significantly stronger than the vertical ones, thus providing evidence for our conclusion.

However, one page before, they cannot find proof of this transport (although the technique used is not appropriate to investigate a high variability processes).
Evidence for the importance of horizontal exchanges is provided by the correlations presented in Figure 10 of the original manuscript. Our statistical approach investigates the outcomes of a variety of oceanographic processes which are discussed in the manuscript.

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'.

This figure relates to the Section 2.2 (energy considerations) and is removed in the revised version.

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

Legend amended as advised. The isobaths are not labelled to avoid clutter. The numbers show the depth of the sigma-theta=14.2 surface in metres.

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.

Amended as advised - the revised Figure 3 now uses colour to show the extent for different months. The term 'locked' introduced in section 3.1 is further clarified in Discussion and by including new Figures 2 & 10.

Figure 5: Please clarify if 'the average depth of the BSW boundary at the isopycnal sigma-theta =14.2 means the averaged depth of the layer sigma-theta= 14.2 because it is not clear.

Amended as follows: 'the average depth of the BSW boundary at the isopycnal σθ =14.2 kgm-3' is replaced with 'the average depth of the isopycnal σθ =14.2 kgm-3 which represent the upper boundary of the BSW'

Figure 7: The change of sign of anomaly occurs when the type of data change and when the authors used MHI data instead if 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.

The MHI database contains the data of the whole Black Sea including most of the data presented in the World Ocean Database (WOD), not only the Sebastopol Bay. It contains more data over the last 15-20 years than the WOD. The WOD does not include most of the Romanian data, so combination WOD+Romanian produces a more homogeneous spatial coverage than the WOD alone. The curve calculated using only WOD+Romanian data set without the MHI has a similar shape but greater noise in the last 15-20 years. The figure shows the 'standard error of the mean' rather than standard deviation. The standard error of the mean is a commonly used measure of uncertainty of measurements and is calculated as the standard deviation of the sample mean estimate of a population mean (see e.g. http://en.wikipedia.org/wiki/Standard_error_(statistics)). The word 'standard' was omitted by mistake and it is now re-instated.