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Ocean Science Discussions



# *Interactive comment on* "Modeling long-term changes of the Black Sea ecosystem characteristics" *by* V. L. Dorofeyev et al.

**M. Bell (Editor)**

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This is an interesting paper which should be worth publishing when it has been revised by the authors.

The two main difficulties with the paper are somewhat linked.

1) I believe that figures 1-10 and 12-18 only show results from the model integrations. The comparisons with satellite data in figures 19-21 are useful but not particularly reassuring. It is difficult for the reader to assess the reliability of the results as they are currently presented. 2) Some of the more important references on the in situ observations are to papers written in Russian. These are relatively inaccessible to western scientists.

The other reviewers have requested clarification on quite a number of important details. I support their requests and do not repeat them here.

The next version of the manuscript should have line numbers. The present version does not even have page numbers !

Section 2.1

Para 4: The Knysh and Moiseenko references are in Russian. It would be helpful to give a better picture of the data from the hydrographic surveys. Some indication of the number of profiles taken per survey and the months surveyed. Perhaps one or two figures showing the distribution of observations.

Para 4: The description of the optimal interpolation is concise. Could you add a brief description of the following details:

(a) How are the covariances specified for the calculation of the monthly climate arrays ? (b) You describe how climate values are calculated for each day. How are these values assimilated (e.g. what value do you use for the nudging coefficients).

Para 5: Could you briefly describe how the variances are specified. Do you interpolate in the vertical ? Are the temperatures and salinities interpolated independently ?

Para 6: By a strobe pulse of +/- 45 days do you mean a 90-day time window centred on the middle of the month ? Could you describe in more detail how the fields are assimilated (what weight is given to the observed field relative to the model background).

Section 2.2: I presume that Korotaev et al 2003 describes the assimilation for this period in detail. If so please refer to it in the last 2 sentences.

Section 2.3

Para 1: Most of this paragraph is redundant (covered in 2.1).

Para 2: Typo “ne-dimensional” in first line.



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Last para: River discharges are evidently crucial. Is the Ludwig reference readily accessible ? Last sentence: Is all nitrate upwelling at 200 metres set to zero ? Is nitrate upwelling confined to a shallower layer (e.g. the UML).

Section 3.1

Figure 1 is it possible to show a corresponding time-series from your monthly arrays (built solely from observed values) ? Clearly there would be gaps in that time-series so the results would have to be presented as values for individual months.

Para 3: Are the values for UML quoted basin averages ? If not what are they ?

Para 4: In winter Figs 2a and 2b appear to show that the temperature increases monotonically with depth in January. The definition of CIL given then only applies for months when there is an intermediate temperature minimum. Because of my lack of familiarity with the Black Sea I was initially rather confused by this paragraph.

Para 5: Fig 3.2 should say Fig 2b. Near the end of the para the ref to "(Fig 3)" could helpfully say "(Fig 3a and 3b)".

Para 6: change "enormously warm" to "very warm" or "exceptionally warm". Last 2 sentences: the 50 metre level is right at the base of a very sharp thermocline. Would it not be better to focus on the volume of water colder than 8oC in summer which does show the variations you intend to describe.

Para 7: The trend in layer 500 – 1000 m is really small isn't it ? (0.0002 K over 20 years). I don't think it is worth highlighting.

Figure 4: Please label the plots a) to e). The scale on d) needs correction (8.6 for all values at the moment).

Para 8: "The temperature increase on 200 m horizon": presumably means to refer to 100 – 300 m depth range and figure d). These changes again appear to be very small and not worth highlighting.

Section 3.2

Figure 5: Same comment as figure 1.

Figure 6: Why are the years 1976 and 1984 chosen here when figures 2 and 3 used 1993 and 1981. Typo in caption “1076”. A consistent choice would increase the value of the figures.

Para below figure 6: Is “1923” correct ? Why is it “possible to conclude .. the fresh water balance . . .” ? Last sentence: should be 0-20 m not 0-50 m ?

Next para: “Thimplis” please check spelling (I think it is “Tsimplis” ? )

Figure 7: This figure shows salinity decreasing over the period at the surface and increasing at depth. This is consistent with the water becoming more stratified over the period and less winter-time mixing. Should your discussion capture this better ?

Section 3.3

Does Figure 8 show variations due only to basin-scale circulations i.e. are the contributions from mesoscale variations negligible ?

The units of the trends might be clearer as m2s-2 per year (note use “s” not “c”)

Section 4

Previous figures (e.g. 4 and 7) have only covered 1971 – 1993. Figures in this section cover 1971 – 2002. I have some concerns that the change in the data available for assimilation will affect the trends previously presented and the ecosystem dynamics. I think you should present the earlier figures for the full period (to 2002) if you have the data available.

Para 1 3 lines above figure 10: “lateral support”. More evidence would be needed to substantiate this assertion.

Figure 11: Why are the model scales (on lhs of figure) 1000 times smaller than the



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measurements ? Does the figure show data from the Purcell model or the integrations described in this paper ?

Para after figure 11: This paragraph appears to describe measurements reported in Konovalov & Murray. Some figures comparing model results with bio-geochemical measurements would greatly strengthen the paper.

Figure 16: Please define the “central part of the Black Sea” more precisely.

Last 2 sentences above figure 17: This explanation is not very convincing to my mind. The slopes of the blue and red curves in figure 17b are nearly equal and opposite after 1992 but the red slope is larger than that of the blue before 1992.

Figures 20 and 21. Does the chlorophyll edge in the SeaWIFS data follow bathymetric contours ? Why is the chlorophyll in the model so much more diffuse ? Is the bathymetric slope in the model greatly smoothed (to avoid pressure gradient errors) ?

Section 5

General question: Are there major sources of nutrients from other rivers than the Danube ?

Last sentence: “coup” should be “cope”. “biomas” should be “biomass” earlier in the page.

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Interactive comment on Ocean Sci. Discuss., 9, 2039, 2012.