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

### **Anonymous Referee #2**

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Review of “Modeling Long term changes of the Black Sea ecosystem characteristics by Dorofeyev et al.”

The manuscript presents the results of a 3D numerical model applied to the Black sea for investigating tis long term dynamics. The model assimilates various data sets (temperature, salinity, currents, sea surface height) and the period of simulation has been chosen when data are available. Since the model has the aim to be used for operational and prediction purposes, this is important to know what are the performances of the model without data assimilation. I recommend that the authors assess model errors (e.g. bias, RMS, correlation) when run without assimilation. Indeed, data assimilation has to be used when knowing model deficiencies. It seems in this case that the model performances strongly rely on data assimilation and it is important to know

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what will be the performances of the model when no such a large amount of data will be available (which will be the case when run in operational). In the simulations presented, we are told that the model assimilates data each day (interpolated data, but the model is corrected daily leaving very little freedom to the model dynamics). Since all the available data are assimilated, the model can not be validated with independent data set and this is not optimal. This prevents testing the adequacy of the assimilation protocol. The manuscript does not investigate which type of data need to be assimilated to improve at the best the results, as well as the spatial and time frequency that is necessary to obtain good performances. This testing is very important because it could give recommendation on the types and frequency of data that are needed in the Black Sea for operational purposes. This manuscript use all the available data sets collected during 1971-1993 and this prevents to validate the approach. Besides, this is sure that in an operational model all these data sets will not be available and this is why I strongly recommend to 1) test the model without assimilation over the period 1971-2001 , 2) to test the assimilation scheme/protocol, 3) to derive recommendations of the frequency/type of data that are needed in operational model. This is not clear whether data assimilation is also used in the ecosystem model. The long period of simulation performed with the ecosystem model is really challenging and I would like that the authors comment on the balance of nitrogen in the Black Sea (how the sources and sinks of nitrogen balance after 28 years of integration). This is surprising since the authors do not represent benthic processes which are important for the balance of nitrogen in the Black Sea (see for instance Friedl et al , 1998 and Friedrich et al ., 2002 who show than benthic denitrification was a critical sink of nitrate consuming the huge stock of nitrate brought by the Danube). General remarks: The authors refer in the analysis of results of generalities on the long term Black Sea dynamics already referred in the literature. Since the authors realized long term simulations of the physics and ecosystem (1971-1992), it is very important that in the revised version the authors analyze how the model represent the long term variability already analyzed in the literature and use the model as a tool to interpret/explain this variability. Besides, most of the figures

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show volume integrated values (volume salinity, phytoplankton, nitrate,..), in the revised version, it would be very relevant to show the long term evolution (1971-1992 period of simulations) of these quantities for the physical model (salinity, temperature, mixing layer) but also for the ecosystem model such as phytoplankton, nitrate, zooplankton and gelatinous, sulfide, oxygen. In all the figures it would be helpful that the authors clarify whether they show model results or interpolated data. At some places in the text, they refer to collected data.

Other Comments: Introduction : please add some references Please provide in the appendix a description of the ecosystem model. For instance: Please specify how hydrogen sulfide is represented in the model (In Oguz work, Iron and Manganese were represented which is not the case here). How sulfide is generated? Please give some details about the representation of gelatinous (are they explicitly simulated or are they forced from the data?) Bottom boundary conditions of the northwestern shelf where sediments are important It is specified that the model only extends to 200m but at page 6 we are told that the model imposes a huge pool of hydrogen sulfide and ammonium, this is contradictory. More important, how the model can reach a steady state conditions? Spin up time? Balance of nitrogen after 28 years of integration? Role of the sediments on the shelf? Do you conclude that sediments degradation of the shelf are not important if one wants to integrate the model over decades? Page 6, section 3: “the reanalysis data collected every five days are used. . .” You mean simulated? Figure 1 is model results? Or data?! would suggest to make the average over the upper layer where changes are important Page 6: The authors conclude that “Such characteristics of the thermohaline structure as the autumn-winter cooling and spring-summer heating of surface waters, formation of the upper mixed layer (UML) and the seasonal thermocline, renewal of the cold intermediate layer (CIL) and formation of a new CIL, decrease of cold reserve in CIL by autumn are resolved by model simulations. “ Please show some comparisons. For instance show the cold content of CIL as defined by Stanev (1995) and compared with data (even if we can expect a good agreement since the model assimilates all available data). Nevertheless this comparison can give informa-

tion on how the assimilation of temperature data allow to represent the dynamics of CIL which is one of the key processes in the Black Sea dynamics. Same idea with the mixing layer depth, stratification index (potential energy anomaly for instance. . .). Please add this comparison with data. Figure 5: Please give a similar plot for the surface salinity. Please explain why the volume average salinity increase from 1971 to 1992 although the Bosphorus inflow is estimated in order to keep the system in balance (as said by the authors above). Why the salinity plot stop at 1992 although simulations where performed until 2009? Figure 7: The variability is much lower than in Figure 5. Did you average the salinity field? Pages 10 and 11: the authors analyze the trends present in the salinity fields shown in Figures 5 and 6. They enumerate a lot of possible factors that can explain these trends but do not identify which is/are the factor(s) actually responsible for these trends. Since the authors have a model, they can use the model in order to select the cause. This is the advantage of using a model compare to only data analysis. Moreover, this is surprising that the authors refer to Repetin et al (2006), Lipchenko et al (2006) although they are not using these data sets (they are using ERA-40 and Ludwig river data). Also, the authors may analyze the trends in the river data they use and atmospheric precipitation they use to force the model in order to investigate the existence of trends in these forcings to explain the long time trends they simulated.

Section 3.3 (pages 12-13): The authors analyze the Black Sea circulation at different depth. For clarity it would be very helpful to have plots of this circulation at different depth and for different years in order to highlight what is described in the text as well as the position of the Kizilirmak, Batumi and Caucasian eddies mentioned in the text. Please compare results shown in Figures 9 and 10 with available observations. Since the model is 3D it would be very relevant to show spatial maps for different years of phytoplankton (ditaoms/flagellates) and zooplankton in order to appraise the spatial structure. Page 15, 1st paragraph: The authors enumerate the conclusions of the Konovalov and Murray (2001) paper on the long term evolution of the nitrate content. Since the authors have realized long term simulations, it is important to check whether

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the model is able to represent these trends identified in Konovalov and Murray (2001). Also, I suggest showing the evolution of the vertical nitracline during 1971-1992 as well as for phytoplankton, zooplankton and gelatinous as already mentioned in my general comments. Vertical plots over 1971-1992 are required. I guess that Figure 12 are data and not model results. Is it correct? Please specify. Figure 16: Please specify the units. I repeat that in the revised version, the authors may provide similar plots but continuously from 1972 to 2002 in order to see the transition from periods because this is very challenging to have a model that is able to simulate this transition especially without data assimilation for the ecosystem. Page 19, the authors refer to a climatic change (warming tendency). Do you identify a climatic trend with your simulations? Figures 19 and 20 Please give some quantitative estimations of the model errors in agreement with the quality required for MyOcean products (for instance is annual average comparison enough for a MyOcean product ?) Conclusions: They authors may provide some conclusions about the ability of the model to be used for operational purposes in the Black Sea investigating the type of data that are needed to reach good performances.

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