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Biogeosciences Discussions

Interactive comment on "Hydrological changes in the Ligurian Sea (NW Mediterranean, DYFAMED site) during 1995–2007 and biogeochemical consequences" *by* J. C. Marty and J. Chiavérini

Anonymous Referee #3

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Review of Âń hydrological changes in the Ligurian Sea (NW Mediterranean, DYFAMED site) during 1995-2007 and biogeochemical consequences Âż by Marty and Chiavérini

The paper analyzes temperature and salinity data collected during the period 1995-2007 at DYFAMED. The paper has two main aims: 1) to derived long time trends, 2) to study a particular deep convection event. Also, from their data, the authors estimated the rate of annual variation of temperature and salinity in the WMDM and LIM from 1995-2007. The study also focuses on the extreme convection event that occurred in 2006. The connexion of intense winter convection with the amount of nitrate in the mixed layer and the intensity of the subsequent phytoplankton bloom is done. The im-

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Interactive Discussion

Discussion Paper

pact of such intense mixing on the phytoplankton groups composition is also estimated. This study is important and is based on the large data set available at DYFAMED. However, some conclusions are derived subjectively from graphics inspection and have to be more nuanced. Since one of the aims of the paper is to derive long time trends at DYFAMED, I would suggest to use the whole data set since 1991 and not only from 1995. I would also recommend to compute regression line and to superimpose them to the presented graphics. Besides, when the authors explains some variations by different factors, I would recommend to quantify the relation between these factors by using correlation coefficients for instance, because as it is now, it is sometimes quite descriptive. For instance, we are often told that winter conditions in 2005/2006 were extreme but we do not have any plots showing the wind stress and atmospheric temperature. Here below are my detailed comments:

Abstract: Please clarify where (which depths) was observed the increased of T and S in 2006. Specify what you mean by biogeochemical characteristics (nitrate and vertically integrated chlorophyll concentrations). Long term evolution of surface salinity not shown. It is very tricky to determine why surface salinity is increasing. Is it due to decreased river discharge? Or is it due to the enhanced mixing with LIW waters? I would suggest to superimpose regression line to your figures (4, 5 and 6) in order to assess the trend. The authors said: “The frequency of extreme events increased in recent years”. This is a very vague sentence, what are your arguments? What do you mean by “recent years”, in the data we saw the presence of an extreme mixing in 1999, recent years means “1995-2006 period”, if yes we need to have information about previous years in order to assess more quantitatively this increased occurrence (what about the FRONTAL data). Last sentence, the authors mentioned that models predicted a decreased productivity in the NW Med, we need a reference. Which models? As it is written, we have the impression that this sentence raises the fact that models have been applied in the NW Med and have predicted a decrease of productivity for the ‘2000’ years which is in disagreement with observations performed at DYFAMED. However, from the following of the text, I understood that the authors spoke about models developed for the open ocean and not for the NW MED which has a different dynamics compared to the Med. The fact that models predictions show a decrease of biological productivity due to global warming in other areas does not necessarily mean that the models are wrong but it means that the NW Med may have a different response to increased atmospheric temperatures. Besides, it is not clear at which time scales the models predict a decrease of productivity. Is it for now? Or is it a tendency that could be expected in the future in response to increase atmospheric temperature? I do not like this sentence, it has to be much more discussed.

Introduction Lines 22-25, I would suggest to put this part in the section “Materials and methods” and to extend the part devoted to the description of the objectives of the paper highlighting the innovations.

Results and discussion Lines12-15 (p1382), the authors mentioned that the characteristics of the LIW have changed due to the EMT event. Could you be more specific? Did you observed these changes at DYFAMED? When and how? Line 14 p(1382): “One notes” instead of “On note” Figures 4 and 5, I would add regression lines on this curve in order to appraise the tendency. Besides, the authors choose specific depths for studying the LIW and MMDW waters, would you have the same tendency at others depths? Why not to use averaged values for the different layers (LIW: 200-600m, WMDW: 1600-2000m)? The curves are shown in Figure 5, are the tendency similar? Lines 19-27 (p 1383): The authors analysed the salinity increase observed in 2002-2004. They explain it by reduced river discharges (no real comparison with discharges showing a tendency for this period) and by the EMT event (why only in 2002-2004? Are there evidence that EMT affected the NW Med during these particular years and not before?). Lines 27-30 (p1384): The authors mentioned that the winter 2004/2005 created optimal conditions for the generation of deep convection in winter 2005-2006 by decreasing the density gradient between surface and LIW waters, however in Figure 4a, we can not see a change of density of 2004/2005 compared to past years. Pages 1385 and 1386, the authors explained the deep convection event that occurred in win-

BGD

7, C374–C379, 2010

ter 2005/2006: 1) it would be necessary to add the wind stress data (available at the meteo station of DYFAMED) in order to stress the importance of the winds on the hydrophysical vertical properties and maybe atmospheric temperature (the authors spoke about particular winter atmospheric conditions for 2003-2006 in the line 24 p 1386 but with no reference, please illustrate) 2) you show river discharges from one river, where does it come from (real data? Model results) besides this is only shown for one river but as far as I know there are other rivers that affected the DYFAMED station 3) again, please add a regression line because for the river discharges for instance it is not clear that it is lower after 2000 than before (on the contrary). Lines 16-19 p 1386: the authors mentioned that the amount of rain collected at the two stations "Nice" and 'Cape Ferrat" can trace the global input of freshwater (rivers and rains) to the Med Sea" What about the influence of the Rhone rivers for instance? Line 19 p 1386: The authors said that they showed that the rain amounts at Nice airport and flow of the Ligurian river Roya were correlated. How was computed the discharge? From precipitation? Line 26, p 26: I would replace this drastic change by drastic convection.

I think we have to be very careful when deriving general conclusions about the evolution during these recent years of meteorological, hydrological conditions and water budget in the NW Med and attributing these changes to climate change. It is clear that there are long term tendencies for the evolution of temperature and salinity on the MED (confirmed by a lot of papers). However, what occurs in 2003-2006 can also be due to heat waves combined with severe winter conditions. It seems that this is the first time that such an event of very deep convection occurs in the Ligurian Sea(according to the authors at least during the period 1995-2007, that is why I would suggest to extend the period of analysis from 1991 and even before if possible), is it really the case or is it due to the high frequency sampling at DYFAMED that allows to capture these types of events? If the frequency of such events increases in the future, we may speak about global change.

Page 1387, line 18, please specify where is the MEDOC area. P1388, lines 1-10: we are told that the amount of nitrate in the upper layer depends on the intensity of mixing. However, if we look at the nutrients data of January (not shown) the differences are not so marked. For instance, in Figure 8b the authors compared the nitrate profiles of early February 2005 (7th) and 2006 (2nd) with the profile of March 20th 2002 and conclude that the stronger mixing in 2005 and 2006 leads to increased nutrients content in the surface layer. However, if we look at the nitrate profile of the 20th January 2002 (not shown), we also see high nitrate concentrations > 5μM. The same for January 2000 and 1995. In fact, during years of intense convection the bloom maybe delayed and the consumption of nutrients too. In March, you look at the nitrate profiles after the bloom. Page 1389, lines 1-5, this section should go to the "Material and Methods" Page 1389, lines 19-20: The authors said " The link between high winter MLD and integrated fucoxanthin was even tighter than with chlorophyll a". Once again this higher correlation should be quantified computing for instance the correlation coefficient between the depth of the MLD and the vertically integrated fucoxanthin at a given period. The same with chlorophyll. A correlation could also be computed between the amount of nutrients in the surface layer and the vertically integrated fucoxanthin. Page 1390, lines 6-8, we are told that the interannual variability of the annual phytoplankton biomass is mainly governed by the variability of the spring bloom: higher spring blooms leading to higher annual biomass. However, looking at Figure 9, we have much lower annual biomass in 2005 and 2006 compared to 1999, 2001, 2003 although this is in 2006 that we have the highest spring bloom. Once again, the correlation should be quantified and not only deduced from looking at figures. Page 1390, lines 15-24: please compute regression coefficients for estimating tendencies in the total chlorophyll, pico-, nano-, and microphytoplankton and also for the percentages. I would also add the whole data set from 1991. I do not agree that the contribution from picophytoplankton to the total phytoplankton biomass decreases from 1995-2007. For instance, in 1995, 1996, 1997 it is about $\sim$10% while in the 2000's it is more than 20%. That is why I suggest to use regression coefficients in order to really estimate the tendencies. Page 130, line 22-23: we are told that " the increase of microphytoplankton in recent years was

BGD

7, C374–C379, 2010

apparently linked to the higher frequency of intense winter convergences" What do you mean by recent years? In 1999 and 1996 you have a much higher contribution of the microphytoplankton than during 2000-2005. What do you mean by "the higher frequency of winter convergence"? do you speak about the particular event of 2005-2006? Figure 9, legend replace "annal pico" by "annual pico"

Interactive comment on Biogeosciences Discuss., 7, 1377, 2010.

BGD

7, C374–C379, 2010

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