Biogeosciences Discuss., 7, C937–C949, 2010
www.biogeosciences-discuss.net/7/C937/2010/

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

J. C. Marty and J. Chiavérini

marty@obs-vlfr.fr

Received and published: 11 May 2010

Response to referee comments.

Response to referee #1

Referee: The manuscript describes hydrological – and subsequent bio-geochemical – changes in the Ligurian Sea over period 1995-2007 based on the DYFAMED station data. The paper is rather straightforward, well presented and written. I recommend publication after minor revision. Response: We would like to thank the reviewer for his/her valuable comments which helped us to improve our manuscript.

Referee: Minor comment: - The paper presents a thorough analysis of hydrography data and precipitation data but falls somewhat short on advection issues and atmospheric fluxes. The role of air-sea fluxes (e.g. from NCEP) could be investigated further to better support the conclusions of the paper by evidencing the relative role of T and S. Section 3.4 will require some update. I believe there are other moorings in the Ligurian (CNR ODAS buoy and Southern Ligurian next to the Corsican channel). Some comparison e.g. on meteorological and hydrographic data could be useful Response: Concerning the role of air-sea fluxes by using NCEP data: Smith et al. (2008) have already investigated the air sea flux data from NCEP/NCAR in the Ligurian Sea in winter 2004-2005 and winter 2005-2006 together with data from Argo floats in the occurrence of the intense winter mixing. We will discuss this aspect with reference to the paper of Smith et al. 2008 in the final version in section 3.4. Concerning additional meteorological data: We have added a new figure with data from the meteorological "Cote d'Azur" buoy located on the DYFAMED site since 1999. Air temperature and wind speed records are presented and discussed in the text.

Referee: - By any chance, is there any impact on Redfield ratios following these major hydrodynamic changes? Response: Unfortunately we were not able to detect changes in Redfield ratios.

Referee: - These changes could have an impact on the steric level. A rapid estimate (e.g. 1995-2005 against 006) could be a nice 'added value' and lead to subsequent useful update work (e.g. cf Tsimplis et al JGR, 2004; Tsimplis & Rixen, GRL 2002). Response: We agree with the referee that hydrological changes observed could have an impact on the steric level. This suggestion has been added in the final version with reference to the work of Tsimplis et al. JGR 2004 and Tsimplis and Rixen GRL 2002.

All editorial changes proposed by the referee have been corrected in the final version. Many thanks for the careful reading.

Response to referee #2

Referee: General Comments This is a very interesting study of the hydrological changes and their impact on phytoplankton biomass production in the Ligurian Sea based on the DYFAMED station data. The paper is well organized and it presents the type of interdisciplinary study that is consistent with the scope of the journal. Furthermore a well written and in depth discussion of the data analysis results is provided. I think that the paper is suitable for publication in this journal after minor revision. Response: We would like to thank the reviewer for his/her valuable comments which helped us to improve this manuscript.

Referee: Specific comments

a) The authors use freshwater input data in the Ligurian Sea such as precipitation and River Roya runoff in order to link surface salinity changes and winter convection intensity. However, the River Roya annual discharge seems to be quite small as compared to Evaporation-Precipitation fields to control surface salinity in the area. Furthermore heat fluxes could also be investigated during the considered period as a key parameter regulating winter convection. Therefore in order to further strengthen the paper the authors could use available evaporation-precipitation fields and heat fluxes variations derived from atmospheric databases (such as ECMWF, ERA40) to find links between air-sea fluxes variations and the observed salinity and temperature variations in the DYFAMED station during the 1995-2007 period. Response: The River Roya runoff was reported in our paper solely in order to indicate that precipitation and river runoff were coupled and that interannual variations of precipitation could be used to trace the intensity of all freshwater inputs to the Ligurian Sea. This was by evidence badly expressed (cf also referee 3). In the final paper we have changed the figure. We have deleted the river Roya data since they appear to be confusing. And we have redrawn a new figure with meteorological data: precipitations at Nice airport (on a monthly basis) ; wind intensity and air temperature at DYFAMED buoy. The discussion has been accordingly modified. Concerning NCEP/NCAR data, Smith et al. (2008) have already investigated the air sea flux data from NCEP/NCAR in the Ligurian Sea in winter 2004-2005 and

winter 2005-2006 together with data from Argo floats in the occurrence of the intense winter mixing. We will discuss this aspect with reference to the paper of Smith et al. 2008 in the final version in section 3.4.

Referee: b) Figure 9 shows that during winter 2006 the large nitrate concentration increase in the surface layer (induced by the intense mixing) resulted in a large increase of annual mean microphytoplankton biomass whilst, in contrast, pico- and nanophytoplankton biomasses remained in lower levels than in previous years when less intense winter mixing occurred. The authors should comment on the different response of the three phytoplankton size categories to winter mixing intensity. Response: We agree with the referee that the response of phytoplankton size classes to increase of nutrients during years of intense winter mixing was not sufficiently discussed. We have rewritten the discussion and included some regression curves also in response to referee 3 comments.

All the technical corrections proposed by the referee have been taken in account.

Response to referee#3

Referee: 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 impact 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.

Response: We first thank the referee for the useful comments. We agree with the referee that the paper was generally descriptive. In response to the comments of the referee, we have included, as suggested, computed regression lines on graphics and some correlation coefficients (detailed below). One suggestion of the referee is to use the whole dataset of DYFAMED time series (since 1991). This was our initial aim but, there is a larger uncertainty of hydrological data before 1995 since we used, at the beginning of the time series, many CTD profilers from various origins and it is only since 1995 that we get the "DYFAMED" Seabird (SBE 911 plus) CTD. That is why the 1995-2007 period was used because of the more homogeneous series. Nevertheless we understand that the referee should have preferred the whole data set. The tendencies described here are the same when we use the entire dataset and most of data from 1991 to 1999 were published in the DSR II ("Studies at the DYFAMED (French JGOFS) time series Station, N.W. Mediterranean Sea") 49, 11 in 2002. Concerning the meteorological conditions during the survey we added information issued from the "Cote d'Azur" meteorological buoy which is located on the DYFAMED time-series area and operational since 1999.

Response to detailed comments: Referee: 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). Response: As suggested, we have indicated the depth of increased T and T in 2006. We have detailed the biogeochemical characteristics (nitrate and integrated

pigments).

Referee: Long term evolution of surface salinity not shown. Response: The long term evolution of surface salinity is presented in Fig. 5 for the 0-200 m layer.

Referee: 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. Response: The increase of surface salinity since 2002 is one of the important points of the paper and we suggest that it is the key factor for the 2006 event. This increase of surface salinity is linked both to decreasing inputs of freshwater due to successive drought years 2003 2004 2005 (as illustrated by rain inputs) and mixing with LIW in winter 2004-2005. This is more clearly apparent on fig. 5 where we added the regression lines. We see that the salinity of surface water is increasing since 2002 whereas the LIW salinity is stable until 2005 and decreases in 2005 and 2006 by the winter mixing events.

Referee: 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). Response: We agree that the sentence "frequency of extreme events in recent years" is vague. We changed here and along the text "recent years" to "from 2003". Nevertheless, even if 1999 was a year of high mixing the regression line for the MLD since 1995 is increasing (cf. fig. 7 where we added the regression line), and there was no mixing event recorded during the first years of the DYFAMED time series (since 1991; Marty et al. 2002), nor during FRONTAL records in the preceding years. There where no report of direct observation of intense winter mixing event in the Ligurian Sea before this study. The principal area of WMDW formation through intense winter mixing is the MEDOC area (Gulf of Lions) due to the major impact of Mistral wind in this area.

Referee: 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. Response: You are right; we have deleted the end of the sentence.

Referee: 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. Response: OK This has been done.

Referee: 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? Response: The EMT event affected the eastern med sea in the late '80's (Lascaratos et al. 1999) and the max salinity of LIW in 2003-2004 could be the signal of propagation of EMT in the Ligurian Sea (Schroeder et al. 2006). This explanation has been added.

Referee: Line 14 p(1382): "One notes" instead of "On note" Response: OK Changed.

Referee: 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? Response: Regression lines have been added. 400m for LIW and 2000m for WMDW are the depth usually documented in the literature. The tendencies are similar for the average values for LIW (200-600m) and for 1600-2000 m fro WMDW. Moreover the tendencies remain the same with the cumulated salt and heat content for the whole layers.

Referee: 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?). Response: The paragraph was effectively confusing. We have rewritten it. At this stage the relation between increase salinity and reduced river discharge is for the Eastern Mediterranean Sea and not from our results. At DYFAMED, the increase of LIW salt content in 2002 2004 has been considered has the signal of EMT propagation by Schroeder et al. 2006.

Referee: 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. Response: The best illustration is furnished by Fig. 5b: the winter maximum of density of the surface waters is regularly increasing from 2002 to 2006 (5/Apr/2002: 28.92; 8/Apr./2003: 28.95; 31/March/2004: 29.02; 3/Feb./2005: 29.07; 7/March/2006: 29.10) whereas the density of intermediate water is almost stable (29.07). We changed the text for a better reference to this figure.

Referee: Pages1385 and 1386, the authors explained the deep convection event that occurred in winter 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) Response: We introduced in the paper the wind intensity and the air temperature data from the Meteorological Buoy 'Côte d'Azur' which is situated in the DYFAMED area since 1999.

Referee: 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. Response: The aim of this Figure for river Roya (real data) was, by evidence, not to monitor the discharge of rivers influencing DYFAMED site. The objective is to indicate that the precipitation record in Nice parallels the river input and thus that we can argue on decreasing freshwater inputs to the Ligurian Sea. In the new Figure we have indicated air T, and wind intensity from the buoy and precipitations recorded at nice airport monthly accumulations.

Referee: 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). Response: We have deleted the river Roya data since they appear to be confusing. For information the regression line indicated nevertheless a decreasing of flow even if we delete the year 95 with high flow.

Referee: 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? Response: The text was effectively confusing. We only wanted to estimate, by showing real data, inter annual variations of freshwater inputs to the Ligurian Sea. The discharge of rivers was not computed (see above). The text has been changed. The reference to inter annual variations of river flow was deleted. A discussion has been added on meteorological aspects.

Referee: Line 26, p 26: I would replace this drastic change by drastic convection. OK done. 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. Response: The question raised by the referee about the high frequency sampling at DYFAMED allowing the capture of drastic events is interesting. An indirect response is that estimation of the T rise in WMDW (eg. from Bethoux and Gentili 1999) since 1960 conclude to a regular increase from a relatively well distributed measurements. An event like 2005/2006 should have introduced a breaking of slope not observed. The data of DYFAMED since 1991 give the same indication since winter MLD was never recorded below 200 m (Marty et al. 2002).

Referee: Page 1387, line 18, please specify where is the MEDOC area. Response: Done. (The open sea MEDOC area is centred on 42°N and 5°E as the principal area of dense water formation in the Gulf of Lions.)

Referee: 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. Response: In the Fig. 8a the "winter" nitrate profiles for each year have been selected for most of them in February. Nevertheless owing to some sampling missing in our near monthly time-series some years were represented by profiles acquired in March. In any cases the surface layer contents were higher during years of strong mixing. In Fig. 8b we want to focus on 2005 and 2006 profiles. We agree that the reference nitrate profile used (20th March 2002) is not the best since it could have been influenced by spring bloom. Since a reference profile for years with weak winter mixing is not of use for this comparison we delete the March 2002 profile. (The January 2002 profile presents some values over 5μM but questionable variations in concentrations were also reported for the surface layer, and in any case they are below the values recorded in 2005 and 2006).

Referee: Page 1389, lines 1-5, this section should go to the "Material and Methods" Response: OK Done.

Referee: 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. Response: A correlation between MLD and Chlorophyll a and MLD and fucoxanthin was computed as suggested by the referee. A new Fig. and Table is included in the text and discussed.

Reeferee: 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. Response: Looking at figure 9 we can say that 1999, 2003, 2004, 2005 and 2006 have annual biomass > or = 500 mg m-2. These years are also the years were MLD is >200 m. The only exceptions are for 2000 (High MLD and low Annual biomass) and 2001 (low MLD high biomass but no fucoxanthin). We agree with referee that this co occurrence is not a quantitative relation (in this respect 2006 should have a higher annual biomass) but we are not surprised since the data of annual biomass are computed from monthly data with some gaps, and since the monthly frequency is too low to be certain having captured both the maximum MLD and the maximum phytoplankton biomass. We have rewritten part of text to precise the limitations and used the regression of increasing MLD and increasing phytoplankton biomass to illustrate.

Referee: 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 _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. Response: Regression coefficients have been computed and the discussion consolidated. The contribution of picophytoplankton is effectively increasing. What we wanted to express is that the increase of biomass which was attributed to pico and nano phytoplankton in our preceding paper (Marty et al. 2002) is for the complementary series appears now as also linked to an increase of microphytoplankton due to successive years with high winter mixing. The page 1390 has been rewritted.

Referee: Page 130, line 22-23: we are told that " the increase of microphytoplankton in recent years was 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? Response: The utilisation of recent year has been avoided in the corrected text. The increase of MLD during the survey has been illustrated by the regression line on the Fig.7. The last part of the text has been rewritten to better explain the link that we expose between the increasing of the intensity of winter mixing and the increase of phytoplankton biomass.

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