Ocean Sci. Discuss., 4, S158–S161, 2007
www.ocean-sci-discuss.net/4/S158/2007/

Ocean Science Discussions

Interactive comment on "Seasonal variability of phytoplankton fluorescence inrelation to the Straits of Messina (Sicily) tidal upwelling" *by* F. Azzaro et al.

Anonymous Referee #2

Received and published: 17 May 2007

General comments:

In this manuscript of Azzaro et al., a description of the seasonal variability of surface characteristics of the Straits of Messina is presented according to a data set acquired during several research cruises made from 1993 to 1995. The discussion of hydrological and biogeochemical patterns are made according to tidal stage and for each considered season.

While the subject addressed is of interest, I find some weakness that should be considered by authors before the manuscript is accepted for publication. My main concern is about the validity of the conclusions obtained has there are many aspects not consid-

erer by the authors (see bellow) that should be addressed to understand the behaviour of the surface chlorophyll in the area.

Moreover, there are severe problems with the language which is confuse and, sometimes, too wordy. I strongly recommend a native English to revise the manuscript.

Specific comments:

The introduction to the work is vague and somehow confusing. The reader has the feeling of not getting enough information about the study area, its dynamic and main forcing at work. I'm quite used to work on Straits areas and, honestly, find quite difficult understanding the description made of this particular area. In my opinion the authors should rewrite all the introduction section, shortening it and reorganising ideas.

In the Material and Method section it is said that the cruises were made according to tidal prediction to sample in the tidal slacks (after high water and after low water). However, it is said that each sampling lasted for 3 hours. How long is the tidal slack in that zone? Because usually in the medium north-Atlantic the slack only last around 45 minutes. My question is whether the situation described in the work correspond to a no-tidal-current situation or if there could be a distortion of the surface patterns because of a re-activation of tidal movement before the end of the sampling. Maybe this point could be addressed looking some satellite images of SST or chlorophyll for the sampling dates. The authors referred to a work by Azzaro et al (2001) to state that the sampling strategy has been already validated with satellite imagery. However showing the corresponding image for each sampling presented should add value to the present work.

I have a number of questions on the results section:

Page 423: in the relationship reported between termohaline parameters and chlorophyll the highest r2 is 0.0529 corresponding to the relationship with salinity (r=0.23). Although the reported p value indicate a significant correlation ($p<0.01$) the extremely

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low values of r2 coefficient are indicating that, at a practical level, there are no correlation between chlorophyll and the other variables. The author should clearly state this latter point and discuss it in the corresponding section. Also, a table grouping all the values of r2 and p should be very clarify for this section of the manuscript.

Page 424: the entire paragraph regarding figures 7 and 8 is extremely confused and difficult to understand. The authors must try to rewrite all this sub-section more clearly.

Page 425, final paragraph of results: the fact of the lack of correlation between chlorophyll and salinity or temperature (see comment above) is clearly indicating that the surface distribution of chlorophyll is not controlled by the upwelling phenomena as the authors seem to state. In my opinion the patterns of this surface chlorophyll should be discussed more as a consequence of advection from coastal areas that as a result of a growing driven by nutrient injection in the surface layer. In this sense knowing the wind field patterns in each sampling should be clarify so I recommend to explore the meteorological conditions in the area.

Finally, regarding the conclusions: this section is, in general, too concise. The overall sensation is of un-conclusion of the work. There are many aspects which could influence the surface chlorophyll dynamic that are not commented or even named (such as seasonal cycle of PAR, photoadaptation or photo-inhibition, etc..). For the sake of the work I'll put much effort in this last section.

Other comments:

Table 1: the SD of data in LW and HW slack seems to be too high compared with mean data to determine if there are differences between both situations. A statistical analysis of mean (such as t-student) should be made in order to elucidate if there are real differences.

Figure 2: I miss an x-axis scale to fully interpret this diagram.

Technical corrections:

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At its present state there are so many typing errors to be listed here. Please revise language carefully.

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