Response to the referees' comments :

The first referee is fully satisfied with this second version of the text and acknowledges the validation of satellite turbidity. Referee1: "It is, to my knowledge, the first time that satellite derived SPM has been validated against turbidity in such extent"

The second referee (referee3) is much more critical, particularly on the statistical validation of Chl, and SPM estimated by satellite. Very concerned by the processing of the satellite reflectance, the second referee asks also much more information and details on the procedures. In fact the main issues addressed in this paper proposed to the special Issue of ECOOP (and therefore to a community involved in operational oceanography and not remote-sensing) have to be clearer. We all know that it is not recommended to put forward too many ideas in an article. In this manuscript, the main idea is that we can handle together Chl-a, SPM and Turbidity data, obtained in situ from conventional networks or from satellite sensors (MODIS and MERIS), to assess the annual cycles of these variables in coastal waters.

What is new is the evaluation of Turbidity, derived from Chl-a, and non-algal SPM, whatever the source, satellite or in-situ (NTU or FNU). What is also new is the proposition of monthly climatologies (36 maps in annex) of these three parameters over western Europe. The introduction has to be reshaped to put forwards this issue. In fact, even the title of this paper was ambiguous, leading to misunderstanding, and that is why I propose a new title closer to the reality of the work and ideas described in this text:

Annual cycles of Chlorophyll-a, non-algal Suspended Particulate Matter and Turbidity observed from space and in-situ in coastal waters

Therefore, in our response to referee3's comments, we'll consider that the Chl-a and SPM procedures have been already described in previous papers and that the chapters dedicated to the satellite processing will only resume the procedures developed in the following articles :

-Int. Journal of Remote Sensing 2002 for Chl-a
-Remote Sensing of Environment 2005 for Non-algal SPM

After doing so, we enhance in this article the fact that it is possible to obtain Turbidity from Chlorophyll and non-algal SPM (satellite or in-situ) for providing consistent annual cycles, and climatologies, of these parameters in coastal waters.

Referee 3's remarks and response:

« Section 2.1 page 959: The author says that the in situ dataset used in this paper are provided by the SRN and RHLN network plus additional measurements from short scientific cruises. The information on the measurements provided by these networks is not clearly presented in the paper. It is no clear if the 27 stations provides the same type of measurements, have the same sampling strategy, ect. Please provide for each station information on type of measurements acquired, frequency of the samples and number of measurements available, distance from the coast, ect. I believe that a table providing these information for each station can be very useful to quantify in situ dataset used in the paper and to qualify the results.»
It is true that this information is missing in the text. A table will be added to describe the main characteristics of the different stations. I am contacting the persons in charge of these networks to have more details on the measurements. However, we have to keep in mind that these networks are mainly regional and that they converge slowly into effective national networks developing new intercalibration procedures, particularly required for the application of the european directives on the water quality.

« Section 2.1 page 959: The author says that in order to use the Cabourg data he built the satellite matchup using a pixel 3.5 Km far from this station. Since, in general the Chlorophyll and SPM near

The Cabourg point is important as it is in the vicinity of the plume of the Seine River. That is why we wanted to add it to our satellite-in-situ data set. However, it is not observed by satellite (land flagging) and a shift is required for comparisons. Prior to validating this station and accepting the shift, we have carried out several transects northern from the station (during RHLN sampling). We have not observed any significant gradient on that distance. In fact, the main Chlorophyll (and SPM) gradient is not crosshore at this station (South-North) as there is a strong influence of the Seine plume, advecting enriched waters westwards (waters also subject to tide mixing and advection), therefore along the shore (West-East). Due to all these effects there is no clear gradient in the turbidity or Chl features in the vicinity of Cabourg.

Section 2.2.2 page 961: The author affirms that the method used by this paper is empirical as the OC4.

I wanted to say that the method is empirical and derived from OC4. I agree with the remark of the referee that OC4 is not a remote-sensing algorithm (although largely applied as it is) and will change the sentence.

Section 2.2.3 and Section 2.2.4
These two sections present the method to compute the turbidity from satellite Rrs.The two sections are poorly written. At the present the description of method starts in section 2.2.3 then, start again in section 2.2.4 and continue with additional information in 3.1.3. I strongly suggest to merge these two sections in one section introducing also the material of section 3.1.3 relative to the retrieval of the turbidity from satellite. The new section should present the method used to compute turbidity from satellite data in details, providing all the required information. In particular, the coefficients used for converting chl and NaP in absorption and backscattering coefficients must be provided in the paper. The variable R*(550) should be defined. The dataset used to compute alfa and beta in eq (2) should be described in details, giving information on how it was built (e.g matchup criteria used to build the dataset, station used, number of data available, range of data, ect). A figure showing the R*(550) compute used by insitu measurements versus Rrs(550) with the fit line should be introduced in the paper. The values of the alfa and beta coefficients should be provided in order to allow other researchers to apply the method. Finally the problem in operational application of method should be presented at the end of the section.

There, we arrive to the points that I mentioned in the first paragraph of this response. This method is not new and has already been published in details (RSE, 2005). Paragraph 2.2.3 resumes this work already published. There is only an extension to MERIS. There are a lot of coefficients involved in the equations (they are provided for Channel 550 SeaWiFS in RSE, 2005), with unequal weights in the equation. Providing these coefficients would weigh too much this article (coefficients related SPM(550), SPM(670), for MODIS and MERIS). I also prefer to keep separated the SPM (2.2.3) and Turbidity (2.2.4) chapters to take into account that the SPM method is not new.
Referee3 notes *The values of the alfa and beta coefficients should be provided in order to allow other researchers to apply the method* I would be happy to give the routine (in IDL) calculating SPM to anybody asking for it, as I do for the LUTs used for retrieving Chl from MERIS and MODIS reflectance. It will be much more useful to have a direct communication with potential users of this method as we periodically recalculate the alfa and beta parameters following the modification of the L1-L2 procedures by the Agencies. This happened recently with MODIS (release of new L2 reflectance data by NASA) and it will happen probably in the next months for MERIS (new ESA MERIS processor). Therefore I will add at the end of the text, after the acknowledgement the following sentence:

"The author will be happy to provide the LUTs and routines to derive Chl and SPM from MODIS and MERIS reflectances to anybody interested to test the method in another area. The LUTs and

coefficients of the SPM equations (not provided in this article) are likely to evolve following the major modifications of the L2 production chains by the Space Agencies."

Section 3.1.1 page 964: Please provide the details on the method used to build be matchup. The information on bias a r2 are not sufficient to quantify the error. Please add information on other statistical standard parameters in general provide to validate OC data (eg. root mean square error, Absolute Percent Difference, relative percent difference).
Section 3.1.1 page 964: the author affirms that the method he used to validate the data, based on statistical properties of the annual cycle is more sophisticated and more appropriated then the use of match-up. Why? This should be discussed and clarify.

This is true. It is not a validation of the method itself but a comparison of the annual cycles. In most of the cases, the satellite-derived cycles are similar to those derived from in-situ measurements but they can be different. The annual cycles of the satellite-derived and in-situ SPM at Roscoff are not similar (probably due to some in-situ artefacts with high values in summer). Conversely, the satellite turbidity in the Mediterranean Sea seems to be overestimated. The text (title, abstract, introduction, chapters heads) will be rewritten to better fit the illustrations, figures and results (monthly averaged maps), giving weight to the annual cycles in the discusssion.

Example of modified chapter, here 3.1.1 which was "validation" in the first version of the text :

3.1.1 The annual cycle of Chlorophyll-a observed from space and in-situ

To have a quick overview of the overall relationship between satellite-derived and in-situ data, a scatterplot of the satellite versus observed Chl at the selected stations is shown on Figure 2. The match-up is considered for satellite and in-situ observations observed at the same pixel location and the same day. The coefficient r^2 obtained on the log-transformed Chl data is equal to 0.66. This r^2 coefficient appears a little bit lower than the value of 0.7 obtained in the processing of SeaWiFS data in Gohin et al. (2002). This cannot be interpreted as a proof of a lower quality of the MODIS products compared to SeaWiFS as the coastal data set considered in this study is much more heterogeneous than that used in the 2002 publication. In the 2002 publication, the data set was obtained from cruises on the continental shelf. There is also a clear alteration of the quality of the retrievals approaching the coast due to scattering of the photons by land and failures in the atmospheric corrections which may affect our coastal set of satellite data (Gohin et al., 2008).
To assess the capability of the satellite method to provide monthly climatologies of environmental variables so useful for operational oceanography, comparisons of the seasonal cycles of Chl have been carried out locally at the selected stations.

Fig 1. The name of the locations are very hard to read, please can you increase the characters or substitute with symbols.
This figure is really of high quality and it is possible to zoom to have a more accurate view on the area where the names of the stations are too close (northern transects)

Fig 3, Fig 4 Fig 5. Increase the size of the labels. Eliminate the information on the statistical results and put them in a table.

The numerous illustrations on these figures are of poor quality (as Figures 7, 11, 12, 13).
The size of the labels will be increased and the quality improved but I would like to keep information on the statistics as it is. These indications make easier the comparisons of the different cycles (as for the points of the Dunkerque and Boulogne cross-shore transects). It seems to me better to have the values on the graphs when comparing a station to another one as the vertical scales are in logarithm.

Thanks to referee3, the title and the main issues of this paper have been made clearer and more in adequation with the results shown under the shapes of annual cycles and monthly climatologies.