We would like to gratefully thank the anonymous referee for your very positive and constructive comments. Almost all suggestions have been accepted as described below.

RC = Referee's comments; AR = Authors' Response

General Comments

RC:

Page 4992: Equation 2: Why do you use a high order polynomial equation for converting >10μm chl-*a* to >5μm chl-*a*. Does this equation give a significantly better fit than a linear equation? According to Equation 2, above 1.5 mg m^{-3} of >10μm chl-*a*, the >5μm chl-*a* begins to increase sharply as a function of >10μm chl-*a* (is this realistic, or a feature of the high order polynomial equation?). If you use this equation I would be careful to clearly state the range of >10μm chlorophyll-*a* it can be applied to (i.e. the range of data the model was fitted to), should others be inclined to use the equation.

AR:

We had made a mistake in Equation 2 which is conversion from the ratio Chl-$a_{>5\mu m}$/Chl-a_{total} to another ratio Chl-$a_{>10\mu m}$/Chl-a_{total}, not the chlorophyll concentration itself. It is miss description. Comparison of Chl-$a_{>5\mu m}$/Chl-a_{total} and Chl-$a_{>10\mu m}$/Chl-a_{total} is shown in Figure attached (figure A). Contribution of Chl-$a_{>10\mu m}$ to Chl-$a_{>5\mu m}$ tend to be larger when its value is high, but in the middle scale (Chl-$a_{>10\mu m}$/ Chl-a_{total} is around 0.3) contributions of Chl-*a* attributed between 5 and 10 μm cells become larger. To represent such relationship we believe 3rd degree polynomial function is better than linear regression.

RC:

Page 4998: Equation 8: While I find this equation elegant (I like the way F_L is constrained to vary between 0-100%), as it is non-linear, I am concerned as to whether such a model should be applied directly to monthly averages of reflectance data from MODIS. Should the model first be applied to daily values then averaged over the month? Will this cause any differences?

AR:

We agree and understand that the calculation order will cause miss-derivation of F_L and Chl-*a*. There is good correlation between F_L and Chl-*a* created from monthly composited R_{rs} and those composited after calculated daily. But as you pointed out, monthly composited satellite product calculated by nonlinear function must be calculated daily first and composited monthly after that. So we recalculated Chl-*a* and F_L from daily R_{rs}.

RC:

Page 4998: Equation 8: It would be nice to provide some statistics which indicate that the performance of the model increases significantly when using both $a_{ph}(488)/a_{ph}(555)$ and as independent variables, and not just a single independent variable (e.g. $a_{ph}(488)/a_{ph}(555)$ or on its own?).

AR:

We can provide determination coefficient increased to 0.72 when $a_{ph}(443)/a_{ph}(667)$ and gamma are used as independent variables for the sigmoid function, and 0.71 on $a_{ph}(488)/a_{ph}(555)$ and gamma are used (r^2 values are 0.58, 0.52 and 0.60 for $a_{ph}(443)/a_{ph}(667)$, $a_{ph}(488)/a_{ph}(555)$ and gamma, respectively). Should we provide AIC or RMSE too?

RC:

Page 4998: Equation 8: Is it possible to provide error estimates (or confidence intervals) on the parameters provided in Table 4. This could be very useful information should one need to run sensitivity analysis, and for additional error estimates.

AR:

Thanks for your pointing it out. 95% confidence intervals have been provided in Table 4.

RC:

Page 5001: Line 16: The authors refer to the satellite model as capable of retrieving F_L independently from Chl-*a*. Whereas the model is fitted using in situ measurements of $a_{ph}(488)/a_{ph}(555)$ and γ, its application to satellite data is different. Firstly, the approach estimates using a blue-to-green reflectance band ratio (equation 6) yet the OC4L (equation 7) also uses a blue-to-green reflectance band ratio to derive chlorophyll-a. In fact the QAA, used to estimate $a_{ph}(488)/a_{ph}(555)$ from satellite, is also very dependent upon blue-to-green reflectance band ratios (see Lee et al. 2002 Tables 2 and 3). Therefore, it would be interesting to know exactly how independent the satellite estimates of $a_{ph}(488)/a_{ph}(555)$ and γ are from the OC4L chlorophyll-a. Can the authors provide some quantitative statistics on this? One approach could be to add to Figure 5 histograms of $a_{ph}(488)/a_{ph}(555)$ and γ, as such it may become clearer what is forcing the differences between F_L and Chl-*a*, is it coming from differences in $a_{ph}(488)/a_{ph}(555)$ or γ or the mathematical formulation of Equation 8? Note that a recent paper by Vantrepotte et al. (2011) showed that the global seasonal cycles for γ and Chl-*a* have a similar pattern, and highlighted inter-annual similarities between γ and Chl-*a* in the global ocean. Is this different in the Arctic regions?

AR:

We had committed a mistake that after the re-composite of monthly F_L and Chl-*a* from daily-calculated ones, there are significant difference between 2006 and 2007 for the all satellite products, F_L, SST and also in Chl-*a*. However we simply analyzed inter-annual and seasonal trend for F_L, Chl-*a*, $a_{ph}(488)/a_{ph}(555)$, γ and SST (Figure B and Figure C) in the same area as the manuscript (60–72°N,166–172°W). Data of 2007 were used to demonstrate the seasonal variability (Figure B) and inter-annual trend of august was investigated from 2003 to 2010 (Figure C).

There is highly significant correlation between seasonal variability of F_L and Chl-*a* (r=0.92). γ also showed very similar seasonal pattern to Chl-*a* like Vantrepotte et al. (2011) have shown (r=-0.90). However, there is relatively low correlation between inter-annual variability F_L and Chl-*a* (r=0.69) in August. Although F_L and $a_{\mathrm{ph}}(488)/a_{\mathrm{ph}}(555)$ exhibit gradual decrease from 2003 to 2010 (p-value < 0.05), Chl-*a* does not show such significant changes. $a_{\mathrm{ph}}(488)/a_{\mathrm{ph}}(555)$ is rather correlated with F_L than gamma, r=-0.92 and r=-0.38, respectively. It can be said that $a_{\mathrm{ph}}(488)/a_{\mathrm{ph}}(555)$ provides F_L to be semi-independent of Chl-*a* as a result of its calculation step of QAA, it would be coming from the subtraction of $a_{dg}(\lambda)$ from $a_t(\lambda)$ (Lee et al. 2002, table 3). In shortly, the differences of inter-annual trend in August between F_L and Chl-*a* is coming from the differences in $a_{ph}(488)/a_{ph}(555)$.

RC:

Page 5000: Line 4 (section 3.2 generally): Although perhaps beyond the scope of this paper, it would also be interesting to try different methods for determining $a_{ph}(488)/a_{ph}(555)$ and γ from satellite data, and use these as input to your model to see how this may influence your satellite retrievals of F_L. Although, the authors would need to be careful to use IOP models that do not assume a spectral shape for $a_{ph}(\lambda)$. This information could be very useful for mapping errors from satellite data.

AR:

We would like to thank the referee for your pointing out these problems. Several IOP models, GSM (Garver and Seigel, 1997, Maritorena et al., 2002), PML (Smyth et al., 2006) and Carder model (Carder et al., 1999, Carder et al., 2004) were tested. As you have mentioned, spectral shape of $a_{ph}(\lambda)$ in GSM and Carder model is fixed so that it cannot be used for SDM because $a_{ph}(488)/a_{ph}(555)$ will be constant. On the other hand, we could not retrieve any valid value of $a_{ph}(555)$ with default setting of PML model. Smyth et al. (2006) also showed that retrieval of $a_{ph}(555)$ is worse than other shorter wavelength. Thus, although not all of the IOP models proposed in the past have been tested, we would like to conclude that QAA is one of the best models to derive spectral shape of $a_{ph}(\lambda)$ as a concept of SDM.

Meanwhile some IOP models estimate γ empirically using blue-to-green reflectance ratio similar to our study did (e.g., Carder et al., 1999, Lee et al., 2009 (QAA-v5). On the other hand, Loisel et al. (2006) calculate γ by independently derived $b_{bp}(\lambda)$. Although theses models were tested for our data, all of them tend to underestimate γ value measured by VSF3P (Wetlabs, Inc) in this study. As a result, F_L will be overestimated. Therefore, we would like to recommend who use SDM to use Equation 6 for deriving γ as SDM input.

We would like to clarify the recommended IOP model to be used to derive

SDM parameters.

Technical Comments

RC:

Page 4986: Line 15-16: (also Page 4999: Line 11-12) Not sure what is meant by the following statement "A validation study demonstrated that the SDM successfully derived an F_L value of 69 % within an error range of ± 20 % for unknown data". Can the authors clarify what error statistical tests are being used (mean absolute error, standard error)? Can the authors clarify what is meant by "F_L value of 69 %"? Is the mean value for in situ F_L (Figure 3 x-axis) the same mean value for the modeled F_L (Figure 3 y-axis) i.e. 69 %?

AR:

In these sentences I would like to describe that 69% of unknown samples (modeled F_L) are fitted to +/-20% in situ F_L value range, that is, 69% of the plots in fig. 3 were in the dashed lines.

RC:

Page 4989: Line 1: Not all these approaches estimate the dominant distribution of PFTs, some (e.g. Uitz et al. 2006; Ciotti and Bricaud 2006; Mouw and Yoder, 2010 and Brewin et al. 2010) estimate the fractional contribution of PFTs to the total biomass. I would also suggest including the references Bracher et al. 2009, Kostadinov et al. 2009, Devred et al. 2011 and Hirata et al. 2011.

AR:

Thanks for your pointing it out. We have correctly described the references and added Bracher et al. 2009, Kostadinov et al. 2009, Devred et al. 2011 and Hirata et al. 2011 to the references.

RC:

Page 4989: Line 13-14: The IOPs do not always vary with the PFT composition in the water. Some PFTs can have similar optical signatures making them especially hard to discriminate using only optical measurements (e.g. some Harmful Algal Blooms are difficult to determine using only optical data as they have similar optical signatures to other non-harmful phytoplankton).

AR:

We have corrected the sentence as follows: "The use of the inherent optical properties (IOPs) (absorption and backscattering) of seawater is the principle method of estimating PFT composition optically because some PFT's dominance can change IOPs of the water."

RC:

Page 4989: Line 18: The structure of this sentence suggests that Morel and Prieur (1977) quantified relationships between IOPs and PFT composition in case 1 waters, whereas I think the authors are using this reference to refer to the case 1 bio-optical principle. If so, the sentence needs restructuring.

AR:

We have restructured the sentence as: "Several studies (e.g., Sathyendranath et al., 2004, Alvain et al., 2005, Mouw and Yoder et al., 2010) have suggested that quantifying the relationship between PFT composition and IOPs allows us to estimate their distribution, especially in Case 1 waters where phytoplankton particles and their related materials are major contributor of optical properties in the water (Morel and Prieur, 1977)."

RC:

Page 4989: Line 28: For clarity I would suggest removing "Then," at the beginning of the sentence and start the sentence with "Montes-Hugo et al." Also I would suggest including the reference Loisel et al. (2006), who also used the spectral slope of the backscattering as an index for small and large particles, though this was conducted globally, not specifically for the Western Arctic Peninsula region.

AR:

We agree. As the referee pointed out that there are several sentences where Loisel et al. (2006) should be referred in the manuscript.

RC:

Page 4990: Line 2: For clarity I would suggest removing the word “bloom” at the end of the sentence.

AR:

We agree and the word “bloom” has been removed.

RC:

Page 4990: Line 10: I would suggest changing “validated in the area. . ..” to “validated or tuned using data in the area. . ...”?

AR:

We agree and the sentence has been corrected as above.

RC:

Page 4990: Line 18: I would suggest changing the words “satellite observation. . ..” to “satellite data. . ...”?

AR:

We agree and the words “satellite observation” have been changed to ‘satellite data’.

RC:

Page 4991: Line 2-3: I would suggest removing the words “discuss and”

AR:

We agree and the words “discuss and” have been removed.

RC:

Page 4993: Line 4: I would suggest removing the words “as IOPs”

AR:

We agree and .the words “as IOPs” have been removed.

RC:

Page 4995: Line 11: The authors mention that data from turbid waters were omitted, using a ratio of $a_{NAP}(443)$ to $a_t(443)$. Are turbid waters also omitted from the satellite pixels?

AR:

No, they are not. We removed such turbid water data just for the SDM development. Such data screening can be done comparing $a_{dg}(443)$ value with $a_t(443)$ derived by QAA.

RC:

Page 5013: Figure 1: Green circles should be correctly referred to (the caption refers to purple circles for KH09-4?).

AR:

We had committed a mistake that green circles exhibit the MR09-03 cruise stations that have not been described. We have corrected the figure caption.

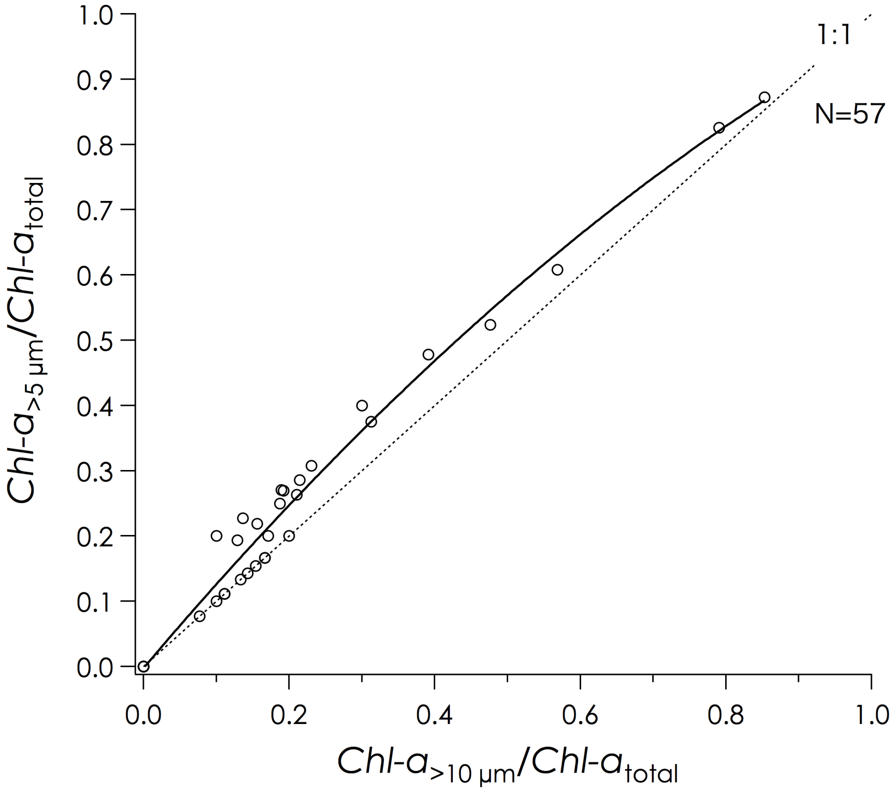

Figure A. Comparison between Chl-$a_{>5\mu m}$/Chl-a_{total} and Chl-$a_{>10\mu m}$/Chl-a_{total} for in situ size-fractionated Chl-*a*. Solid line indicates 3rd-degree polynomial function fitted to the data (equation 2) and dashed line indicates 1:1 line.

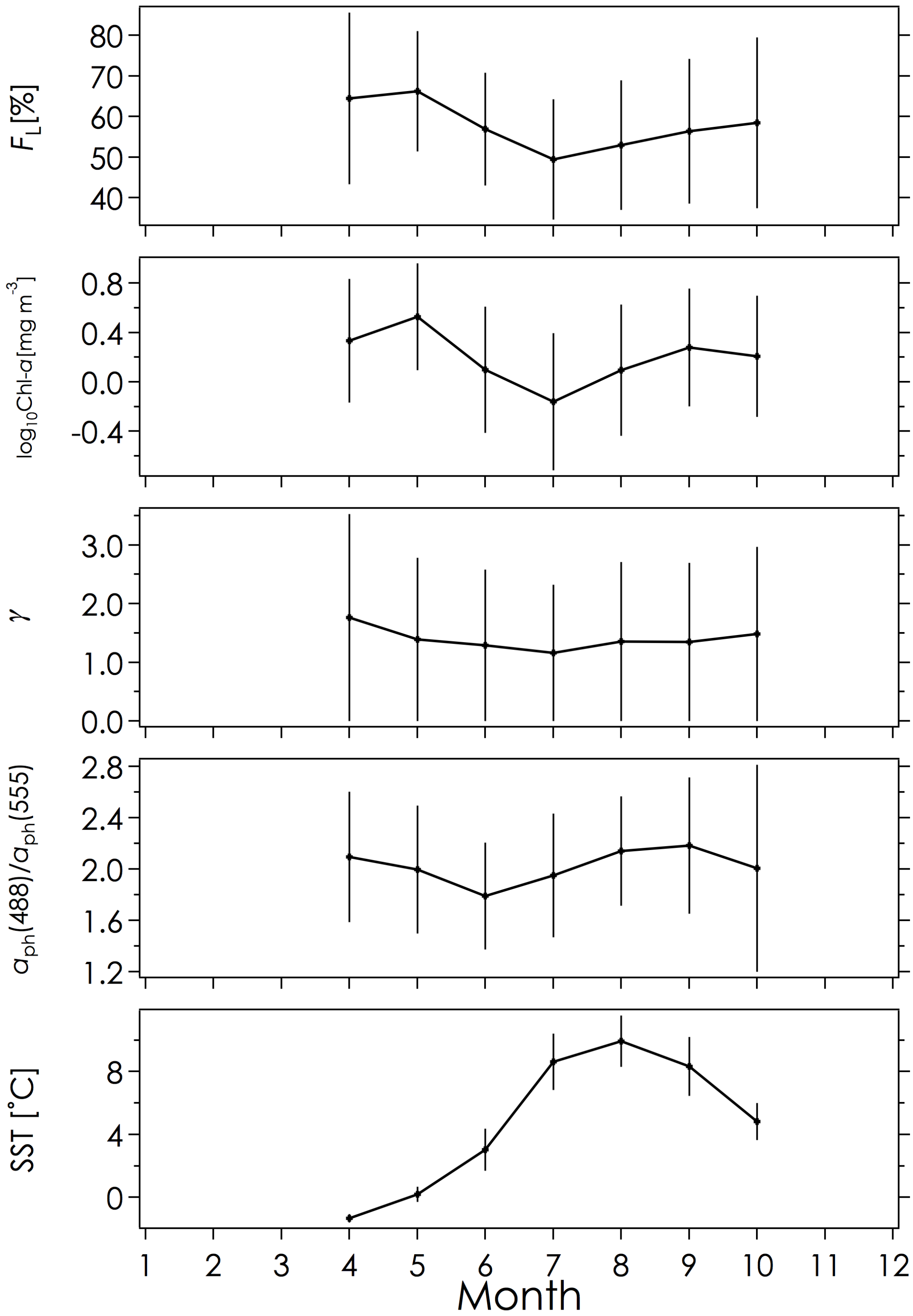

Figure B. Seasonal variability of F_L, Chl-a, γ, $a_{ph}(488)/a_{ph}(555)$ and SST in the box-averaged area (60–72˚N, 166–172˚W) in 2007. Error bars indicate the standard deviation for each month.

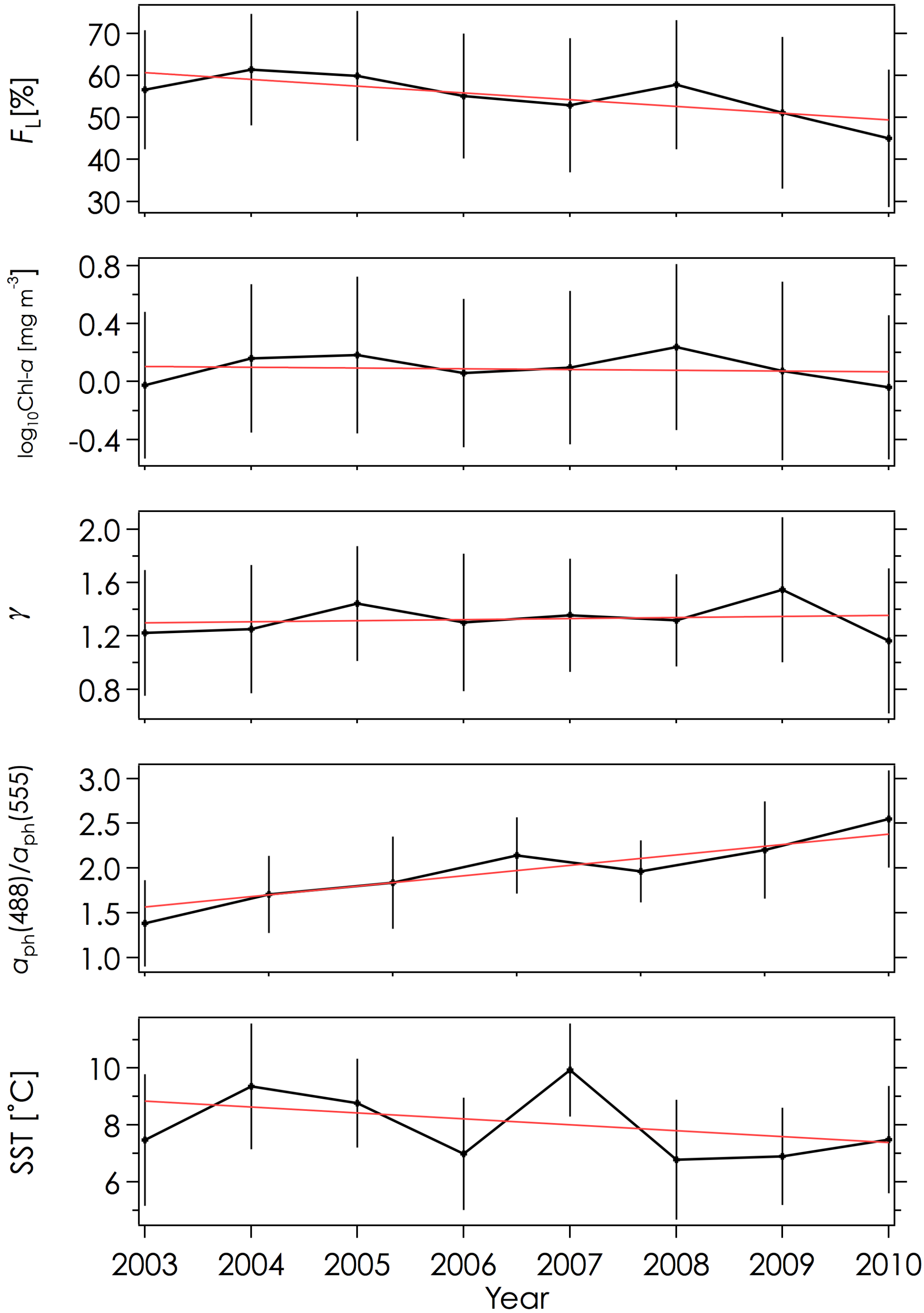

Figure C. Inter-annual variability of F_L, Chl-a, γ, $a_{ph}(488)/a_{ph}(555)$ and SST in the box-averaged area (60–72˚N, 166–172˚W) of August from 2003 to 2010. Error bars indicate the standard deviation for each year. Red solid lines indicate regression line for the time series. F_L (r = -0.78, p-value < 0.05) and $a_{ph}(488)/a_{ph}(555)$ (r = 0.93, p-value < 0.001) show significant decrease and increase along with the time series, respectively.