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Biogeosciences Discussions

Interactive comment on "Large variability in continental shelf production of phytoplankton carbon revealed by satellite" *by* B. F. Jönsson et al.

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Reviewer 1

Comment:

P8958, " ... MODIS Chl_a ... K_{490} ... " *Clearly the estimated PC quantity is highly dependent on these inputs, which are not always accurate from ocean color satellite measurements. It would be important and helpful to tell the readers how good these satellite products are for the Gulf of Maine.*

Response:

The MODIS theoretical basis document (Carder et al., 2003) describes an accuracy goal for Chl_a of 35% or less for Case 2 waters. Moore et al. (2009) used a class-based approach to characterize uncertainty in the MODIS Chlorophyll products (OC-3 and OC-4). Based on their analyses of the NASA NOMAD datasets, they determined error ranging from 16% for oligotrophic waters to 123% for high-absorption waters. The average error (in-situ versus observed) was 73%. According to the Moore et al. (2009) classification, the Gulf of Maine is dominated by optical classes 2-5 (Tim Moore, personal communication). Based on this work we expect errors in the Chl_a retrievals of 51-68%. The absolute error is somewhat less important in our work since we are differencing data over time (e.g. biases in the satellite retrievals attributable to CDOM absorption would not affect the differences in chlorophyll estimates to the degree it would affect individual chlorophyll estimates)

Based on limited data, the MODIS K_{490} product appears to be performing well in the Gulf of Maine. Show here (labeled figure 2) are results from a 2006 University of New Hampshire (UNH) AOP dataset versus MODIS K_{490} for near coastal waters. This plot was originally created for a NASA proposal. We will add the following paragraph to the text: "The OC-3 Chl_a has been evaluated in the Gulf of Maine by e.g. Moore et al. (2009), who determined errors ranging from 16% for oligotrophic waters to 123% for high-absorption waters. Based on this work we expect errors in the Chl_a retrievals of 51-68%. The absolute error is somewhat less important in our work since we are differencing data over time (e.g. biases in the satellite retrievals attributable to cdom absorption would not affect the differences in chlorophyll estimates to the degree it would affect individual chlorophyll estimates) Based on limited data, the MODIS K_{490} product appears to be performing well in the Gulf of Maine. "

Comment:

P8958, "... $z_e = log_e(0.01)/K_{490}$ ... " *This z_e is not the euphotic depth (see Kirk 1994 or Mobley 1994 for the definition of euphotic depth). Westberry et al (2008) also pointed out this error. You may use the z_e product generated by the OBPG. Kirk, J. T. O. (1994), Light & Photosynthesis in Aquatic Ecosystems, University Press, Cambridge. Westberry, T., M. J. Behrenfeld, D. A. Siegel, and E. Boss (2008), Carbon-based primary productivity modeling with vertically resolved photoacclimation, Global Biogeochemical Cycles, , doi : 10.1029/2007GB003078.*

Response:

We assume that the reviewer questions our use of K_{490} instead of $K_d(PAR)$, since the definition suggested by Kirk (1994) is otherwise the same as the one we are using: *"Making the assumption that $K_d(PAR)$ is approximately constant with depth, the value of z_{eu} is given by $4.6/K_d$"* (page 144, line 11-12). We are using the K_{490} product since it is available to us at 1 km resolution for the Gulf of Maine, and is a good proxy for $K_d(PAR)$. We have compared the difference between using $K_d(PAR)$ and K_{490} in our study and found the error to be relatively small. The problem with K_{490} in oligotrophic waters discussed in Westberry et al. (2008) has no relevance in the Gulf of Maine where the euphotic depths seldom are in the critical range. The operational K_{490} algorithm used by GSFC has also been improved to decrease this problem.
(http://oceancolor.gsfc.nasa.gov/REPROCESSING/SeaWiFS/R5.1/k490_update.html)

Comment:

P8962, ". . . Bay of Fundy . . . " Please mark this and other locations in your figures.

Response:

Locations will be marked in Figure 1, left panel

Comment:

P8965, ". . . bbp-based estimate of . . . POC is not reliable . . . " *I would suggest to compare your results with the POC estimates generated with the Stramski et al. (2008) model. Stramski, D., R. A. Reynolds, M. Babin, S. Kaczmarek, M. R. Lewis, R. Rottgers, A. Sciandra, M. Stramska, M. S. Twardowski, B. A. Franz, and H. Claustre (2008), Relationships between the surface concentration of particulate organic carbon and optical properties in the eastern South Pacific and eastern Atlantic Oceans, Biogeosciences, 171-201. It is not clear if/how time interval of image pairs affects the evaluations.*

Response:

The Stramski et al. (2008) algorithm would need to be tested in the Gulf of Maine, but we do not expect it to perform better, because it is based on band ratios. Further, as mentioned in the text, we are interested in tracking the phytoplankton carbon (PC), i.e. that part of the POC that is associated with live phytoplankton and not the POC derived from land and heterotrophic processes. Chl_a provides a good estimate of PC.

Reviewer 2

Comment:

Calculation of productivity through time difference of ocean color (ignoring advection but done on large scales) have been done by Sathyendranath and co workers and recently by Behrenfeld (2010). It is worth mentioning it.

Response:

We should have referenced these studies and have now included them.

Comment:

I find the paper to lack in dealing with some sources of uncertainties:

a. CDOM effect on chlorophyll retrieval (it is known to affect chl algorithm in the GOM). While CDOM is likely to be conserved on the time scale involved, ratio algorithm may still be affected (possibly more than QAA or GSM like algorithm where the decomposition to a_{CDM} and [chl], even if not accurate, by difference will give the correct d[chl]/dt).
b. Effect of lateral mixing cannot be dealt well in the Lagrangian formulation but can affect observed concentrations.
c. Likely uncertainties due to dilution and more generally ML dynamics (see next comment).
d. Uncertainties due to biases in the model circulation fields prediction (e.g. when compared to GoMOOS or other models (in an ensemble sense)). -even if all the above are large, your approach is still useful and your concluding paragraphs can address

what advancements need to be done to improve the state of the art.

Response:

a.The reliability of satellite-derived Chl_a has been discussed in the response to Reviewer 1. We agree that CDOM is a potential source of error and will add this to the text: "The coastal Gulf of Maine is known to have high CDOM absorption values (Balch et al., 2008). CDOM absorption at lower wavelengths will typically cause a high bias in chlorophyll estimates." We note that errors attributable to CDOM are incorporated into the class estimates of Moore et al. (2009) mentioned above.
b. While mixing is not represented by a single Lagrangian trajectory, it does influence the statistics of a large number of particles. A cloud of particles will experience mixing because every particle, even if within the same grid cell, experiences somewhat different velocities. The velocity field is itself subject to some lateral viscosity in the circulation model. Since our results are based on grid-cell averages of a large number of particles (several hundred thousand are used in our model), they do incorporate the effects of mixing.
c. This is potentially a source of error for our method. Please see further discussion in the section about ML dynamics
d. We address the errors arising from the circulation field in Jönsson et al. (2009). It should should be noted that the model fields we use are from the GoMOOS model. The text will be changed to "The model, which is a part of the Gulf of Maine Ocean Observing System (GoMOOS) network, has been rigorously tested with observations."

Comment:

I assume the circulation model has a mixed-layer depth prediction. Why not use it and only focus on changes within the ML? Vertical shear is likely to decouple the ML from *what is below. In addition the $[chl]/z_{eu}$ scheme is extremely simplistic (given various formulations in the literature that provide attempts at vertical structure, e.g. Uitz et al.). It is also expected that below the ML chl/Carbon ratio will vary compared to the ML. In short, it seems (to me) least problematic to confine this analysis to the ML. Entrainment dynamics can be dealt with to some extent (assuming something about chl below the ML) while detrainment (e.g. ML shallowing) is easily accounted for.*

Response:

We had deliberated on whether to use the euphotic depth, z_{eu}, or MLD, and chose to use z_{eu} for several reasons. Most importantly, the MLD from the model is not reliable. This is because the Gulf of Maine is influenced by freshwater runoff, which is an input to the circulation model that is not well constrained. Also, vertical mixing in the Gulf of Maine is highly event (storm, tide, wave) based, making it difficult for the circulation model to capture the MLD response at all times. On the other hand, we have verified by using measurements that the light attenuation coefficient from satellite data, and consequently our estimate of z_{eu}, are quite reliable in the Gulf of Maine. Secondly, using MLD has its own problems. When $ML < z_{eu}$, which is often the case in the Gulf, a good fraction of the PC is produced and found beneath the ML base, and using z_{eu} in this situation is more appropriate, as using MLD would lead to an underestimate in the depth-integrated PC. When $ML > z_{eu}$, and the ML is actively mixed, it would indeed be better to budget PC over MLD, since using z_{eu} underestimates the depth-integrated PC. We want to investigate the connection between z_{eu} and ML in future studies, since we believe this to be a critical issue in estimating oceanic biological production.

Comment:

Providing (in graphs) estimates of mass specific NCP (e.g. $\gamma = 1/PC \cdot d(PC)/dt$) will be most useful as we can directly interpret them (they are $< \mu$, providing a population net doubling time).

Response:

The problem with the suggested plot is that large relative changes that correspond to small absolute values of PC dominate the results. Consequently, a change in PC from 0.1 to 0.5 appears much larger (by an order of magnitude) than one from 50 to 150, tending to exaggerate the errors and obliterate the interesting patterns. We have tried to find a non-arbitrary way of filtering out these spurious effects, but without much success.

Comment:

Please do not describe graphs in the text (e.g. bottom of 8962 for figure 4, top of 8963 for figure 5). Tell us what we should conclude from these figures and refer us to them.

Response:

We will change the text to
"It is clear from Fig. 4b-c, which shows events with coverage exceeding 30% and 60% respectively, that restricting the data to have a certain minimal areal coverage greatly reduces the temporal resolution of the time series that we aim to construct with these data."
and

"One of the objectives of this work is to examine the temporal variability of NCP_e in this continental shelf region. When we plot the median NCP_e of all available trajectories identified during the period 2004-06 (Fig. 5a), we find a significant variability in this estimate due to the large spatio-temporal variability of productivity in this region."

Comment:

I would not use γ to estimate μ as the former is most often much smaller than μ measured in cultures. Since θ_min is chl/C at high light and fast growth conditions here (nutrients are unlikely to be limiting) a guess a of one doubling per day may be less biased, and could at least be tested.

Response:

We have tested the sensitivity of our C:Chl model to different input parameters. It turns out that μ does not have much impact on C:Chl in the ranges of SST and Ig that normally occur in the Gulf of Maine, as long as μ is less than 1. Our method estimates the gulf-wide median γ to be about 0.2, which is probably low compared to μ. There are however, to our knowledge, no published results suggesting an average μ for the Gulf of Maine to be higher than 1, which is where our method becomes sensitive to the growth rate. We suspect that both μ, and the variability of C:Chl, are exaggerated in lab-experiments, where larger ranges in light and nutrient are used than what occurs in the ocean.

Comment:

Effects of buoyancy input on stratification (p. 8963 l. 20-25) should be diagnosable from the circulation model. In addition the large input of CDOM and its dilution may

bias chlorophyll dynamics.

Response:

The buoyancy input suffers from the same problems as discussed earlier when describing the limitations and unreliability in the modeled ML. A further complication is that the model tends to mix freshwater discharge from the coast too fast. The issue of CDOM is discussed in a previous response. It is much less of a problem with MODIS data.

Comment:

You may want to define a generalized "loss term", specify all the process which are included in it and hence on relate to it as "loss processes", rather than having to explain it in several places.

Response:

Losses arising from various processes are addressed in different parts of the text. Using a generalized loss term would not allow us to discuss these separately.

Reviewer 3

Comment:

Typos occur here and there. For example, P8957 Line 25-26: In this study, we use the flow fields from this model for the region shown in Figs. 1–3, saved at 3 hourly intervals for the period 2004-2006. P8958, Line 2: we restrict our analysis to the 2003–2006 period- starting from 2003 or 2004? P8968, Equation (A1): there are two equations; the latter should be right. P8973 Fig.1: Chl (mmol m-3), are you sure it is not (mg m-3)? P8978, Fig.6: Panel (B) no shows Net Primary Production (NPP) calculated using the VGPM algorithm (Behrenfeld and Falkowski, 1997) for the equivalent time span. -there should not be a "no" after (B).

Response:

The typos and errors have been corrected. The modeled period starts in 2004 Chl is expressed in mg/m^3.

Comment:

P8958 Line 16-22: We then convert satellite chlorophyll to PC, by employing a carbon:chlorophyll (C:Chl) model that is based on empirical relationships from laboratory studies (Geider et al., 1997). Our C:Chl model (see Appendix for details) is based on net phytoplankton growth rates derived from the particle trajectory analysis used to calculate the net productivity in our model. It turns out that within the range of surface PAR and temperature for the region, our results are relatively insensitive to the details of this model. *-I am confused with this paragraph. First you say you employ a C:Chl model from Geider et al.(1997). Then you say your C:Chl model is based on μ derived*

from the trajectory analysis. I read your appendix and still can't figure out the exact approach you take. And if you find out your results are relatively insensitive to the details of this model, how is it if you keep using a constant C:Chl ratio as you used for Fig.1 The calculation of Ig is also questionable. Why is a climatology used? And what is it? In a word, in order to show a convincing result, I think it is necessary to well define the uncertainty of each variable/parameter used in the model, including Chl and Ze, which have large uncertainty in the nearshore water.

Response:

We have clarified the text describing our C:Chl model. This model was originally developed in response to some colleagues pointing out that a fixed C:Chl may not be appropriate. It is based on relationships presented in Behrenfeld et al (2005), which are partly based on Geider et al. (1997). The main problem in adapting Geider's C:Chl model for remote sensing purposes is in estimating the nutrient-limited growth rate (μ). We circumvent this by using (dChl/dt)/Chl calculated along trajectories as an approximate value for μ. There are, admittedly, many problems with this approach, as has been discussed in our response to Reviewer 2. However, our results show that normally occurring combinations of SST, I_g, and growth rate (μ) in the Gulf of Maine give a much smaller variability of C:Chl than expected, and hence the C:Chl model is rather insensitive to errors in these input parameters.

We will clarify the text by changing it to: "We then convert satellite chlorophyll to PC, by employing a carbon:chlorophyll (C:Chl) model that is based on empirical relationships from laboratory studies (Behrenfeld et al., 2005; Geider et al., 1997) but uses the change of Chl_a along each trajectory as an estimation of the growth rate (see Appendix for details)".

We are using expressions from Appendix E in the 1978 edition of Almanac for Computers, Nautical Almanac Office via the AIR_SEA matlab package from WHOI to calculate a clear-sky surface PAR for the region. Since these numbers correspond to max (Ig) and our sensitivity experiments indicate that only higher light levels would significantly change C:Chl, we have decided to not use satellite derived PAR. The difference between using our C:Chl model and a constant C:Chl ratio of 60 is about 10-20% in the median NCP time-series (figure 5), primarily in the high highs and low lows. We have also addressed the issues about uncertainties in Chl and z_{eu} in our responses to the first reviewer.

References

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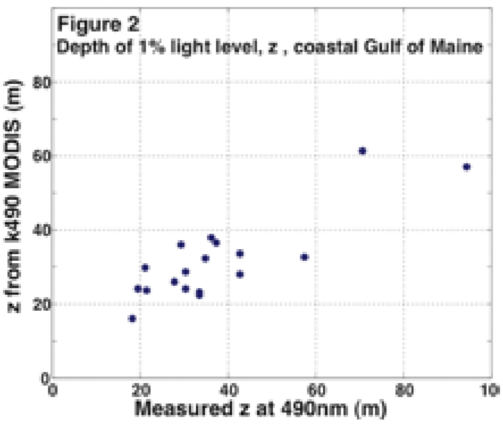

Fig. 1.