

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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This paper provides a novel way to estimate net primary production; combining a circulation model with satellite remote sensing it quantifies the Lagrangian rate of change of chlorophyll biomass. In the process it deals with effect of horizontal advection that is traditionally confounding production estimates.

I found this paper to be innovative and exciting and encourage its publication in BG. It is, in general, well written and clear. It is well suited for Biogeosciences as it deals with coupling of physics and biology and the resulting effect on carbon dynamics.

I have a series of comments that if addressed can significantly improve this manuscript
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which I provide below.

1. 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.

2. 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.

3. 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.

4. Providing (in graphs) estimates of mass specific NCP (e.g. $\gamma = 1/PC \, dPC/dt$) will be most useful as we can directly interpret them (they are $< \mu$, providing a population net doubling time).
5. 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.
6. 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 of one doubling per day may be less biased, and could at least be tested.
7. 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.
8. 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.

Dear authors: I am often wrong; if you feel my comments are 'off base' feel free to contact me and, if proven wrong, I will be happy to change them.

Interactive comment on Biogeosciences Discuss., 7, 8953, 2010.