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Biogeosciences
Discussions

Interactive comment on "A multi-method autonomous assessment of primary productivity and export efficiency in the springtime North Atlantic" *by* Nathan Briggs et al.

Nathan Briggs et al.

natebriggs@gmail.com

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We would like to thank Anonymous Referee 2 for this thorough and very helpful review, which has pointed out a number of minor errors in the text as well as areas in need of clarification. We agree with essentially all of this referee's comments, and incorporation of this feedback should substantially improve the clarity (and usefulness) of this manuscript. Below are our individual responses (black) to each comment (blue).

This manuscript provides a detailed account of a multi-method assessment of primary production and export efficiency carried out in the North Atlantic between April and June 2008. The research team used an impressive array of autonomous and classical

measurement techniques and devoted an important effort to calibrate their instruments. The methodology appears to have been carefully applied and the text is generally well written but difficult to follow in many places (e. g., section 3.2), due to the multiplicity of methods and acronyms (see also comments). The Discussion is thorough and well argued. Overall, this is an interesting manuscript that represents a substantial contribution to marine primary production measurements. Some generally minor comments are given below.

Thank you for your kind words and your very careful review. We are pleased that you find our work to be a substantial contribution to the primary productivity literature, and we appreciate your work to improve this contribution.

Other comments

Page 2 Lines 1-7. The term "understanding" appears 5 times in these lines. Perhaps some synonym can also be used.

We agree that this wording should be changed.

Line 5. "and also of the effects of PP"

We agree that this wording should be changed.

Page 5 Line 19. Define bbp (It does not appear until line 28).

We agree.

Lines 25-27. I suggest adding some brief background concerning the application of volume backscattering functions and POC estimations.

Good suggestion. We can add a single sentence to the beginning of the previous section (POC from beam attenuation) citing previous work linking light scattering measurements (including beam attenuation and backscattering) to POC in open ocean waters (low inorganic sediment load).

Page 6 Line 16. "a 30 m vertical interval and a 1 day time interval were considered equidistant". Explain more clearly. (The same in Page 7, lines 4-5).

We used triangulation-based 2-D linear interpolation (Matlab function griddata). For the purposes of this interpolation, the distance between points was calculated as [(z1/30-z2/30)$^2 + (t1 - t2)^2]^{0.5}$,

where z is depth in meters and t is time in days. This favors interpolation in time when time gaps between measurements (in days) are less than 1/30 of vertical spatial gaps in measurements (in meters), and vice versa.

Page 7. Lines 6-8. Explain more clearly.

When the float is actively profiling (not following the vertical motion of the water), it could entrain water, over-estimating MLD during downward profiles and under-estimating MLD during upward profiles. However, the profile data are critical to the MLD calculation and cannot be discarded. Therefore, the MLD is calculated twice, once using only downward profiles and once using only upward profiles. Note that this method also smooths out effects of internal waves, which can make the depth of an isopycnal in a single profile (up or down) unrepresentative of the mean isopycnal depth. Profiles were distinguished from Lagrangian or near-Lagrangian motion using a vertical velocity threshold of 1 m min^{-1}.

We can slightly expand our explanation in the text to make this method and its motivation clearer.

Line 11. Explain briefly the role of the Bagniewski et al. model, cited in the explanation of Fig. 3 (and later in the text).

The MLD determination described in this section does not utilize the Bagneiwski et al. model. The temperature and salinity fields of the model are strongly constrained by the daily float profiles, but the diel mixing dynamics are slightly different, so MLD was calculated separately using the model output. This calculation used nearly the same

method as described here. For each model timestep, MLD was the shallowest depth where the potential density anomaly exceeded the minimum potential density anomaly by $\geq$ 0.01 kg/m^3.

However, the first three steps, smoothing, binning, and interpolating, were not needed, because the model output is already smooth and continuous. We agree that this should be clarified in the text.

Line 23 "in-situ KPAR". Is this the KPAR derived from eq. 2?

Yes. We should replace the term KPAR in this sentence with the more precise term KPAR(measured)

Page 8 Line 15. Define ï ËŻAs (greek theta).

Agreed. Thank you for catching this.

Line 23 (and following). Air-sea.

We agree that a hyphen should be added.

Page 9 ′ Lines 12-17. Difficult to follow. Explain more clearly.

We can rework this section if it is not clear and run it by other colleagues for clarity.

Page 10 Line 10 (eq. 7). It would be helpful to provide some background on the deduction of this empirical model.

The equation is based on a conceptual model that there is a limiting step in photosynthesis that becomes saturated when it receives too much energy at once. The energy comes in packets and if too many packets arrive during the same period of time, then some energy is wasted. The epsilon parameter denotes how many packets can be received at once without being wasted. It represents a sort of energy "buffer" at the rate-limiting step. It was introduced because empirical models without a buffer don't seem to fit our observations. We can add text to such effect.

Page 11 Lines 12.13. Explain more clearly. Perhaps a scheme would help.

Separation into large and small particles follows the method of Briggs et al. (2011). We will try make this text clearer and also state that a visual schematic is available in Briggs et al. (2011) if further clarification is needed.

Line 4. This observation may be valuable for 14C fixation experiments and should be discussed in more detail.

We assume this comment refers to line 4 of page 10 (our conclusion, based on in situ dO2/dt, that bottle photoinhibition is not representative of most field conditions). We agree that it would be useful to briefly mention and discuss this finding in the discussion section.

Page 12 Lines 5-10. Figure 8 does not have indcations a, b, c . . .

Thank you for catching this error. This text referred to a previous version of figure 8.

Line 8. Where is GPPbbp in Fig. 8?

Thank you for catching this error. This text referred to a previous version of figure 8.

Line 11. "both GOP/GPPChl and GPPcp/GPPChl were substantially lower" Lower than what?

Lower than during the bloom growth phase. We agree that this sentence should be clarified.

Line 24. Eliminate "depth-integrated" (repeated later).

Thank you for catching this error.

Page 13 Lines 1-2. It would be helpful to indicate that this "apparent community respiration" refers to the negative NCP.

We can add "apparent community respiration (difference between GPPChl and NCP)"

Line 22 (and Page 14, line 2). Indicate that the slope is given in Fig. 9.

We agree that this clarification would be helpful.

Page 14. Line 16. Revise sentence.

Thank you for noticing this error. The sentence is missing an "and". It should read: "This conclusion agrees with the coupled physical-biological model of Bagniewski et al. (2011), which assimilated float biogeochemical measurements AND achieved optimal fit when diatom GPP was limited by SiO4 with a half-saturation constant of 1 μmol m^{-3}."

Page 15 Line 1. Eliminate the first "the".

Agreed. Thank you.

Line 32. "advection of the float realtive to ML". Explain more clearly. ËŹ

We are referring to horizontal advection of the float relative to the mixed layer during the hours that the float is below the mixed layer.

Page 19 Line s 9-10. Where can we see the "flux attenuation in the 100 m below the euphotic zone"?.

This statement refers a comparison between export estimates from 60 m (Fig. 10) and export at 125 m (Fig. 11a) during the main bloom. We agree that this should be clarified in the text.

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