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

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We would like to thank Anonymous Referee 1 for this helpful review, which has pointed out several important ways to improve our manuscript. Below are our initial responses (black) to each comment (blue). Final responses with reference to text will be made once we have revised the manuscript.

This is a nice exercise, and adds to the growing literature on comparisons of methods for primary production. I have five comments.

Thank you for your comment. It is also our opinion that this work adds to the broader

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literature of PP method comparisons.

1. I'm not sure why the authors chose to cite Cullen et al. (1992). That study doesn't have any actual diel data; any diel relationships were guessed at. For example, if I remember correctly, they simply multiply their change in cp by 10. Also, Cullen et al. (1992) focus on growth rate, not productivity. Growth rate means a normalization to biomass, and therefore a much tougher estimate. I remember reading a recent paper by White et al., published last year (?) in GRL, which would be a better choice.

Thank you for pointing out the paper by White et al. (2017). This paper is indeed highly relevant, and we apologize to the reviewer and the authors for omitting it. We will need to revise our text slightly regarding the degree of novelty of our beam attenuation results, because this paper contains a more robust quantitative validation of the cp diel cycles method than previous studies that we were aware of.

2. This work is not entirely novel, although I suppose the use of gliders is, and the incorporation of PvsE estimates. But the same kind of results, with similar good (actually, maybe better) agreement was done in JGOFS' NABE, 20 years before these were done, and reported in Marra (2002) and Marra (2009, Aquat. Microbial Ecol., Fig. 4).

Thank you for pointing out the work of Marra (2004) from JGOFS. It contains an important quantitative comparison between daytime net POC production from beam attenuation and other estimates of productivity. We will add reference to this work. We disagree, though, with the reviewer's implication that our validation results do not represent a significant advance over this previous work. We see two main advances:

1. We compare two estimates of the same quantity – GPP – and both of our estimates exclude gross DOC production. This allows a more precise validation than comparisons presented in Marra (2002) between differing, but related quantities (daytime net POC production from beam attenuation, 14C assimilation, and net CO2 utilization). The first of these quantities includes loss terms from export and heterotrophic respiration, the second excludes both of these loss terms, and the third method includes

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loss from heterotrophic respiration but excludes export and further includes net PIC and DOC production. The White et al. (2017) paper mentioned in the previous comment represents an advance relative to Marra (2002) in this respect, but still compares GP with NPP. 2. Our validation dataset shows not only the mean agreement between methods, but also their correlation over an order of magnitude of productivities, including pre-bloom, diatom bloom, and post-bloom conditions. Analysis of correlation over a high dynamic range adds significant value to a validation exercise. The White et al. (2017) paper mentioned in the previous paper also represents a significant advance in this respect, but our data still have higher dynamic range (factor of 10 vs. factor of 3.5) and much higher maximum GPP (8 vs. 1.75 mmol C/m2/d), adding further value.

We will add these citations and do our best to put or contributions in proper context in the revision. We have also decided to add related work on in situ CO2 and DIC diel cycles to our discussion (e.g. Johnson, 2010 and Merlivat et al., 2015).

3. It would have been useful to plot the time courses of GPPchl, Chl, and POCcp together. GPPchl looks to be very close to the biomass measures, which means a simple multiplier to get from biomass to productivity. I'm not sure what this means. I would guess they shouldn't be so well matched, and that GPPchl would be expressed earlier in the bloom than Chl or POCcp. That they are well-matched in time, is dubious. In any case, that matchup should be discussed.

We agree the precise temporal matchup between changes in GPP and biomass is an interesting topic, and this comparison is well suited to our high-resolution, Lagrangian dataset. We will try to cleanly add the Chl timeseries to Figure 8 of the text to make this matchup clear, but for now, please see Fig. 1 at the end of this comment for the temporal matchup between mixed-layer GPPchl, Chl, and POC estimates. While there is clearly a first-order correlation between GPP and biomass, increases in GPPchl do in fact precede increases in biomass in each rapid growth phase, as the reviewer correctly suggests should be the case. This is due to higher average light in the ML, primarily due to shoaling MLD, but also enhanced by higher surface irradiance. As a third-order

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effect, the late April increases in POC slightly precede increases in Chl, perhaps due to reduction in cellular Chl following ML shoaling.

4. Bender et al. 1992 is cited incorrectly. The authors list is: Michael Bender, Hugh Ducklow, John Kiddon, John Marra, and John Martin. Makes me think the authors didn't read the paper.

We apologize for our error in excluding the last two authors and thank the reviewer for catching it. The citation was generated automatically using Mendeley software, which extracts the author list from a PDF, and we did not check the extracted information in enough detail to catch this error. The further suggestion that we did not read the paper, however, is unfounded. The finding that we cite from this paper (GOP/NPP ratio of 2.5) is derived from the ratio of two different numbers in the paper: a GOP/NPP(14h) ratio of 2.0 (Fig. 4 on p1714), and a NPP(24h)/NPP(14h) ratio of 0.8 (on p1712). We are not aware of any way that we could have obtained this number without reading and understanding the relevant parts of the paper.

5. In section 2.8.2 there is the phrase: "...incubations were performed at..." Actors "perform," not ocean-going scientists (at least not at sea).

One of the definitions of the verb "to perform" is "to carry out". If "performed" brings up strange connotations, we'd be happy to replace with the term "carried out".

6. I can't find where the authors talk about the environmental limitations in finding their relationships. Will the agreement among the methods that they find only happen when there is a shallowing mixed layer and increasing biomass? Will GPPchl still agree with GPPcp when the mixed layer is deepening, such as during a storm?

Our validation data span a range of conditions, including periods of ML shoaling, a period of ML deepening at the end of April, increasing biomass, decreasing biomass (Si depletion period), and stable biomass in the post-bloom period. After averaging out some variability due to single episodic events using 3-day means, the methods agree closely during all of these periods, except the period of Si depletion. We agree with Referee1 that it would be helpful to explicitly discuss this aspect of our findings in the revised text in order to help guide future application of the method.

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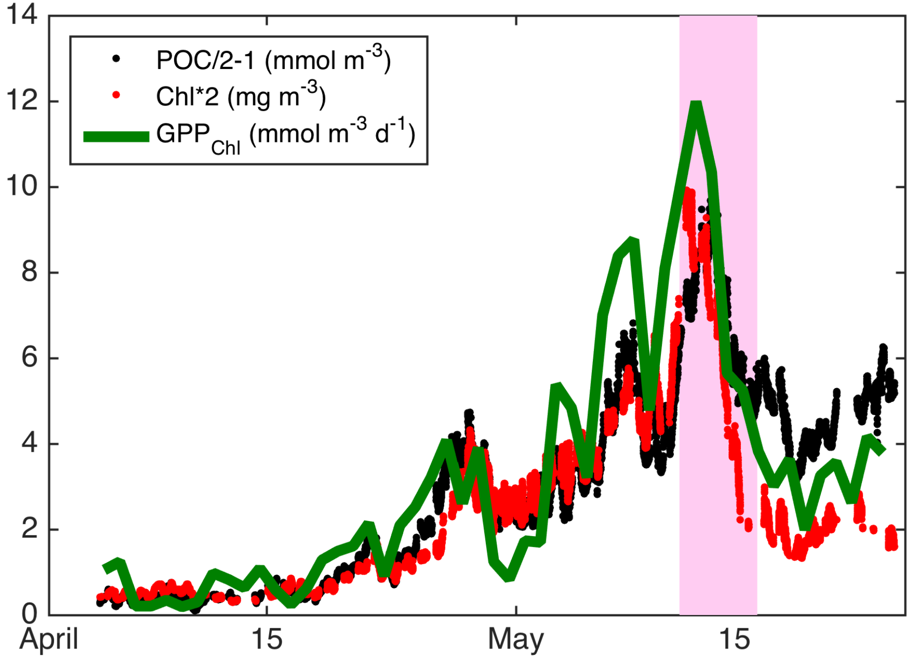

Fig. 1. Timeseries of mixed-layer GPP and scaled mixed-layer biomass

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