Dear Marjorie Friedrichs

We thank you for this opportunity to revise and resubmit our manuscript and are grateful for the many constructive comments provided by our reviewers. We have made multiple changes to the manuscript in response to these comments and believe that this has strengthened our publication. We also made small editorial changes. Please find attached the revised version of our manuscript (both clean and with track change comments) as well as a point by point response to the reviewers comments.

In the manuscript we repeatedly refer to a manuscript submitted to JGR-Biogeosciences in revision (Wang et al. 2019JG005569R, Lagrangian studies of net community production: The effect of diel and multi-day non-steady state factors and vertical fluxes on O2/Ar in a dynamic upwelling region). If possible we would like to keep the citations to this manuscript in the text and if at all possible even link the both MS (once accepted) between the JGR journals. The manuscript is attached to this resubmission.

Best,

Sven Kranz

Response to reviewers:

**Reviewer #1 Evaluations:**Recommendation (Required): Return to author for minor revisions  
Significant: Yes, the science is at the forefront of the discipline.  
Supported: Yes  
Referencing: Yes  
Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.  
Data: Yes  
Accurate Key Points: Yes  
  
Reviewer #1 (Formal Review for Authors (shown to authors)):  
  
This paper presents a novel multi-method approach to assess gross primary production, net primary production, net community production and new production. The comparison of multiple methods is a challenging and complicated process, especially when applied to a temporally and spatially dynamic ecosystem, and the authors have done well to constrain errors and uncertainties associated with each method and have reasonably attempted to explain discrepancies between methods.  
  
It is my recommendation that this paper be published after very minor corrections are applied.  
  
Line 167: minor typographical error 'GGP' should be 'GPP'?

We thank the reviewer for catching this typo. We changed it accordingly.

Line 228: Please briefly detail the acidification method (e.g. direct application of acid or fuming) and acid type and concentration.

As requested, we added additional information on the acidification method. We want to emphasize that the acid quality usually does not matter for 14C analysis as the measurement is only detecting 14C decay. HCl should not have detectable 14C background. Using HCl for POC analysis, the acid is known to cause contamination. In our lab we usually use trace clean acid and fumigation for POC filter analysis while 14C acidification is usually performed with (6-12N) molecular biology grade HCl .

Section 3.4: Line 233-248. In this section acronyms NP (for New Production) and NPP (Net Primary production) are used interchangeably here, but I think it is supposed to be NPP only?

We thank the reviewer for pointing this out! We changed the text and Eq. 1 accordingly!

Section 3.7 Please specify the light level NPQNSV is determined for use in the e:C estimations.

As recognized by the reviewer, is strongly light dependent and changes throughout the water column. Consequently, we calculate based on our data from the P vs. E correlations, the diel changes in light intensity (as measured by the shipboard light meter) and the attenuation coefficient measured/calculated from our noon CTD deployments. Consequently, we used variable ratios to calculate productivity throughout the mixed layer. Data shown are the integrated values throughout the mixed layer.

Section 4.2.7. GPPFRRF Estimates  
This is a commentary, rather than a suggestion for revision. I find the prominent afternoon peak in GPPFRRF intriguing and unexpected. It is such a dramatic increase in GPP and it is a little hard for me to determine the timing from the x-axis, but it looks to be soon after midday. I have been trying to come up with an explanation for this from uncertainties and weaknesses in the algorithm and fitting of the PvI curves but have not come up with anything significant. Silsbe and Kromkamp (2012, L&O Modeling the irradiance dependency of the quantum efficiency of photosynthesis) have previously published some interesting work demonstrating the statistical flaws in FRRF estimated GPP as the derived 'GPP' is not independent of E the dependent variable which impacts derived PvI parameters (uncertainties are higher). They also showed of the most used PvI models applied the Platt et al. 1980 performs poorly when comparing the number of fitted curves with statistically significant derived parameters. I have certainly seen this in my own data analysis, especially in data from significantly light stressed phytoplankton populations. I'm not sure if the authors have evaluated the statistical significance of the derived PvI parameters and/or the model uncertainties from the fits but it may be worth revisiting or at least keeping in mind for future applications of GPPFRRF methods.

We thank the reviewer for this very thoughtful comment as she/he highlights an important issue for optimization in non-linear systems. Since many model parameters may be correlated, determining the uncertainty for a particular parameter is non-trivial. We should highlight that the productivity results shown here are not contingent on individual parameters but instead on a set of “optimal” parameters and that we have not focused extensively on the details of the optimization procedure or on the uncertainty analysis for it (e.g. a Monte Carlo approach). Our modeled rates of JVPII are well constrained by the data with uncertainties of several percent, whereas the uncertainty within a single model parameter (such as Alpha) may be quite significant for the reasons that the reviewer mentions.

Based on the reviewers comments, we revisited our data, performed a cost analysis of the fits, and refitted all our curves. While examining these data, we decided to remove data from the PvsE curves where light intensities were >2000uE. Most of these data were extremely noisy and hence created a larger uncertainty. We believe that this cut-off is reasonable as most communities won’t see these light intensities in the water column. Despite the cutoff, some cycles did show a light inhibiting term (see Fig S2).

Regarding the spikes seen in GPP we have been thinking about this ourselves and our only explanation was a correlation with the afternoon net-tows when we moved through the water mass for a few hours. After revisiting the dataset we realized that in some of the measurements (P1706-C1\_2, C2\_2) the spike coincides with a higher NPQNSV at both EK values and 2XEk value, yet no correlation with a high beta (light-inhibition) was found. With higher NPQ our would increase, which would decrease the rates of GPP, rather than increase it. New rates were adjusted accordingly.

We thank the reviewer for pointing us in this direction and will keep these issues in mind for further GPP analysis.

Section 4.3: Most of this text belongs in the discussion, it is interpretation and discussion of results, not presentation of results. Please move all interpretation and discussion to the discussion section.

We agree with the reviewer and moved all of the discussion from this paragraph to the discussion section (lines 622-642).

Line 488 'EK' the 'K' should be subscript

Done.

Line 488: 'NPQ' should be 'NPQNSV'. Also as suggested below for Supplementary Figure S1, I would highly recommend presenting the NPQNSV values as part of Supplementary Figure S1

We changed NPQ to NPQNSV in the section. We also added NPQNSV (at Ek and 2xEk) to our data.

Line 526: Given that chlorophyll concentration is mentioned a number of times throughout the paper I would suggest including it in either the main data figures or supplementary figures.

A graph containing information on Chl a is now presented in the supplemental material (Figure S2).  
  
Section 4.5: This text also belongs in the discussion. Again, not presentation of results. 

We do agree that some of the sentences were more discussion based and moved those to the discussion section (line 544) or deleted them entirely. We however believe that the data comparison should be placed in the result section.

Supplementary Figure S1: I would recommend including Beta and also NPQNSV in this figure, especially as NPQNSV is such an important parameter for the GPPFRRF estimates.  
  
We added NPQNSV , alpha and beta values to Figure S2.

**Reviewer #2 Evaluations:**Recommendation (Required): Return to author for minor revisions  
Significant: Yes, the paper is a significant contribution and worthy of prompt publication.  
Supported: Mostly yes, but some further information and/or data are needed.  
Referencing: Yes  
Quality: The organization of the manuscript and presentation of the data and results need some improvement.  
Data: Yes  
Accurate Key Points: Yes  
  
Reviewer #2 (Formal Review for Authors (shown to authors)):

Review of Kranz et al.  
  
This is a nice exercise, and a welcome comparison of various methods to assess ecosystem processes in a highly variable environment. The (unstated) question is how can we apply incubator methods to more rapidly changing environmental conditions? Nevertheless, I think it will assist the further investigations in the CCE LTER, as well as in other regimes. Most of my comments are minor, and should be easily addressed by the authors.

We thank the reviewer for the constructive critiques on our manuscript.

1. Lines 22-25: These two sentences should be deleted, since they are redundant with the title. Abstracts should be written in the 3rd person, so the next sentence should read "A mult-method...is presented..."

We thank the reviewer for pointing this out. We performed the suggested changes.

2. Lines 39,40: Specifically, what "ecosystem processes" would have gone unnoticed? You need to be more specific here.

If measured in an Eularian approach rather than a Lagrangian approach, or measuring not all of the productivity metrics different interpretations of productivity will be obtained. In the abstract we now highlight the temporal and spatial scales (see also comment below and lines 37-38), however, we did not find it necessary to go more into detail of which ecosystem processes might be missed as we felt that this is discussed in the manuscript in length. Briefly:

Both rates, rate-ratios and timescales (GPP/NPP or NCP/Export over timescales of days (single cycles) to weeks (e.g. P1706\_C2 and C4) hold important information on ecosystem processes (e.g. how much of the photosynthate is incorporated into biomass and how much of the biomass is leaving the investigated region over a certain timeframe).

3. Lines 40-42: A weak conclusion. Every investigation "can help further our understanding..."

We agree with the reviewer that every investigation can help to better understand certain ecosystem processes. Here we wanted to highlight that surface productivity and export can exhibit strong spatial and temporal decoupling in this complex system. We modified the sentence to highlight temporal and spatial scales and also now highlight that we investigated a gradient between ecosystem (coastal and offshore).

4. Lines 61-62: An understatement!

We modified the sentence to make the statement on eastern upwelling regions stronger. The sentence now reads: Upwelling plays a key role in driving marine primary production along the eastern continental margins of the world’s oceans, making these ecosystems some of the most productive regions in the world  
  
5. Line 64: can support?

Agreed, we deleted “can”  
  
6. Line 75: Delete "By the same token."

Done!  
  
7. Line 76: What does "time-intensive" mean? Why not just say incubation techniques can't resolve highly spatially variable situations?

We thank the reviewer and changed the sentence accordingly.

8. Line 81: I've read Quay et al. (2010), and found it distinctly un-useful and often wrong. 

Quay et al is related to our study as it describes multiple approaches over different timescales to understand ecosystem productivity. While some of the rates and ratios presented in Quay et al might not be comparable with our dataset we still find it fair to cite their effort. Consequently, we decided not to ignore Quay et al (2010).

9. Line 96: Recommend starting a new paragraph at "Two novel..."

Done

10. Line 167: GGP?

Changed

11. Line 295: P vs E is more correct, given that E is the consensus designator for irradiance.

Changed  
  
12. Lines 3i8ff. Light attenuation from the CTD PAR sensor can be difficult with ship and instrument shadow. You need more justification here.

We agree with the reviewer that assessing PAR through the in-situ water column is a challenging endeavor as has been detailed in e.g. Piskozub, 2004 (Oceanologia).

As stated in the text, we used the *in situ* light attenuation from the CTD profile around noon to calculate the light field in the mixed layer. The time-varying *in situ* light field was modeled using the empirical transmission-light attenuation relationship and surface photosynthetically active radiation (PAR) measured by the ship’s meteorological system. The approach used was taken so as to minimize the number of assumptions and rely on an empirical approach. While shadows from the ship may still have influenced some of our measurements, there was no evidence for a systematic offset between near-surface PAR values on some casts (if the ship were a significant factor then upwards of 50% of the casts should have shown an effect). We view this approach as a first order approximation and welcome any suggestions for future cruises.

13. Line 453: "Acclimaitization" is a mouthful, and not a commonly used term. "Acclimation" is better.

Changed!

14. Line 592: Is there a citation for this statement about stress and the PQ?

We thank the reviewer to point out the missing citations. We added two citations to the text! (Iriarte, 1999; Spilling et al 2015)  
  
15. Line 607: I think this could be stated more clearly. Production transfer?

We rephrased the sentence. It now reads: “..the difference in portions of GPP recovered by NPP14C and NPPG/G might be interpreted as measure of production losses during transfer of the organic matter through the food-web. “   
  
16: Line 627: change to "to re-oxidize fully..."

Changed!

17: Line 713: I don't think "seeing" Wang et al (submitted) is possible at this stage of publication.

This is obviously true. Wang et al was recently resubmitted. We hope that we can change the submission status to an accepted/in press status during the MS-proof correction. Nonetheless, we deleted “see” from the text!

18. Line 718: New paragraph?

Yes – change performed!

19. Line 728: "equal to..."

Done  
  
20: Lines 732ff: Interesting discussion, but it comes down to the time scale at which a balance between NP and NCP is achieved. Annually? Seasonally? I'm not that familiar with the cited work, but instead of other processes, the time scale for the potential equivalence might be mentioned.

We agree with the reviewer here but are hesitant to add timescales to the discussion as the time scale of interest depends on the temporal dynamics of a region (and reference frame - Eulerian or Lagrangian).  Essentially, the time scale has to be long enough to integrate over any temporal perturbations to the system (i.e., phytoplankton biomass, POM, NO3, and NH4 have to return to approximately their background state).   For the western Antarctic Peninsula (site of the Tortell et al. and Stukel et al. studies) this means integrating over the summer growth season.  For a specific upwelled water parcel in the CCE tracked in a Lagrangian framework, this means integrating over a long enough period that the residual biomass in the system is small relative to the temporally-integrated export, NCP, and NP.  In a mechanistic approach, one needs to make sure to not sample over just the increasing phase of a bloom or just the decline phase. If, one did sample only over a single phase of a bloom, she/he might measure excess NCP and NP (with the respective export likely not to happen until after sampling was completed) or excess export (because NCP and NP happened before sampling was started).

21. Fig. 2. Nice figure, and I don't remember seeing the results presented on the shoaling or deepening of the mixed layer, except in passing. And I don't remember seeing the secular change in MLD discussed with respect to the parameters measured.

We thank the reviewer for appreciating this figure

The ML effect of PP was mentioned in line 512-513 and line 615. We now added links to the Figure. In general, MLD was calculated into the data presented (GPPFRRF, NPP, NP and Export). Changes in MLD were also accounted for in NCP.