Dear Dr. Wang:

Thank you for submitting "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" [Paper #2019JG005569] to Journal of Geophysical Research - Biogeosciences. I have received 2 constructive reviews of your manuscript, which are included below and/or attached. As you can see, both referees think the manuscript has been significantly improved but raise a number of issues that still need your attention. Rev#1's comments are fairly specific and should be straight forward to address. Rev#2, on the other hand, raises some important questions that I think will require some additional clarification and re-writing. This reviewer also raises several specific comments that you'll want to address in the next revision. Given the comments from both reviewers and the changes expected to address them, I think the manuscript will require moderate revisions and thus I am recommending 'major revisions' to provide ample time to implement all necessary changes.

Please submit a revised manuscript that addresses the reviews and any editorial comments by March 4, 2020.

In your revision, please follow our Checklist and use our Templates for the main file and any supplements. Please provide the following:

1. A response to reviewer file that lists each major comment and describes how the manuscript has/has not been modified in response to those comments.

2. A copy of the manuscript with the changes noted (e.g., highlighted, "track changes," italics or bold changes).

3. The final revised manuscript with changes incorporated and separate final figure files (figure parts should be combined into a single file), which will be used for publication if the manuscript is accepted. If final figures are already uploaded, they can be easily copied over to the next revision version.

4. If any, supporting information text, figures, captions, and small tables in single PDF file using AGU's template. Large data tables and multimedia should be uploaded separately.

AGU requires that data needed to understand and build upon the published research be available in public repositories following best practices. This includes an explicit statement in the Acknowledgments section on where users can access or find the data for this paper. Citations to archived data should be included in your reference list. All references, including those cited in supporting information, should be included in the main reference list and cited in-text. All listed references must be available to the general reader by the time of acceptance. AGU requires the corresponding author, and encourages all authors, to register for an ORCID.

Please check and verify authorship, and that all authors are included, have approved the revisions, and agreed to be listed in the order given. Authorship is final with publication. Responsibilities of the corresponding author are given here.

When you are ready to submit your revision, please login to your account (https://jgr-biogeosciences-submit.agu.org/cgi-bin/main.plex) and click "Revise 2019JG005569."

I look forward to receiving your revised manuscript. If you have any questions, or need additional time to complete your revisions, please contact us at jgr-biogeosciences@agu.org.

Yours sincerely,

Miguel Goni

Editor in Chief

JGR-Biogeosciences

------------IMPORTANT INFORMATION------------------------

Additional information on text preparation, formatting, acceptable file formats, supporting information, graphics preparation, and AGU style, is here.

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Reviewer #1 Evaluations:

Recommendation (Required): Return to author for minor revisions

Significant: Yes, the paper is a significant contribution and worthy of prompt publication.

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):

I recommend accepting this revised manuscript with a few very minor changes. As with the previous version, the problem tackled (testing the O2/Ar method under extreme conditions) will be interesting to the community working with this method. The data collected following water parcels in an upwelling zone is highly relevant to the problem addressed and is interpreted well. I also found the writing in the paper to flow well and be clear. I have a few very minor suggestions from my read of this version.

- Roberta Hamme

Line 240: Suggest using units of umol/kg rather than mL/L for oxygen concentration, or including both units.

**We have changed these units accordingly.**

Lines 267-8: Remove reference to MIMS data, which I think is no longer presented in this version.

**We have removed this sentence, as MIMS data is indeed no longer presented in the manuscript.**

Equation 2: Because you removed the x100 from Equation 1, you do not need to divide the O2/Ar saturation anomaly by 100 in Equation 2.

**This correction has now been made.**

Line 301: Suggest including the erratum to Garcia and Gordon in your citation. The mistake in their original 1992 paper is a bad one. García, H. E., and L. I. Gordon (1993), Erratum: Oxygen solubility in seawater: Better fitting equations, Limnol. Oceanogr., 38, 656.

**In response to the reviewer’s suggestion, we now additionally incorporate the reference to this paper’s erratum.**

Lines 457-8: It might be worth mentioning the proportionality to the mixed layer depth here as well.

**We assume that this refers to the statement made in lines 517-519: “While changes in measured MLD do drive excursions in NCPRT, the overall offset between NCPRT and NCPprior remains generally …” This text at lines 517-519 was actually misleading and was not intended to imply that NCPRT exhibited a relationship with MLD in this study.**

**We have revised this sentence (lines 518-521 in revised document) accordingly to read: “While changes in measured MLD would affect the NCPRT calculation, the overall offset between NCPRT and NCPprior appears generally…”**

Lines 560-1: Suggest rephrasing. It is not violation of the steady state assumption that is the problem here but rather violation of the assumption that vertical fluxes do not affect the surface mass balance.

**Following this suggestion, we have revised this sentence.**

Lines 588-590: Suggest rephrasing. It is not the exponential weighting of the wind speed history that matters to diel variations, especially considering that you use daily-averaged wind speeds. It is rather that the biological productivity signal recorded by O2/Ar is inherently heavily weighted toward the most recent time period.

**We have rephrased this sentence accordingly.**

Lines 631 and 672: Suggest broadening your statement. The problems you bring up apply not just to underway but also to discrete measurements.

**We now mention that these considerations apply to discrete as well as underway measurements.**

Figure 2a: Suggest removing "d(DO2/Ar)/dt = 0" in panel a. The steady state assumption is not necessary to the NCP\_prior method.

**This change has been made.**

Reviewer #2 Evaluations:

Recommendation (Required): Return to author for minor revisions

Significant: The paper has some unclear or incomplete reasoning but will likely be a significant contribution with revision and clarification.

Supported: Mostly yes, but some further information and/or data are needed.

Referencing: Yes

Quality: Yes, it is well-written, logically organized, and the figures and tables are appropriate.

Data: Yes

Accurate Key Points: Yes

Reviewer #2 (Formal Review for Authors):

The authors have done a nice job incorporating some the reviewers comments but I still think there are a few things that are still not totally clear in the revised manuscript. First, the authors state: "We conclude that the non-steady-state rate of change term represents a considerable influence upon attempts to estimate short-term NCP rates in this region." Maybe I am missing something but I don't quite understand why non-steady state conditions would affect NCP evaluated over shorter time scales only (NCP-RT). As Roberta Hamme described in her review, NCPprior represents an exponentially weighted average of the past NCP rates. In the absence of vertical (and horizontal) fluxes, non-steady conditions would affect how we interpret NCPprior, as this estimate is heavily weighted toward the most recent time period (rather than being an even mean). In the case of NCP-RT, the estimate takes into account the current rate of change in ΔO2/Ar so I don't see why non-steady state conditions would affect it. If the reason for the non-steady state conditions is the presence of vertical fluxes, then this would affect both estimates (or only NCPprior if the vertical fluxes occurred prior to the measurements). Please clarify this. It is not obvious to me why the authors think the NCP-RT method is less accurate than the NCP-prior method.

**NCPRT takes into account the current (over the span of measurements) rate of change in ΔO2/Ar but cannot distinguish whether that change is due to real changes in biological activity or due to changes in how air-sea exchange processes are affecting a previously-existing oxygen signal. Accounting for the wind speed history is required to tackle the latter effect. We explain this in greater depth below in our responses to the reviewer’s specific comments (answers to reviewer feedback regarding lines L574-575, L581-583), and we have edited the manuscript to better clarify these points.**

The vertical advection of O2 undersaturated water together with the fact that both estimates integrate over different timescales likely explain the differences observed between NCPprior and NCP-RT in this study. Actually, both estimates are probably wrong because, as the authors state, vertical fluxes need to be taken into account in order to obtain meaningful estimates of NCP in this environment.  
  
**In general we agree with this comment. However, there are a couple of important subtleties that we want to point out. The different timescales of integration are precisely the issue we aim to highlight here. “Non-steady-state” conditions (biologically mediated or physically affected e.g. upwelling) prior to the timeframe of measurement will affect the ΔO2/Ar signal which tends to achieve equilibrium over longer timescales (relaxation towards a steady state system). The d(ΔO2/Ar)/dt term does not sufficiently account for non-steady-state conditions, since the slope term is affected by non-steady-state factors (biology, upwelling or wind-related) that could have occurred prior to the period over which the slope term was calculated. (Please also see response to comment L574-575).  
  
Vertical fluxes also could explain some of the differences between NCPprior and NCPRT, but as described above in the response to reviewer 1 we do not think that they are the only factor responsible.**  
  
This brings me to my second comment. Using the model output the authors estimate vertical O2 advection fluxes. However, they do not seem to trust these numbers as there is no attempt to correct NCP estimates (despite presenting equations 4 and 8). That is likely because these fluxes are huge. I think the authors should discuss a bit more whether these advective fluxes are at all realistic. For example, if you corrected NCPprior or NCP-RT for vertical advection you would obtain NCP up to ~1000 mmol O2 m-2 d-1 (for P1706-1, with a mixed layer depth of only 20m). Are these rates physiologically plausible (given the biomass)? How do they compare with previous NCP or export estimates from similar environments? Would you still be able to observe a diel cycle in ΔO2/Ar with the presence of such large O2 vertical fluxes? I think the manuscript would improve by having such an analysis.

**We do discuss the plausibility of our estimated vertical advective fluxes in detail in section 4.3. As the error associated with measuring the subsurface O2 gradient or the mixed layer depth is small relative to the uncertainty involved in the use of modeled vertical velocities, those vertical velocities represent the largest potential source of error. As described in this section, however, such vertical velocities are not unusual for this region and similar coastal upwelling sites.**

**In response to this feedback, however, we have expanded upon our discussion of whether productivity rates corrected using our estimated vertical fluxes could be physiologically plausible or comparable to other productivity measurements from this region or similar environments. We also suggest an alternate approach (using Lagrangian trajectories), that is beyond the scope of the present study, but may provide a fruitful avenue for future attempts to incorporate vertical velocities into NCP estimates.  
  
At the stations with the largest advective fluxes (P1604-4, P1706-1, P1706-2), NCP rates reaching the thousands of mmol O2/m^2/d once corrected for vertical advection would not be physiologically plausible. Chl-a values observed during our study were between 3.1-9.3 ug/L, compared to peak values of ~25 ug/L during blooms in Chesapeake Bay, during which integrated primary production rates as determined from 14C incubations did not exceed ~300 mmol O2/m^2/d.**

Specific comments

L267-268 This sentence should be removed as the authors no longer present MIMS data.

**This correction has now been made.**

Eq 2 The authors no longer multiply by 100 in equation 1 to calculate ΔO2/Ar, so there is no need to divide by 100 in equation 2

**Equation 2 has been corrected to reflect this.**

L307-308 NCPprior represents an exponentially-weighted average over the residence time of O2 in the mixed layer (not over 30 days)

**Following the reviewer’s suggestion, we have edited this sentence accordingly.**

L378 I am not sure that you need "(positive downwards)" for the mixed layer depth.

**We have edited the Methods to remove these unnecessary clarifications.**

L519 I would remove "long-term"

**Edit made.**

L544-547 It is not entirely clear to me why short term deployments would overestimate rates of change.

**Short-term deployments would not necessarily overestimate rates of change – they could also underestimate the slope term if O2/Ar saturation showed a more gradual trend over a couple days as opposed to when evaluated over several days to a week. We have edited the text accordingly to express this more clearly:  
  
“This observation indeed suggests that short deployments (2-3 days) can produce differing rates of change in mixed-layer biological oxygen saturation anomaly compared to calculations using longer-term (weeklong or greater) measurements.”**L566-567 As well as vertical fluxes.

**This comment is somewhat unclear to us, as we do not imply here that vertical fluxes would necessarily result in productivity or rate of change overestimates. We have clarified this sentence with added explanation:  
  
“Alternatively, elevated productivity in the surface mixed layer might contribute to O2/Ar supersaturation despite the presence of a strong influence from vertical fluxes of low O2/Ar waters, leading investigators to take productivity rates at face value when they in fact represent underestimates.”**

L574-575 I do not agree with this argument. I do not think that there is a problem with the NCP-RT method per se, but with the need to accurately correct for vertical fluxes (affecting both NCP-prior and NCP-RT estimates).

**We can illustrate with a couple of cases that because the d(ΔO2/Ar)/dt term in NCPRT dominates the NCPRT calculation yet is influenced by not only vertical fluxes but also changes in wind speeds and biology prior to the deployment period, it may not reflect an accurate estimate of productivity over the measurement period - even in the absence of vertical fluxes. For instance:  
  
Case 1 (no vertical fluxes): Prior to the measurement period, wind speeds are slow and constant while net productivity is zero, although there is a residual positive oxygen signal present. Then wind speeds accelerate just before measurements begin. The mixed layer subsequently begins losing biological oxygen, leading to a negative d(ΔO2/Ar)/dt term. Resulting NCPRT is negative, when actual NCP over the period was zero.  
  
Case 2 (no vertical fluxes): Prior to deployment, actual NCP rates are constant, wind speeds are constant, and the oxygen signal is positive. Winds die just before measurements begin. The mixed layer accumulates biological oxygen, leading to a positive d(ΔO2/Ar)/dt term. NCPRT increases relative to what would have been measured during the previous period, when actual NCP did not change.**

L581-583 Again this does not make sense to me. NCP-RT-vflux should in theory reflect the net biological production or consumption of O2. If it doesn't, it is because of the inaccuracy of some of the terms in the equation. In principle the ΔO2/Ar/dt term shouldn't have large uncertainty. It is the vertical fluxes the ones associated with large uncertainty.

**To continue our above explanation, the d(ΔO2/Ar)/dt term is uncertain because it cannot distinguish between a slope in the oxygen signal that is due to changing biology or due to a lag in ocean-atmosphere equilibration in response to changing air-sea exchange conditions. The change in biological activity could take mixed layer productivity from autotrophic to heterotrophic and vice versa, but air-sea equilibration can only bring the ΔO2/Ar term to zero.**

L660-662 Wouldn't upwelling of undersaturated O2 water result in a positive flux?

**This is correct. The flux is negative at cycle P1604-2 due to upwelling of supersaturated waters. This portion of the discussion has been revised to elaborate on the discussion of vertical advective fluxes, however, and so the correction is no longer applicable as the abovementioned text has been removed.**

L721-725 But you are not using the model data to correct for physical advection.

**Yes, we did not attempt to correct productivity rate estimates for vertical fluxes because of the large magnitude and uncertainty associated with those fluxes. That said, we were able to assess the direction of their impact as well as their potential importance relative to the productivity signal. In response to the reviewer’s general comments above, we have also added more discussion regarding the plausibility of calculated advective fluxes and corrected productivity rates.**

L690-692 Is this method valid in coastal environments where there could be horizontal heterogeneity or other sources of N2O to the mixed layer? Would the fact that the mixed layer is shallower than the euphotic zone poses a problem?

**In response to the reviewer’s comment, we have mentioned that the method relies upon assumptions of negligible mixed layer N2O production and minimal horizontal fluxes.  
  
Regarding the potential differences between the mixed layer depth and the euphotic depth, the O2/Ar method, even under optimal conditions, has never been intended to estimate depth-integrated NCP down to the boundary of the euphotic zone – it reflects only mixed-layer depth productivity, and consequently can differ from euphotic depth-integrated production rates.**

L734-738 I would remove this from the conclusions as the authors didn't really tested the N2O method in this environment, and there are other approaches (such as the 7Be) that can be also used to estimate vertical fluxes.

**In response to the reviewer’s suggestion, we have edited this passage to better place our recommendation in the context of our findings. We do point out that the 7Be technique involves a different residence time relative to mixed-layer O2.**