

Biogeosciences Discuss., author comment AC2
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Reply on RC1

Melissa Ward et al.

Author comment on "Reviews and syntheses: Spatial and temporal patterns in seagrass metabolic fluxes" by Melissa Ward et al., Biogeosciences Discuss.,
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Our responses can be found in italics below. We have included the reviewers comments in our responses so that it is clear what we are responding to.

RC 1: Kroeker et al report on a meta-analysis of seagrass ecosystem metabolism from a selection of recent studies. Major goals of this study were to identify variations in ecosystem metabolism due to 1) seasonality, 2) regional differences, and 3) biological/thermal drivers, and determine the extent to which seagrass ecosystem metabolism may help to mitigate coastal OA. Unfortunately, this study suffers on two major fronts. First, the literature considered in this meta-analysis is very incomplete, and for some reason only considered dissolved oxygen-based studies (omitting all studies estimating NCP from carbonate system measurements). Secondly, the very simplistic (bordering on sloppy) approach to carbonate chemical accounting is troubling, given that the key aim of the meta-analysis was to address possible OA effects of seagrass metabolism. Because of these concerns, I find that the conclusions of the study are not supported by the analysis presented, making this work unsuitable for publication at Biogeosciences. To appropriately address the key questions of this study will require a complete re-compilation and analysis of the dataset, with an eye towards more appropriate carbonate chemical accounting.

Our choice to focus on oxygen metabolism studies was based on our interest in characterizing the variability in metabolism through time and space. We did not think there was one true "effect size" of seagrass on seawater chemistry – this is likely to vary for many reasons (i.e., species, health of the meadow, etc). Thus, we wanted to highlight how much of the variability could be explained by seasonal patterns in productivity, and how much these seasonal patterns varied among broad geographies. To do this, we did not use a standard meta-analysis with an effect size because most studies were reporting the similar response variables. Thus, we decided to limit our comparisons of apples-to-apples (e.g., studies measuring the same thing). Based on our searching of the literature and our decision to compare similar studies, we decided there were too few studies of seagrass NCP from carbonate system measurements that were easily comparable to other studies to adequately and appropriately synthesize in way that demonstrated the temporal and spatial drivers of variability.

However, we concede that there are lots of challenges in translating oxygen metabolism to seawater carbonate chemistry that just cannot be done with this dataset (e.g., too few studies report any of the other necessary variables such as changes in total alkalinity).

*Thus, to address the reviewer's concerns more broadly, **we propose to reframe the paper based on oxygen evolution studies, remove the simplified box model and only discuss the OA mitigation implications in the discussion as one aspect of the work.***

We appreciate the questions raised and the ideas presented by the reviewer regarding how we could improve our manuscript. However, we strongly encourage the reviewer to consider their tone and word choice in future reviews. This manuscript represents the hard work of several scientists over a few years – several of these scientists were graduate students during the work. Research demonstrates that comments regarding the quality of the work done by scientists (e.g., sloppy) are not actually helpful in improving the science and disproportionately negatively affect underrepresented groups (see Silbiger and Stubler 2019 and the associated commentary in Science).

RC1 136: The authors raise two concerns regarding the existing literature, 1) that no data exist for the N Pacific, and 2) no studies exist using direct DIC measurements. For 1), the authors should see Tokoro et al (2014) and more recent work from the group. Regarding point 2), there are in fact quite a few studies which have relied directly on DIC or DIC+TA measurements to quantify NCP in seagrass meadows. At least one of these studies (Van Dam et al., 2019) was included in this meta-analysis, and it is not clear why the authors chose to omit these and other DIC-based metabolism estimates. Others including DIC measurements include, but are probably not limited to: Perez et al 2018; Eyre et al., 2011; Ribas-Ribas et al., 2011, Dollar et al., 1991; Turk et al., 2015

Please see our comment above about our intention regarding characterizing the temporal and spatial variability in metabolic effects of seagrass on seawater chemistry and the limited number of studies measuring carbonate system parameters to adequately characterize drivers of the variability. We propose to reframe the introduction to focus on seagrass oxygen metabolism, remove the box model, and limit discussion of OA amelioration to a paragraph or two in the discussion.

RC1 143-145: There is another major conceptual flaw here regarding the treatment of data collected using different methods. It is unclear what the authors intend NCP to represent. Is this only a consideration of benthic community productivity? Or is it inclusive of water column processes as well? While some approaches may be direct metrics of water column + benthic NCP (mass balance or eddy correlation in some cases), others are very clearly metrics of only a single component of NCP. For example, benthic chambers explicitly exclude water column production, and are therefore only metrics of benthic productivity. Larger benthic chambers, which include greater water-column heights may be some combination of water column and benthic NCP. It is therefore hard to understand why all methods were combined, except for 'mass balance'.

Our intention was to be transparent about the variability arising from the different methods used to measure seagrass metabolism. Because of the focus on seagrass in particular, we are by definition trying to characterize benthic NCP. However, as the reviewer noted, some methods are more explicitly focused on the benthic component of NCP (i.e., small chambers) but these methods also have drawbacks (e.g., limited flow that influences the estimates, which has been discussed extensively in the literature). Rather than making a judgment call on which method is better, we decided to show the variability that resulted from the methods (Fig. S1). Because the estimates from the mass balance approach were much greater in magnitude and variability than other approaches, even those that also include some aspect of the water column NCP, we removed them from the variability analyses but kept them in the methodological comparison. All other methods could not be easily distinguished from each other visually.

RC1 168: Again, there are studies in this meta-analysis that measured changes in

seawater DIC directly.

We will remove this statement from the manuscript.

RC1 170-175: The goal of this study was to assess the ability of seagrasses to mitigate coastal OA. This is necessarily a question of carbonate chemistry variability, and as such, I find the DO-based approach used here to be highly suspect. Yes, if PQ and RQ are exactly 1:1, then a conversion of DO-based NCP to DIC-based NCP is appropriate. However, there is no reason to think that either PQ or RQ should be 1:1 in a seagrass meadow where a variety of processes consume/produce DIC (calcification, anaerobic metabolism) irrespective of Oxygen exchange. As such, prior measurements place PQ somewhere between 0.5-2.6 (Turk et al., 2015), and direct comparisons of NCP_DO and NCP_DIC show very weak correlations (Barron et al., 2006), certainly not 1:1 behavior (Van Dam et al., 2019). As a related factor, anaerobic metabolic processes in sediments can generate appreciable sediment-water TA fluxes in seagrass meadows. The impact of this on carbonate chemical buffering (thereby OA-amelioration) will depend on the stoichiometry and relative rates of the various anaerobic processes.

We agree with the reviewer that the PQ and RQ are unlikely to be 1:1, and we discuss this in the manuscript. Indeed, we report a wider range of potential PQ values than 0.5-2.6 in our own discussion (Line 499). To deal with this issue, however, we propose to refocus the manuscript on variability in O₂ fluxes associated with seagrass metabolism, remove the box model, and limit any discussion of potential OA amelioration to a paragraph or two in the discussion.