

Reply on RC2

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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Reviewer 2: This study aims to identify drivers of change within submerged vegetated habitats through a meta-analysis of seagrass ecosystem metabolism from studies reporting productivity using oxygen as a proxy for carbon fluxes. While this eventually could be a very useful paper, the manuscript has too many flaws to be accepted in its present condition.

(1) The study has omitted large parts of the literature on seagrass productivity, there are several other, very important studies, many reporting O₂ dynamics (especially in the older literature). And why report only oxygen evolution studies? There are also reports from other methods, like e.g. Tokoro et al that measured CO₂ directly, or in situ PAM work, like e.g. Gobert et al 2015, and references therein.

*We considered the full range of papers on seagrass metabolism, but because our intention was to characterize **the variability** in space and time of seagrass metabolism, we could not find enough studies using other approaches (i.e., measuring CO₂ directly) to characterize the variability aspect. We did not include approaches that only measured leaf-scale productivity (i.e., PAM) because we were interested in community metabolism associated with seagrass meadows and their associated community.*

Regarding the older literature, it is possible that some studies might have been missed using our search terms while some could have been excluded because they did not meet our criteria for inclusion (which were set to try to increase comparability between studies that we synthesized). We have reported our search terms in the paper, but we propose to include a more comprehensive table that outlines which papers were found using our search terms, which were excluded and for what reasons, in the supplementary materials.

(2) The comparison between enclosures and other methods is tricky. At lines 124-126 the authors write: "In contrast, measurements of NCP taken over longer time periods or that incorporate the full 24 hour cycle (full-day NCP) provide insight into the cumulative effect of seagrass on seawater chemistry"

This might not be true (for enclosures) since it has been shown that incubations as long as 24h might yield very low values as the chamber effect will be considerable. Thus these studies might have been severely underestimating productivity. (Olive et al 2016)

We agree with this interpretation. However, we decided that transparency was the best

option for characterizing the variability associated with the different methods, and as such, included the plot that showed the differences across the different methods. Visually, we couldn't clearly distinguish between methods except for the mass balance approach. We propose to include more discussion of the limitations of the incubation approach in the discussion.

(3) Overall the authors neglect to account for any changes in seawater carbonate chemistry. Especially calcification (very important in many tropical areas) is forgotten. The formation of CaCO_3 have been shown to decrease pH and force CO_2 from the water to the atmosphere. However, some researchers suggest that the net effect of increased productivity and calcification has a positive effect on the overall productivity within the system. This must be discussed properly, a good starting point for such a discussion could be found in e.g. Gattuso et al 1995, Frankignoulle et al 1995 and Mazarrasa et al. 2015.

While we appreciate this suggestion, we accept that there may be too many issues to contend with to adequately translate from oxygen evolution studies to OA amelioration using this dataset. Thus, we propose to reframe the manuscript on seagrass metabolism and oxygen evolution and limit discussion of OA amelioration potential to one or two paragraphs in the discussion.