

Interactive comment on “The response of seagrass (*Posidonia oceanica*) meadow metabolism to CO₂ levels and hydrodynamic exchange determined with aquatic eddy covariance” by Dirk Koopmans et al.

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I (DK) would like to apologize to the referee for my delay in submitting this response from all of the authors. We are grateful to the referee for their continued thoughtful contributions to this manuscript. They introduce points that we were also concerned about in the development of this study, and points that we had not considered. We address the rationale for our approach, and introduce improvements that can be made to the manuscript below. The referee's suggestions follow in italic.

C1

This assumption [that biomass is proportional to meadow height] is only valid if shoot density is constant. However, this is never the case as shoot density is notoriously variable in time and space (Gobert et al. 2003; Mayot et al. 2006; Terrados and Medina-Pons 2011; Vasapollo and Gambi 2012). Hence, in absence of shoot density data, the authors do not have any grasp on the biomass differences among the two sites. In absence of this information, the authors cannot conclude if the differences in O₂ fluxes are due to different biomass or a response of primary production to the presence of a CO₂ vent.

We agree that biomass measurements would be a valuable addition to this study. However, as the referee states, shoot density is notoriously variable in time and space. Biomass is even more variable, and the greatest variability is observed at the smallest measurement scale, i.e., quadrats (Vasapollo and Gambi 2012). Because eddy covariance integrates oxygen fluxes over larger spatial scales, a large number of biomass measurements would be required to sufficiently characterize biomass within the footprint. Dive time was limited, so this would reduce the number of eddy deployments.

Our rationale in pursuing eddy covariance deployments to the exclusion of biomass measurements was that the effect of the CO₂ vent on seagrass productivity can be revealed from eddy covariance measurements alone. Many prior studies have examined seagrass biomass at CO₂ vents (e.g., Hall-Spencer et al. 2008; Apostolaki et al 2014; Takahashi et al. 2016). In these studies, biomass was quantified to examine the effect of the CO₂ vent on seagrass net primary production. Our study used oxygen fluxes to quantify net primary production directly. Implicit in the design of the experiment is that the effect on seagrass productivity is due to the CO₂ vent. The same implicit assumption was relied on for the above studies.

C2

We agree with the referee in an important respect. We saw that net seagrass meadow primary production was low at the CO₂ vent, but without biomass we cannot tell if gross primary production was elevated within leaf tissues at the CO₂ vent. We will now include in the discussion the possibility that biomass-normalized gross primary production was elevated. However, in the absence of an understanding of the vertical distribution of photosynthetic production within these meadows, normalizing by biomass may be a mistake. Dalla Via et al., (1998) found that 50-60% of light was attenuated by horizontal fronds at the top of a *P. oceanica* canopy. In *T. testudinum* meadows 34 to 90% of irradiance was attenuated in the top 20 cm (Enriquez and Pantoja-Reyes 2005). Therefore, it is likely that the top of the meadow contributes disproportionately to primary production. To our knowledge, the vertical distribution of photosynthesis has not been resolved in a seagrass meadow. However, as the fraction of downwelling irradiance absorbed by a meadow increases from 20 to 90%, biomass-normalized photosynthesis declines three-fold (Zimmerman 2003). Zimmerman (2003) also found that canopy height is a good predictor of irradiance absorbed. Thus, we would expect biomass-normalized photosynthesis to be greater in the short meadow at the CO₂ vent than at the taller meadows at Elba due to differences in canopy architecture alone. In this way, normalizing by biomass may obscure, rather than reveal, the effect of the CO₂ vent on seagrass meadow productivity. Instead of normalizing by biomass, our approach was to normalize by area. This approach avoids the confounding effects of canopy architecture on biomass-normalized seagrass meadow primary production.

*This [assumption that if biomass is proportional to meadow height, the gross primary production of the CO₂ vent is similar to that of the open-water meadow] was exactly my point in my initial review. If biomass normalized GPP is similar among sites, then you cannot conclude on nutrient limitation due to the CO₂ vent. The conclusion is that changes in pH do not affect the productivity of *P. oceanica*, in line with pH manipulation experiments (Cox et al., 2016).*

C3

We agree with the referee's fundamental suggestion that given the low meadow height, gross primary production may be elevated within leaf tissues at the CO₂ vent. We find, however, that nutrient limitation is one of the reasonable explanations for reduced meadow productivity at the vent. Nutrient-limited *P. oceanica* meadows add biomass when nutrients are added (Alcoverro et al., 1997). Between nutrient-limited and nutrient-replete conditions, seagrass biomass may double (Powell et al., 1989). Therefore, biomass-normalized productivity at nutrient-limited and nutrient replete meadows may be confounded by biomass. The dependency of biomass-normalized primary production on canopy height (Zimmerman 2003), makes a comparison of biomass-normalized primary production across these sites potentially misleading. Under these circumstances, area-normalized measurements offer advantages over biomass-normalized measurements for identifying suppressed meadow productivity. We will address this gap in our manuscript by justifying area-normalized measurements in the introduction.

A change of 15 $\mu\text{mol L}^{-1}$ is actually quite a large change in total alkalinity... Hence, in absence of data to constrain production/dissolution of CaCO₃ in the study sites, the authors cannot fully account for "metabolism." They can provide information on organic carbon metabolism given by O₂ fluxes, but leaving out the inorganic carbon metabolism has been shown to be relatively important in these ecosystems (Barron et al., 2006).

We thank the referee for making this point and for their calculations to estimate the contribution of inorganic carbon to metabolism. We agree with the referee and regret that we did not address this point in our discussion. Our terminology and assumptions follow convention (e.g., Duarte et al., 2010), but production and dissolution of CaCO₃ could indeed enhance total primary production by perhaps one-fourth. We will

C4

include this point, and rationale for the magnitude of this contribution, in our discussion.

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