

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.

Anonymous Referee #1

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I thank the authors for taking into account my suggestions, and for replying in detail to my comments. I would like to react on a couple of points from the reply.

Authors' reply: "If we assume that biomass is proportional to meadow height, (...)"

This assumption is only valid if the 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 & Medina-Pons 2011; Vasapollo & Gambi 2012). Hence, in absence of shoot density data, the authors do not have any grasp on the biomass

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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 the CO₂ vent.

Authors' reply: "the gross primary production normalized for biomass at the CO₂ vent is similar to that of the open-water meadow"

This 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).

Authors' reply: "According to Frankignoulle (1986) diel alkalinity changes in seagrass meadows can be 15 mmol L⁻¹, or 0.6% of seawater alkalinity. This small change in alkalinity has an insignificant effect on DIC calculation from pH (Lewis et al., 2008)"

(I assume there's a typo and the alkalinity diel change is 15 $\mu\text{mol L}^{-1}$ instead of 15 mmol L⁻¹).

A change of 15 $\mu\text{mol/L}$ is actually quite a large change in total alkalinity at daily scale that can be assumed to be related calcification from epiphytes and/or dissolution of carbonates, based on Barron et al. (2006). If we assume this, then the related change in DIC is TA/2 (Smith and Key 1975). Based on figures, the observed diel change of O₂ was about 40 $\mu\text{mol/L}$ which translates to a change of DIC of about 30 $\mu\text{mol/L}$ due to photosynthesis/respiration (O₂:DIC = 138:106). Hence the change of alkalinity of 15 $\mu\text{mol L}^{-1}$ due calcification/dissolution of carbonates would translate to a change of DIC of 7.5 $\mu\text{mol/L}$, equivalent to 24% of the expected change of DIC due to photosynthesis/respiration based on the diel signal of O₂ (20 $\mu\text{mol/L}$) reported by the authors. This is not an insignificant effect.

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

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on organic carbon metabolism given by O₂ fluxes, but leave out the inorganic carbon metabolism has been shown to be relatively important in these ecosystems (Barron et al. 2006), in line with the simple calculations give above.

References

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