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Biogeosciences Discussions

Interactive comment on "The response of seagrass (*Posidonia oceanica*) meadow metabolism to CO_2 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

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 μmol L-1 instead of 15 mmol L-1).

A change of 15 μmol/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 μmol/L which translates to a change of DIC of about 30 μmol/L due to photosynthesis/respiration (O2:DIC = 138:106). Hence the change of alkalinity of 15 μmol L-1 due calcification/dissolution of carbonates would translate to a change of DIC of 7.5 μmol/L, equivalent to 24% of the expected change of DIC due to photosynthesis/respiration based on the diel signal of O2 (20 μmol/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

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.

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