

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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General comments

The paper by Koopmans et al. seeks to address an important scientific question and is within the scope of Biogeosciences. The scientific methods are clearly outlined, and the authors use state-of-the-art methods with clear descriptions of data treatment. Authors give credit to previous work and highlight their own new/original contribution. Overall presentation is well-structured and clear, and the length of the paper is appro-

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ropriate for the dataset. Language is fluent and precise.

The core dataset of the paper consists of benthic oxygen flux measurements. Aquatic eddy covariance (AEC) oxygen fluxes were quantified at 5 shallow sites nearby the Mediterranean islands of Elba and Panarea: three seagrass beds (open-water, nearshore, and CO₂ vent) and two sites with bare sands (nearshore and CO₂ vent). Flux datasets for the individual sites range in duration from 15 h (CO₂ vent bare sands) to 58 h at the nearshore seagrass bed. Based on these datasets, the authors resolve gross primary productivity (GPP), respiration (R), and net ecosystem metabolism (NEM) at each site, and then draw conclusions about metabolism in seagrass and bare sediments in relation to their environmental setting (CO₂ levels, hydrodynamic exchange). This dataset is rather limited for the study question being investigated, but I believe there are sufficient novel elements within the data, as well as within the new data processing tools the authors present, to warrant its publication.

Specific comments & technical corrections

Title

Title is clear and reflects the approach taken in the paper. I would suggest adding 'light availability' to the title, e.g. "...response of seagrass meadow metabolism to CO₂ levels, light availability, and hydrodynamic exchange..."

Abstract

L13-15: It would be useful to give some indication of actual rates.

L14: This sentence seems to contradict itself. Perhaps, simply: "Thus, *P. oceanica* meadows are oases of productivity."

L17: "Oxygen depletion and replenishment within the meadow does not contribute to turbulent O₂ flux" This needs to be clarified. Clearly, this process affects the turbulent O₂ flux as resolved using the AEC, mostly by 'dampening' the flux signal (Fig. S1). Perhaps: is not captured by turbulent fluxes measured above the canopy?

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Methods

L86-87: Study site descriptions. For future studies I would recommend considering biodiversity aspects more carefully. Meadow height and coverage are of interest, but quantifying shoot densities, animal and plant biomass, and presence of ephemeral algae, for instance, would go a long way with helping to better interpret the resolved rates of metabolism.

L87: Please add daily integrated PAR, or daily average PAR to Table 1 or 2. Otherwise it is very difficult to interpret GPP values at the different sites.

L99: Should read “10s of m²”

L132-133: “. . .do not contribute to fluxes above the meadow”. They do, otherwise you wouldn’t measure a dampened flux. It is essentially a “missed flux”; a flux that is not captured by AEC measurements above the canopy.

L140: Is there another way to phrase this, instead of ‘negative production’? Consumption reflects (secondary) production.

Results

L174-176: How were these incubations performed? Presumably only on parts of the leaves?

L180: Should read “. . .overlying seawater”.

L186-187: Is this referring to photosynthetic production or to net O₂ flux? That is, is this difference due to actual decreased photosynthetic production, or is it due to higher photosynthesis-coupled respiration in the afternoon?

L195: GPP values in Table 2. What explains the difference in GPP from one day to the next at the open-water and nearshore seagrass meadows? Light availability, perhaps? It would be informative to have daily integrated PAR values (e.g. in mol photons m⁻² d⁻¹) for day 1 and day 2.

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L213-214: “In none of the meadows Pmax was reached” needs to be rephrased.

L215: “Ik varied was one-third. . .” rephrase.

Discussion

L230: Typo- should read “greater”

L234: I suppose that differences in above- vs below-ground biomass, i.e. the ratio between photosynthetic and non-photosynthetic tissue can be different for different species of seagrass (e.g. Duarte and Chiscano 1999 Aquatic Botany). Furthermore, it is important to keep in mind that eddy fluxes represent habitat-scale fluxes, and not just seagrass respiration. Animals, for instance, will contribute through respiration and bioturbation.

L252: “Epifauna biomass. . .” Presumably autotrophic epiphytes would contribute to the eddy flux signal also?

L258-259: This conclusion is based upon a ‘snapshot’ dataset. Without investigating this in more detail (e.g. a seasonal study), it may come across as a little premature. It should be stated clearly that these results are specific for the period of investigation.

L265: “. . .but the negligible NEM suggests that this meadow was not storing organic carbon.” It really depends on how production and respiration are partitioned within that habitat. This statement suggests that all of the new production by the seagrass is consumed, but seagrass C:N typically is high, so what is consuming all of that biomass? Presumably these plants are growing and are shedding leaves on an annual basis. There exist other sources of organic matter than the seagrass themselves. One alternative theory could be that seagrass $GPP > R$, but R is stimulated by sediment entrapment, resulting in a $GPP \approx R$.

L287: “. . .hydrodynamic exchange with surrounding waters is limited.” Again, this is based on a small dataset, and was not observed at the other sites. ‘Can be limited’, perhaps?

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L325: “These meadows had high productivity. . .” Is this referring to NEM? If so, this needs to be specified.

L329-332: As I understand it, the point being made here is based upon a single flux dataset (the one that required O₂ storage correction). The other datasets did not require this correction, and thus (presumably), this functional adaptation applies only to this one site. However, GPP and NEM rates at the nearshore seagrass sites (no storage) were comparable or higher than the rates observed at the offshore site, which seems contradictory.

Figures

Figure 3: Typo in units for PAR (should be $\mu\text{mol photons m}^{-2} \text{ s}^{-1}$)

Figure 5: Typo in units for PAR (should be $\mu\text{mol photons m}^{-2} \text{ s}^{-1}$)

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