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Biogeosciences Discussions

Interactive comment on "Dissolved organic carbon release by marine macrophytes" *by* C. Barrón et al.

Anonymous Referee #3

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General Comments:

This manuscript seeks to combine scattered measurements of flux rates of DOC from seagrass beds (in the Phillipines, in Spain, and in Florida) with a literature review of DOC flux rates reported from seagrass beds and macroalgal beds. The ultimate synthetic goal of the authors is to combine this data to derive distributions of DOC flux rates and ultimately scale this data to provide estimates of flux of DOC from these systems worldwide.

Generally speaking, this approach fails to derive accurate global estimates of DOC flux. The new data are quite sparse and are synthesized with published rates poorly. The final analysis oversells the conclusions which might be drawn from these data: it is inappropriate to calculate a global mean and variance of areal DOC flux from ben-

thic chamber experiments conducted in many different habitats without a cohesive way to propagate error and incorporate biases from differences in experimental design. Moreover, the authors have no consistent approach to how chambers are measured, doing such things as presenting multiple independent measures over successive days as equivalent independent measures as mean values derived from other studies conducted from multiple replicate chambers. As I recommend below, I suggest that the authors remove from the title, abstract (final lines) and discussion/results (final paragraph) the work seeking to scale their analyses to global flux of DOC from seagrass and macroalgal beds.

However, the additional metanalyses done in this paper, including analyzing relationships between temperature and DOC flux, analyses of the distribution of flux rates among studies, and the relationships between DOC flux and GPP/R/NCP are very useful and I believe sufficiently well-analyzed to allow the reader to draw meaningful conclusions. In addition, the comparative analysis of seagrass beds with macroalgal beds is useful because, despite the inaccuracies in the absolute magnitude of DOC, it allows a rough conclusion that macroalgal beds release significantly more net DOC than do seagrass beds, either due to decreased heterotrophic activity (likely in sediments) or to increased productivity and shunting of carbon to exudates.

The new data on community DOC flux from various habitats are appropriately collected and a valuable contribution. However, the methods are very sloppy, and unacceptable as currently presented. I have made specific comments below to clarify the time frames of DO change measurement, the raw DOC concentrations, and to reconsider how volume differences in the chambers are accounted for and might have affected changes in DOC. In addition there is limited metadata presented on the locations or status of the seagrass beds analyzed, which is inexcusable and should be corrected (as other reviewers have noted, temperature is not even included in Table 1). Aside from minor technical issues (discussed below) I think that the data are worth publishing, though clearly not groundbreaking.

Specific Comments and technical corrections:

- Please change the title; the current title implies that this paper does a thorough analyses of DOC release by marine macrophytes when it does not.

- The final sentence in the abstract should be removed - the authors have provided no reason for readers to believe that these estimates are comprehensive, accurate, or justifiable in any way, and without that numbers like this are dangerous in an abstract because they can be so easily misused.

- No raw DOC concentrations are provided. In this age it would be appropriate to provide tables of raw values so that readers could assess contamination issues and also see the magnitude of DOC releases which were being measured before normalization for volume (problematic in its own right) and surface area of the substrate.

Table 1,2 - I believe that "Unpublished" means that the data are reported in this study. I recommend that Unpubl be replaced with "this study" in these tables.

Fig 1 - The histogram bins should be made smaller, perhaps by a factor of 4. This will give a far more informative idea of the variance for the reader.

Fig 2 - How was this figure produced when so little temperature data is presented in Tables 1 and 2? I recommend eliminating this figure, or at the very least noting clearly here and in the results that only a handful of the studies in Table 1 were used in the analysis (refs 3,5,6,7 only). More importantly, the authors' own additional measurements do not appear to include temperature ancillary data, which eliminates them from the analyses.

Fig 3,4 - lines are both solid?

Fig 4 - report exact p-values please in the methods

Fig 5 - This figure should be removed and the results should simply state that there is no significant relationship.

Fig 6 - Remove minor tick marks from axis - misrepresents the axis as continuous when it is categorical.

1532 P1 - present the literature review at the end of the methods in a separate section (in other words, separate experimental and review methods)

1532/7 - How long after cementing were the chambers allowed to acclimate?

1533/4 - exact times of the incubations are necessary

1533/10 - what was the thickness (mil) of the plastic on bags?

1533/28 - concentrations depend on volume of chamber (ranged from 5-20L) and amount of seagrass. How would you normalize for this in rates of change, as rates of concentration change will depend on starting concentrations (concentration dependency)? It would be better to calculate flux rates by correcting for volume disparities. How did other studies accomplish this? Can you compare your rates with other studies on this basis?

1534/2-4 - What does this statement "DOC samples retrieved subsequently..." refer to? The samples from the chambers were handled one way as stated in previous sentence, so what does this mean?

1534/13 - Precision estimates for the instrument? Stability? HTCO TOC systems are notoriously imprecise and require careful and constant calibration and stability assessment.

1534/15 - it is not clear how "hourly" rates were estimated. Over what time frame? Were these instantaneous rates (measured over just a few hours in day and night) or were these diel changes (ie differences from one dawn to the next)? Much more detail must be provided.

1537/11-13 Is this a correlation or a regression? Be clear.

1537 P2. This paragraph is poorly constructed. First off, neither system showed a relationship between shading intensity and flux rates. Say this first. Then move into a presentation of the nuances of the Phillipines experiments: 1) net DOC source under all irradiances after 2 days (save one), 2) net DOC consumption under all irradiances (save 2?) after 6 days, 3) Wilcoxon test to show fluxes differed between day 2 and 6.

1538/P1: See the work by Haas et al (2011, PLoS ONE) for accurate estimates of DOC release as a proportion of primary production without using 14C (ie separate measurements) and incorporate into this discussion. Various of these authors (Esp. Haas, but also Naumann and Wild) have published rates of macroalgal DOM release. ALthough these are not community release rates they are still informative to contrast. I believe that Haas et al. 2010 (MEPS) also has seagrass release rates.

1541/24-end - As noted above I patently object to the scaling up to global DOC release rates from macrophyte/macroalgal communities. These are small scale studies and should not be scaled as such beyond a comparative approach (such as that done in the preceeding sentences to compare seagrass beds with macroalgal communities). Certainly it is inappropriate to propose that this DOC is advected as export from coastal habitats or to imply that it is not immediately recycled locally without better evidence. I would remove this statement also from the abstract.

Interactive comment on Biogeosciences Discuss., 9, 1529, 2012.