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Biogeosciences Discussions

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

C. Barrón et al.

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Reviewer 1: This manuscript by Barrón et al. estimates the global release of DOC by marine macrophyte communities (seagrasses and macroalgae beds), based on a compilation of literatura data and some unpublished work by the author(s). In addition, they attempt to determine driving factors which regulate the amount of DOC release in individual communities by examining relationships between DOC release rates and temperature, gross primary production, net community production, and light conditions.

Reviewer 1: * While of potential interest, I have a number of concerns that make me only lukewarm about this manuscript and its added value. -The global estimate of DOC release by marine macroalgae is based on 7 estimates, which range over an order of magnitude (8.4 to 71.9 mmol C m-2 d-1), and of which 6 are new, unpublished data. I

have a number of problems with this. While I understand that in the absence of more data, we have to go with what is available but it seems a rather small dataset to base any global estimates on. In addition, only 1 of these 7 datasets is well described (Maher & Eyre, 2010), for the authors' unpublished data the only information we have is the species/site involved. To evaluate the appropriateness of using these data, readers should have more precise information on the data used: sites, period of the year when these experiments were run, depth, density of the vegetation etc.

Author comment: We agree that we must provide additional details on the upubl. Data reported in this table. We have now added a "comments" field to table 2 (DOC release by marine macroalgae) where we provide necessary details, including the depth and time of the year when the incubations were run.

Reviewer 1: * Also, it is unclear how the data were extrapolated to an annual basis: the authors mention in the Methods section that rates were converted into daily rates based on number of hours of daylight and night – but is this over the period when the experiments were run, or somehow extrapolated to the entire year ? Were these data collected in a single period per site, or is it the integrated value for measurements taken throughout the year ?

Author comment: We concur that additional detail is needed and that the limitations of the estimates need be acknowledge. Indeed, our aim was to use the empirical basis available, which we acknowledge to be thin, to derive a first order estimate of the possible magnitude of the global fluxes. Shall this first-order estimate indicate that this flux is globally-relevant, as it is, we expect that this will encourage others to contribute to better understand and quantify this process. At the end of section 2.1 Net DOC fluxes from marine macrophytes in the method section we have added "A first order approximation of the global net DOC flux from marine macrophyte communities were estimated using the average net daily DOC release by seagrass meadows and macroalgal communities estimated from published and unpublished sources synthesized here multiplied by 365 days and the area of seagrass meadows and macroalgal communities. We acknowl-

edge that this estimate bears considerable uncertainty, as the data base available is still limited, both in terms of the total number of communities investigated, their geographic spread and the capacity to represent an annual flux. Yet, the estimate derived here provides a first-order estimate sufficient to assess whether macrophyte-derived DOC maybe a globally significant C flux in the ocean, thereby helping raise awareness on the importance of this process. ”

Reviewer 1: * This brings me to another important issue in the way data are used in the upscaling effort. The authors should standardize the way individual data/estimates are defined. For the authors' own data, measurements made at the same site during different months are treated as separate datapoints (e.g. Posidonia oceanica data for Magaluf Bay, Table 1), whereas for literature sources only the annually intergrated values are used, even if the data are available on a seasonal basis (Maher & Eyre 2010, for example, as used in Table 1 and Table 2). This implies a bias in your dataset. At first sight, it would make more sense to use annually integrated values when upscaling DOC release rates to the global level, or use the individual datapoints (site/species/time of experiment) when examining relationships between DOC release and temperature, GPP, NCP, etc? On page 1541, the authors mention the previous estimate of DOC release by seagrasses is based on 11 datapoints; while this new estimate is based on 66 datapoints – this of course is somewhat of a misrepresentation in light of the above comment.

Author comment: As suggested by reviewer, we agree that it would be best to calculate the average DOC fluxes when we have more than one estimate from the same site. For instance, we will average the DOC fluxes form the seasonal study run in Posidonia oceanica in Mallorca and Greece, and DOC fluxes from Cymodocea nodosa along a colonization gradient. We will use weighted averaged values to upscale to provide first-order estimates of global DOC fluxes, but will use individual observations to explore the relationship between DOC fluxes and possible drivers, such as community metabolism. This will imply that the number of estimates used for the global upscaling would be 28,

whereas the total number of individual estimates would 66, as shown in Table 1.

Reviewer 1: * I am a little surprised when looking at Table 1 that temperature data are missing for so many of the authors' own experimental data. Given that during the experiments, changes in O2 were also measured – this should mean that temperature data are also available (mentioned in Barron et al. 2004) ? If we look at Figure 2, and put some rough estimates on the data for which temperature is not listed in Table 1, the resulting Figure would look a lot less attractive, and I doubt the relationship would still hold. Consider again the Cymodocea nodosa data (Barron et al. 2004) which were all done on the same site and season (different stands with different age): we can assume the temperature range is quite limited but the data span a wider range than now given on the Y-axis of Figure 2. Even an estimated water temperature would suffice to show that the relationship given in Figure 2 would no longer hold when including all data. Not sure if the other references have presented temperature data, if not it would be worth asking.

Author comment: We agree that we should provide temperature for as many observations as possible. We have been able to obtain the water temperature corresponding to the benthic chambers run in Bolinao, the Philippines (Gacia et al. 2005) and Delta del Ebro Spain (Llebot et al. 2011). Adding these additional temperature data to table 1 we obtain this regression line Net DOC flux (mmol C m^{-2} d^{-1}) = -19.3 (± 16.3) + 1.5 (± 0.7) T ($^{\circ}$C), r2 =0.1, p<0.05. Although much weaker than the original relationship, this relationship is still statistically significant. We will removed this figure and we will show the r2 =0.1, p<0.05 in the results section.

Reviewer 1: * Table 1 gives range of -53.6 to +81.6 mmol C m-2 d-1 for Cymodocea nodosa data (Barron et al. 2004), the original reference gives a range of -66.2 to +81.6 mmol C m-2 d-1; please check.

Author comment: The net DOC release of -66.2 mmol C m-2 d-1 in Barron et al. 2004 includes the DOC fluxes from unvegetated sediments, which are not relevant here. The

mean value provided here represents that derived from data in Barron et al. 2004 when excluding unvegetated sites, as Table 1 is restricted to DOC fluxes form macrophyte communities.

Reviewer 1: * Figure 3: please check or clarify the following: panels A and B do not seem to correspond very well, although as far as I understand (correct me if I'm wrong) the distribution in panel B is based on data in panel A. Panel B mentions 9 datasets with a value lower than 1, i.e. DOC flux light > DOC flux dark. If I draw a 1:1 line on panel A, however, there would appear to be many more points (around 15 I would estimate). Also the number of datapoints in panel A (45-50 ?) appears to be much higher than the sum of observations given in panel B (37).

Author comment: We will insert in the figure caption the number of estimates used in figure 3a to estimate the linear regression in seagrass and macroalgal communities. We will also explain in the method section that in order to estimate the frequency distribution of the ratios between net DOC fluxes in the light and the dark we used only ratios that were positive. This means that only when both net DOC fluxes (under light and dark conditions) were positive (i.e. there was a net release) or negative (there was a net uptake) the ratios were calculated and used in Figure 3b. This implies that when a seagrass meadows had a net uptake and a net release in light and dark conditions (or viceversa) these negatives ratios were not used in the figure 3b. This explains the difference in the number of observations in figure 3a and figure 3b. We will explain the reduction in the number of observation in the figure caption of fig. 3b.

Reviewer 1: -Figure 4: why present all the linear regression lines if they are not significant ? -Figure 5: dito: if not significant, why present a linear regression line ? I do not even see a reason/mechanism to expect a linear relationship between the two parameters.

Author comment: We agree, and will remove the linear regressions that are not significant from both figures.

Reviewer 1: * Minor comments -P1530 L7: all most: almost -P1530 L10: seagrass meadow: seagrass meadows -P1530 L11-12: "this relationship": be specific. -P1531 L4: "the release of dissolved organic matter [: : :], although the form, particulate or dissolved, of this release has not been resolved". Something not right there.

Author comment: We will correct both spelling mistakes in the revised manuscript. The sentence will be changed to "The export of organic matter has been reported to account, on average, for 25% and 44% of the net primary production of macroalgae and seagrass, respectively (Duarte and Cebrián, 1996), although the form, particulate or dissolved, of this release was not resolved."

Reviewer 1: * The authors should be more careful in choosing the number of decimals used to present their data. A striking example is on P1541L11 where the global average net DOM release (this should be DOC release, by the way) is cited as 4047.85 _ 967.25 mmol C m-2 y-1, etc. Obviously those decimals are not quite appropriate. Another example are some of the temperature data mentioned in Table 2 , temperature values with 2 decimals for experiments run over several hours – I don't see the point of this; also some given with 0, 1, or 2 decimals.

Author comment: We agree. We will homogenize the number of decimals and we will use just one decimal along all the estimates shown in the manuscript.

Reviewer 1: * Table 2 caption: "negative values represent : : :": there are no negative values in this Table.

Author comment: The reviewer is correct, there are not negative values in this table. This sentence will be removed.

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