

Interactive comment on “Dissolved organic carbon release by marine macrophytes” by C. Barrón et al.

Anonymous Referee #1

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Barron et al. Dissolved organic carbon release by marine macrophytes

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by marine macrophyte communities (seagrasses and macroalgae beds), based on a compilation of literature data and some unpublished work by the author(s).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.

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,

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which range over an order of magnitude (8.4 to 71.9 mmol C m⁻² d⁻¹), and of which 6 are new, unpublished data.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 In addition, only 1 of these 7 datasets is well described (Maher & Eyre, 2010), for the authors' unpublished data the only information we have is the species/site involved.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. 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?

-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 integrated 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.

-I am a little surprised when looking at Table 1 that temperature data are missing for

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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.

-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.

-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).

-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.

Minor comments -P1530 L7: all most: almost

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-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.

-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.

-Table 2 caption: "negative values represent . . .": there are no negative values in this Table.

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