Biogeosciences Discuss., 9, C2974–C2980, 2012
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Biogeosciences Discussions

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

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Received and published: 6 August 2012

Reviewer 2: * This paper attempts to quantify the DOC flux from coastal macrophyte (seagrass and macroalgae) communities, and the importance of light, community metabolism and temperature in controlling the flux, using new and previously published data. This area of research is important and has received relatively little attention (for example the authors only list one previous estimate of DOC flux from macroalgae communities). While I like the idea of this paper there are several issues that need to be resolved. My main concern is the rather haphazard way a global value for the macrophyte DOC flux is derived. Firstly, there are essentially only a couple of estimates for macroalgae, making any kind of global upscaling effort somewhat dubious.

Author comment: Reviewer one is also concerned about the global DOC fluxes esti-

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mated. We will calculate the average DOC fluxes when we have more than one estimate from the same sites. Thus, the number of estimated for the upscaling will be 28 instead of the 66 individual estimated.

Reviewer 2: * Secondly just averaging all the values in Table 1, and assigning the error based on the averages, when the errors for these averages are presented is wrong. For example, if I just calculate the average of the errors (as what was do one to estimate the average DOC flux), the average error is _ 7 mmolC m -2 d -1 or by calculating the proportional error for each individual measurement (i.e. error/mean) and average these for all the individual measurements, the proportional error is _0.53 or 53%. This leads to a much larger error than that presented (_6 mmolC m -2 d -1). A better way to estimate the error is through standard propagation of error methods.

Author comment: We agree, the standard error (SE) shown is wrong. We will carry out the error propagation analysis to the DOC fluxes estimates shown in table 1. First, as suggested by reviewer 1, we will use just one estimate per site leading to the use of just 28 estimates for the first-order approximation of the global net DOC flux. Then we will calculate the standard deviation, as we know the number of replicates use for each average (3 o 4 replicates except for Maher and Eyre data where they used 12 replicates). Then we will use error propagation technique to estimate the standard error of the average of DOC fluxes that will be shown in Table 1, and from this derive the standard error of the estimate. Also the SE of the average net DOC flux from macroalgal communities is wrong, and will be re-calculated and reported using error propagation. In table 1 and 2 we will show the weighted average (as described in a previous reply) and the SE calculated with the error propagation method.

Reviewer 2: Another issue is how are these daily rates scaled to annual rates? If they are simply just multiplied by 365, I think this is a serious error, particularly in light of the relationships presented in 2, 4, and 5. The upscaling to annual rates needs to be better explained.

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Author comment: We will clarify in the method section the upscaling of daily rates to annual rates and at the end of the section 2.1 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 acknowledge 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 2: * In Table 1 and Table 2 why is there temperature data missing from the authors own published and unpublished data? Is this left out because it will render the temperature versus DOC flux regression presented in Figure 2 insignificant. Please present this data or explain why it is excluded.

Author comment: We have obtained from the literature 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). Now regression line in Figure 2 is Net DOC flux (mmol C m$-$2 d$-$1) = $-$19.3 ($\pm$16.3) + 1.5 ($\pm$0.7) T (°C), r2 =0.1, p<0.05. Although much weaker than the original relationship, this relationship is still statistically significant. We have removed figure 2, as requested by reviewer 3.

Reviewer 2: * Specific Comments P1530 L7 All most should be almost P1531 L4-L9 This sentence does not make sense, you start by saying the release of dissolved organic matter accounts for : : : then you say that the form of this release (i.e. particulate or dissolved) is unknown. Please check P1532 L15-L16. This paper does not present the first assessment of global DOC flux from macrophytes as Maher and Eyre 2010 previously estimated this.

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Author comment: We will correct all these errors in the revised manuscript.

Reviewer 2: * P1533 L28 to P1534 L5. There appear to be two sets of sampling protocols used here, please specify which experiments used which protocol (acidified and kept at room temperature, versus frozen)

Author comment: We will clarify in the method section the DOC protocols used and we will add this sentence: "DOC samples collected from March 2001 to May 2001 were kept frozen in acid washed material (glass vials encapsulated with silicone-teflon caps) until analyses. DOC samples collected subsequently were kept acidified with 2 N HCl at room temperature in acid-washed sealed ampoules, a procedure which improved accuracy in DOC determinations".

Reviewer 2: * P1534 L7 I assume the 2 should not be here?

Author comment: This is indeed an error. It should read: "DOC standards provided by Dennis A. Hansell and Wenhao Chen (University of Miami) of 44 - 45 μmol DOC and 2 μmol DOC were used to assess the accuracy of the estimates." This will be corrected in the revised version.

Reviewer 2: * P1536 L13 As discussed above, I think the error in this value has been determined incorrectly, and should be much higher.

Author comment: We agree. As described above we will estimate the standard error using error propagation techniques.

Reviewer 2: * P1539 L15-L20 What about Maher and Eyre 2010 you use the values from that paper in Table 2.

Author comment: We agree, we should include in this paragraph the values from Maher and Eyre 2010 shown in Table 2. The paragraph will be changed and would read: "Yet, information on DOC release rates by macroalgal communities is even more sparse than those for seagrasses, and the only published reports we are aware of corresponds to an Ulva lactuca community reported to release dissolved organic nitrogen (DON) to

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the water column under laboratory conditions (Tyler et al. 2001) and the DOC fluxes shown in Table 2 by Maher and Eure (2010).

Reviewer 2: * P1541 L1-8 Maher and Eyre 2011 discuss the source of DOC in macrophyte communities (i.e. autochthonous versus allochthonous) using changes in stable isotope ratios of the DOC pool over an incubation. This paper may be relevant to this section.

Author comment: We agree that this paper is relevant and will improve this section of the discussion on the contribution of macrophytes communities to coastal carbon budgets. The discussion section will read: "Because carbon burial and particulate carbon export by macrophyte stands typically exceed 40% of NPP (Duarte and Cebrián, 1996), the high net DOC release relative to NCP of the macrophyte communities studied here is unlikely to be supported by authochthonous photosynthetic production alone (Barrón et al. 2004). Recently, Maher and Eyre (2010) reported that the δ13C-DOC value from estuarine water column is similar to the seagrass and algal 13C values indicating that the DOC pool was dominated by macrophyte-derived DOC. However, inputs and subsequent decay of seston in seagrass sediments (e.g. Gacia et al., 2002; Hendriks et al., 2007, Kennedy et al. 2010), may play an important role in supporting the net DOC release of macrophyte communities. Use of stable isotopes to explore the link between macrophyte communities and DOC pools will help to further resolve the sources of DOC fluxes, authochthonous or allochthonous, in these communities."

Reviewer 2: * P1541 L9 and L10 DOM should be DOC P1541 Check the units used. For example when talking about yearly DOC fluxes use molC/m2/yr and why present global figures as 0.015 _ 0.003 Pg C/yr when you can present the sam values as 15 _ 3 Tg C/yr. Also as mentioned previously, the error is actually much larger than the value presented here.

Author comment: We agree. We will Tg C yr-1 to represent global annual fluxes.

Reviewer 2: * Table 1 Present temperature data for all of the authors own data.

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Author comment: We have obtained form the literature two references for water temperature that will be added to table 1.

Reviewer 2: * Table 1 The total error calculation is wrong. Table 1 Seasonal values for the Maher and Eyre study can be presented rather than annual values (i.e. an additional 21 values)

Author comment: As recommended by reviewer #1, we will use annual estimates per site when a seasonal study is available. We will use the annual values from Maher and Eyre 2010 shown in table 1 to estimate the average flux.

Reviewer 2: * Table 2 As with Table 1 the error estimate is not right

Author comment: As described above, we will use error propagation techniques to derive proper error estimates.

Reviewer 2: * Figure 2 How does this figure look with all the available temperature data?

Author comment: We have added additional water temperature data from the literature (see also reply to similar comment by reviewer 1), and we have removed this figure as suggested by reviewer 3.

Reviewer 2: Figure 3a The different regressions need to be easily identifiable (i.e. without having to look for the line slopes in the caption text).

Author comment: We agree. We will use different lines to differentiate those for seagrass and macroalgal communities.

Reviewer 2: * Figure 4 Do not show non-significant regressions. Figure 5 Do not show non-significant regression Figure 6 Why not present this in the same way as Figure 5, with 2 data sets (one for 2 days and one for 6 days of shading). Also in the caption why present the relationship if it is not significant?

Author comment: We will remove the non-significant regression from Fig 4 and 5. In

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Figure 5 (shading experiment in Florida) we just have net DOC fluxes after 5 days of shading, so we do not have 2 data sets.

References Kennedy H, Beggins J, Duarte CM, Fourqurean JW, Holmer M, Marbà N, and Middelburg JJ. 2010. Seagrass sediments as a global carbon sink: Isotopic constraints. Global Biogeochemical Cycles 24, doi:10.1029/2010GB003848.

Maher, D., and B. D. Eyre (2011), Insights into estuarine benthic dissolved organic carbon (DOC) dynamics using _13C-DOC values, phospholipid fatty acids and dissolved organic nutrient fluxes., Geochim. Cosmochim. Acta, 75, 1889-1902.

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